PEIRCE RUSTLED, RUSSELL PIERCED: HOW CHARLES PEIRCE AND BERTRAND RUSSELL VIEWED EACH OTHER'S WORK IN LOGIC, AND AN ASSESSMENT OF RUSSELL'S ACCURACY AND RÔLE IN THE HISTORIOGRAPHY OF LOGIC*

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Abstract. Russell gave scant attention and assigned little importance to the work of Peirce and Schröder in particular and to the so-called "algebraic" tradition in logic in general, compared with the generous notice and attention he apportioned to the work of Frege and Peano. Yet at the turn of the century the work of logicians in the Boole-Peirce-Schröder tradition was ajudged by most

* Work on this paper was begun around 1986, about ten years after Benjamin S. Hawkins, Jr. began his *[1992]* paper comparing Peirce and Russell's work on logic and examining their attitude toward each other's work. The approaches of my paper and Hawkins's differ, since mine is written from the historigraphic viewpoint and is concerned primarily with attitudes and with the historic impact which these views had on subsequent developments in logic, whereas Hawkins deals primarily with specific points of difference on logical matters from the technical viewpoint. Nevertheless, both papers utilize many of the same sources and come to much the same conclusion regarding the relative significance and logical correctness of the work of Peirce and Russell. Because I had greater and more frequent access to some of the archival materials than did Hawkins, I concluded my study only a few months after he completed his. I first learned of his research project in September 1989 (through an abstract he prepared for the logic sessions of the Peirce Sesquicentennial Conference), and I am grateful to him for sharing his work and views with me. As a matter of priority, however, Hawkins's work must take precedence.

logicians to be the acme of logical research. This naturally leads to the questions of what Russell knew about the work of Peirce and Schröder and when he learned about their work, and in turn to the question of why Russell gave such little attention and assigned such little importance to the work of Peirce and Schröder.

This paper reports on the available documents that were examined with an eye to answering these questions. The evidence adduced points towards the conclusion that Russell deliberately and consistently undervalued the work of the algebraic logicians in public while privately admitting their positive value.

Russell's assessment of the lack of importance of alegebaic logic tradition has had a lasting historiographic influence. Thus, even such historians of logic as Jean van Heijenoort regarded the "algebraic" tradition as a minor sidelight in the history of mathematical logic. The pervasiveness of this view leads to the question of how Peirce and Russell's contemporaries conceived of the relative merits of the Boole-Peirce-Schröder "school" and the Frege-Peano-Russell "school", and of the relationship between these two "schools". Of special interest, in couterpoint to Russell's public and private assessments of the contributions of Peirce to logic are the assessments of Peirce, his colleagues and students, of Russell's work in logic.

Our examination of the documentary evidence shows that Peirce and his associates held little esteeme for Russell's work, often regarding him as little more than a hack at best, as mathematically unsophisticated at worst.

In the past few years, historians of logic have begun to move closer to a balanced assessment of the relative merits and accomplishments of Peirce and Russell, giving greater credit to Peirce than had been afforded him by Russell and the "post-Russellian" historians of logic who shared Russell's evaluation of Peirce and of the algebraic logicians. The present paper is a contribution to this historiographic reappraisal.

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The historiographical background. Historians of mathematical logic frequently tell us that there are two traditions, the algebraic tradition of Boole, Schröder, and Peirce, arising from the algebraization of analysis, and the so-called "quantification-theoretical", or more accurately, in view of the work especially of Frege and Russell, function-theoretic (or logistic) tradition of Peano, Frege, and Russell, arising from the development of the theory of functions. It is said that these two traditions, together with the independent set-theoretical tradition of Cantor, Dedekind, and Zermelo arising out of the search for a foundation for real analysis in the work of Cauchy, Weierstrass and others, were united by Whitehead and Russell in their *Principia Mathematica* to create mathematical logic. The concern of most historians has been to contrast the algebraic and quantification-theoretic traditions and to show that the algebraic tradition had been the inferior of the two, that it reached a dead-end and was absorbed, along with set theory, into the quantification-theoretic tradition in the *Principia*. [Anellis & Houser *1991*] hold that the distinction between the algebraic and quantification-theoretic traditions is artificial, and that the algebraic logic of the nineteenth century *was* the mathematical logic of its day. [Couturat *1914*, 92], the French logician and historian of logic, for example, who was much closer to the historical situation, wrote that the algebra of logic "ought...to develop into a logic of relations, which LEIBNIZ foresaw, which PEIRCE and SCHRÖDER founded, and which PEANO and RUSSELL seem to have established on definite foundations." [Anellis & Houser *1991*] briefly explore the attitudes of some of those who contributed to the development of mathematical logic, especially those who belonged to the "algebraic" tradition, and suggest reasons, based upon the historiography of logic, for the bifurcation between algebraic logic and quantificational logic. [Anellis *1989*, 185–189] has even claimed that Russell deliberately distorted the history of logic for the purpose of self-glorification. Here, I shall examine in detail the evidence behind that claim, within the context of a survey of the views which Russell and Peirce held of each other's work.

The importance of special theories that are absorbed into more general theories is sometimes forgotten, neglected, or belittled, even by historians. This disregard of earlier trends seems to have been the case for algebraic logic, which, despite its contributions to the general development of modern mathematical logic, nowadays is most often carried on by algebraists rather than by logicians, as a part of universal algebra. Even proponents of algebraic logic sometimes reinforce this attitude and help perpetuate this trend. Whitehead [*1898*, vi] noted that "Symbolic Logic" has been "disowned" by some mathematicians for being too logical, and by some logicians for being too mathematical. Whitehead's goal in [*1898*, v] was to provide a "thorough investigation of the various systems of Symbolic Reasoning allied to ordinary Algebra", the "chief examples" of which were Hamilton's Quaternions, Grassmann's Calculus of Extension, and Boole's Symbolic Logic.

The Cambridge University logician W.E. Johnson ([*1905*]; quoted by [Lowe *1985*, 263]) thought that Whitehead's contributions to Boolean algebra and algebraic logic, although yielding "remarkable results and in a manner exhibiting extraordinary power" and "giving new life to the study of symbolic logic," did not receive the attention it deserved. According to Lowe [*1985*, 262–263], Whitehead's work may simply have come too late to have a real impact; that is, it came precisely as the algebraic tradition was giving way to the logistic and function-theoretic approaches of Frege, Peano, and Russell. Lowe believed that Johnson thought likewise

Contemporary historians of logic, until recently, have either ignored or downplayed the value of the algebraic logic tradition of the nineteenth century, in part because it had been "absorbed" into the more general "mathematical" logic in Whitehead and Russell's *Principia Mathematica* [*1910-1913*]. Jean van Heijenoort was one of the most influential of these historians giving attention to the algebraic tradition only to dismiss it. Twentieth-century pioneer researchers in universal algebra have occasionally acknowledged their nineteenth century antecedents, but seldom refer to specific results of Boole, Peirce, Schröder, or others, which they incorporated into their own research. By contrast, Whitehead [*1898*, x] not only named Hamilton and De Morgan as "the first to express quite clearly the general possibilities of algebraic symbolism," but continually expressed indebtedness to Boole, Grassmann, De Morgan, Schröder, and Venn, and, like Peirce and Schröder, cites specific examples of their influence and contributions.

Lowe [*1985*, 262–263], in referring to W.E. Johnson's estimation of the value of Whitehead's early contributions to logic, offers one possible explanation of why histories of logic ignore Whitehead's early work in algebraic logic and universal algabra, saying that "they came at a time when, because of the apparently final form that Schröder had given it, a decline might have set it," and he opined that Johnson would likewise have held this assessment. E.V. Huntington [*1933*, 278] saw Whitehead's contributions as the culmination of work on the Boolean algebra, writing that it was "originated by Boole, extended by Schröder, and perfected by Whitehead."

In much of the historical literature, especially prior to the mid-1970s, the algebraic logic tradition which effectively began with Augustus De Morgan and George Boole in the mid-nineteenth century has been perceived either as a secondary strain in the development of modern mathematical logic or as a virtual dead-end. Jean van Heijenoort expressed such a view (see, e.g. [van Heijenoort *1967*; *1967a*, vi; *1974*, 1; *1987*]). According to van Heijenoort, and as expressed by most historians, algebraic logic, having reached its most mature development in Schröder's *Algebra der Logik* and Whitehead's *Universal Algebra*, was effectively replaced by Whitehead and Russell's *Principia*, where it survived as the class calculus and the calculus of relations (*Principia*, vol. I, pt. 1, §§C, D [*1910*, 187–301]). An older example of this view was presented by [Behrens *1918*, 9–10], who in his study of the goals of Schröder's, Russell's (and König's) work, wrote that

> Schröder's algebra of logic still very much rested on its mathematical model. In this case, it can perhaps with great justification be stated that the *Vorlesungen über die Algebra der Logik* can be treated as a special mathematical field which leans heavily upon logic. The author can by no means show a maximum of practical applications. ...
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> Russell contrariwise. He proposed the goal of investigating the work of deducing the system of mathematics. Unlike Schröder, he therefore developed a presentation of the logical calculus.

In that same year, C.I. Lewis (see [*1960*, 118]), a proponent of the Boole-Schröder calculus, had to declare that it already had the status of a "classic" in the sense that it was fast becoming an antique. Citing the statement in *Principia Mathematica* [Whitehead & Russell *1910*, vol. 1, Summary of *4, p. 114] that "symbolic logic considered as a calculus has undoubtedly much interest on its own account; but in our opinion this aspect has hitherto been too much emphasized, at the expense of the aspect in which symbolic logic is merely the most elementary part of mathematics, and the logical prerequisite of all the rest," [Behrens *1918*, 10] suggests that Russell's goal can be seen in a "most clear light" in contrast to Schröder's goal. [Behrens *1918*, 7] nevertheless states that, subsquent to Peirce, it was Russell who carried out a general investigation of theory of relations. Peirce's posthumous "reply" to Behrens can be found in [Peirce *n.d. ca. 1897(b)*, MS 524:4–5], where he wrote that Schröder developed a calculus which "embraces all ordinary formal logic as nothing but an egregiously simple case. The logic of relations is, therefore, far from being a specialized branch of logic. On the contrary, it greatly enlarges and amplifies all logical conceptions...." The fact, however, is that Schröder's *Vorlesungen* systematized and extended Peirce's theory of relations and thereby provided the basis for Russell's theory of relations. Quine in his Ph.D. thesis *The Logic of Sequences: A Generalization of Principia Mathematica* [*1932*] took 290 pages to rework the first 400 pages of the *Principia* in order to permit proofs of theorems concerning relations in general because in the *Principia* it is "impossible...to adduce theorems in general about n-adic relations without having first specified the value of n" [Quine *1932*, ii]. Similarly, in a marginal note to p. 24, lines 25–28 of his copy of

Russell's *Principles*, Peirce wrote that "He considers only dyadic relations."

The more exaggerated claims made for Russell and his work name him as the greatest logician since Aristotle. One of those making these sorts of claims was Karl Popper, who asserted (in the text of an address of 19 January 1947; quoted in its original German in [Grattan-Guinness *1992*, 12] and in English translation by Grattan-Guinness in [Popper *1992*, 21]), for example, that Russell's

> *The Principles of Mathematics* was the most important contribution to logic that had been made at the time of its publication [1903] since the death of the founder of logic, Aristotle. The influence of this work on the later development of logic and the philosophy of mathematics was enormous.

Russell himself wrote to Lady Ottoline Morrell on 21 August 1912 of the influence of his *Principles* that "mathematical philosophers have different thoughts from what they w[oul]d have if I had not existed" (see [Clark *1975*, 189] and [Garciadiego *1991*, 132]). The statement is a legitimate estimate of Russell's influence and a correct appraisal of the historiographic situation in logic history for much of the post-*Principia* period.

The detailed technical surveys of the contributions of algebraic logic to post-*Principia* mathematical logic tend to ignore work done before the end of the nineteenth century; thus, Quine [*1941*] considers only the work in Whitehead's *Universal Algebra* [*1898*], but does not consider the work of those researchers which Whitehead's book summarizes. Historical surveys devote very little attention to the algebraic tradition. Thus, for example, Bocheński's [*1970*] history of logic devotes only some ten pages to "the Boolean calculus" and some twelve pages to the logic of relations, most of which focus on Russell's work rather than on that of De Morgan, Peirce, and Schröder, while the historical survey [Kneale and Kneale *1962*] devotes all of thirty pages to Boolean algebra and the logic of relations. Shields [*1981*, 142] and G.H. Moore [*1977*] are among the very few historians who have taken van Heijenoort to task for the way he belittled the contributions of the algebraic logic tradition. But even as early as the Spring of 1914, Josiah Royce had stated, as [Lenzen *1965*, 4] remembered, that Russell, then at the height of his fame as a logician, had "received more attention than any logician since Aristotle." More recently, it has been acknowledged by Thiel [*1987*] that the Boole-Schröder tradition has not been given its due, and

Thiel and his colleagues [Thiel, *et al. 1987*] planned to give full consideration to the algebraic logicians in their social history of logic. Even in the anthology *From Frege to Gödel* [van Heijenoort *1967a*], which was intended as a representative documentary history of the formative years of mathematical logic, the algebraic tradition is virtually ignored, represented only by the papers of Löwenheim ("Über Möglichkeiten im Relativkalkül" [*1915*]) and Wiener ("A Simplification of the Logic of Relations" [*1914*]), which characterize the "final stage" of the "absorption" of algebraic logic into the new mathematical logic.

It is crucial to keep in mind that, for logicians working in the period before the influence of *Principia* led to the relegation of the Boole-Schröder tradition to a logical "backwater", algebraic logic *was* mathematical logic, or was, at any rate, the late-nineteenth century's state-of-the-art version of mathematical logic. Thus, for example, in their encyclopedia article on "Symbolic Logic", Huntington and Ladd-Franklin [*1905*, 1], state that "Symbolic Logic, or Mathematical Logic, or the Calculus of Logic,— called also the Algebra of Logic (Peirce), Exact Logic (Schröder), and Algorithmic Logic or Logisitic (Couturat), — covers exactly the same field as Formal Logic in general..." and that these terms are quite synonymous. Thus, it is our view that the algebraic logic of the late nineteenth century should be reassessed. This view has been reenforced by the claims of the most recent scholarship. Thus, Peirce scholar Nathan Houser has gone so far as to claim [*1991*, 7] that the logic of the Peirce-Schröder school was well advanced of the contemporary work of the Frege-Russell-Peano school of the day.

Tarski [*1941*, 74] noted that, given the wealth of unsolved problems and suggestions for further research to be found in Schröder's *Algebra der Logik* [*1890-1895*], it is "amazing that Peirce and Schröder did not have many followers." Tarski's analysis of this situation and the reasons for it appear to rest on the assumption that the absorption of algebraic logic into Whitehead and Russell's logical system was at the cost of ignoring the mathematical content of the algebraic theory. Tarski [*1941*, 74] wrote that

> It is true that A.N. Whitehead and B. Russell, in *Principia mathematica*, included the theory of relations in the whole of logic, made this theory a central part of their logical system, and introduced many new and important concepts connected with the concept of relation. Most of these concepts do not belong, however, to the theory of relations proper but rather establish relations between this theory and other parts of logic: *Principia mathematica* contributed but slightly to the intrinsic development of the theory of relations as an

> *independent deductive discipline. In general, it must be said that* — though the significance of the theory of relations is universally recognized today — this theory, especially the calculus of relations, is now in practically the same stage of development as that in which it was forty-five years ago.

As a consequence, Tarski saw it as his task to take up where Peirce and Schröder left off and saw himself in an important sense as the direct mathematical descendent of Peirce.

What Tarski said of Schröder can be applied to many of the logicians of the "algebraic" tradition of Boole-Peirce-Schröder. It is particularly true that "Peirce's contributions to logic have been neglected or undervalued, both by his contemporaries and by the majority of historians of logic" ([Anellis & Houser *1988*]).

Bell [*1945*, *556–557*] (who words should always be taken *cum gran salis*) gave a different explanation for the failure of Peirce's logical work "to make the immediate mark its penetrating quality should have made," as a result of which "others retraced his steps, unaware that he had gone before." He cites (without reference; but see [Houser *1994*]) "Peirce's own explanation for his lack of adequate recognition, ...attributed to him on good authority," according to which "my damned brain has a kink in it that prevents me from thinking as other people think." Christine Ladd-Franklin gives a similar explanation in her passing remark [*1892*, 126] that Peirce "wrote his papers with the brevity and abstractness that befit a scientific journal." Peirce's friend Thomas Scott Fiske [*1988*, 15] recalled Peirce as being dramatic and as having a 'reckless disregard of accuracy in what he termed "unimportant details".' By way of example, Fiske [*1988*, 16] remembered an incident at a meeting of the New York Mathematical Society (before it became the American Mathematical Society) in the early 1890s when

> At one meeting of the Society, in an eloquent outburst on the nature of mathematics C.S. Peirce proclaimed that the intellectual powers essential to the mathematician were "concentration, imagination, and generalization." Then, after a dramatic pause, he cried: "Did I hear someone say demonstration? Why, my friends," he continued, "demonstration is merely the pavement on which the chariot of the mathematician rolls."

It would not be difficult to see, then, that logicians who have become accustomed to the style found in the *Principia Mathematica* might be uncomfortable with Peirce's style and even find it difficult as a consequence to digest his mathematics. But explanations for neglect of Peirce's work that depend upon such issues as the real or perceived difficulties of Peirce's "style" nevertheless are unconvincing, inasmuch as the same can be said about the work of others (the typography of Frege's *Begriffsschrift* notation, for example), and to the extent that algebraists and logicians from Schröder to Huntington and from Birkhoff to Tarski remained willing to study Peirce's work despite such difficulties.

Moreover, it is true that the Boole-Peirce-Schröder tradition was to some extent absorbed through the more recent quantification-theoretic and set-theoretic traditions into the new mathematical logic, and has, as Tarski noted in [*1941*], been reduced to a consideration of the connections of algebraic logic to first-order functional calculus. Thus, Peirce himself, in his third Lowell lecture of 1903 [*1903*, MS 459:20 = Peirce *1976*, III/1, 347], speaking of Russell's and Whitehead's work in logic, declared that "...quite recently Mr. Whitehead and the Hon. Bertrand Russell have treated of the subject; but they seem merely to have put truths already known into a uselessly technical and pedantic form." Elsewhere, [Peirce *1934*, 91] wrote that "My analyses of reasoning surpasses in thoroughness all that has ever been done in print, whether in words or in symbols — all that De Morgan, Dedekind, Schröder, Peano, Russell, and others have ever done — to such a degree as to remind one of the difference between a pencil sketch of a scene and a photograph of it."

It is this reduction of algebraic logic that has at least in part, if not exclusively, led historians of logic such as van Heijenoort, among others, to assign a minor role to the algebraic tradition within the broad structure of the entire development of mathematical logic. But this treatment and appraisal of the algebraic tradition is based upon an incomplete and erroneous understanding of the history of algebraic logic. Indeed, it is precisely the work of the algebraic logicians, in particular of Peirce and Schröder, and, later, as a more explicit connecting link, Löwenheim, in introducing quantifiers for the algebra of logic, that made possible the absorption of algebraic logic into the quantification-theoretical "mathematical" logic of Frege-Peano-Russell. It is true that Peirce worked almost exclusively in equational logic until 1868. But he abandoned equations after 1870 to develop quantificational logic. Thus, Peirce had begun to develop a quantificational theory in "The Logic of Relatives" of [*1883*], when he defined existential and universal quantifiers respectively in terms of logical sums and products. In Peirce's [*1885*] "On the Algebra of Logic: A Contribution to the Philosophy of Notation", we find quantifiers a first-order functional calculus, and a tentative second-order theory, and in his "Second Intentional Logic" [*1893*], we find a fully developed second-order theory. Thus [Quine *1985*] has stated that

> General quantification theory is the full technique of "all", "some", and pronomial variables, and it is what distinguishes logic's modern estate. Charles Sanders Peirce arrived at it independently four years after Frege. Peirce's work did indeed take off from that of Boole, De Morgan and Jevons. Ernst Schröder and Giuseppe Peano built in turn on Peirce's work, while Frege continued independently and unheeded.
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> The avenue from Boole through Peirce to the present is one of continuous development, and this, if anything, is the justification for dating modern logic from Boole; for there had been no comparable influence on Boole from his more primitive antecedents. But logic became a substantial branch of mathematics only with the emergence of general quantification theory at the hands of Frege and Peirce. I date modern logic from there.

Today, Quine [*1995*, 259; *1995a*, 24] is even more emphatic that "Peirce and not Frege was indeed the founding father" of quantification. And Putnam [*1982*, 297] likewise admitted that it was Peirce who effectively introduced quantifiers as we know them today. But this work was ignored by Russell (in *The Principles of Matehmatics* [*1903*, 23]), who concentrated his attention on Peirce's "On the Algebra of Logic" [*1880*] and [*1881*] papers on the calculus of relations. Łukasiewicz [*1921*; *1970*, 89] and Nidditch [*1962*] appear to be among the very few writers on history of mathematical logic of the earlier period to recognize Peirce's work with quantifiers, the former writing that "the term and the symbols for 'quantifiers' are due to Peirce," who was using primarily the symbolism of Boole and Schröder.

It must be against this backdrop that we must understand that, for logicians working during, and especially after, the period of the growing influence of *Principia*, the Boole-Schröder tradition was relegated to a logical "backwater". The "post-*Principia*" logicians "forgot", or did not know of, Peirce's work leading to the quantification of the algebra of relations. For Peirce's pre-*Principia* colleagues, algebraic logic *was* mathematical logic; that is, the algebraic logic of the late nineteenth century is the late nineteenth century's state-of-the-art version of mathematical logic. The dual "algebraic" and "quantification-theoretic" traditions, as a matter of historical fact, simply did not exist for logicians at the turn of the century. It is a false retrospective duality which derives from the *Principia* and is a post-*Principia* phenomenon. The attitudes which so many historians of logic display towards the algebraic tradition not only are rooted in the submergence of that tradition by Russell (and Whitehead) in the *Principia* into the quantification-theoretic tradition as defined by Frege-Peano-Russell, but also echo the prejudices asserted by Russell in his role as an expositor of the history of logic. Dipert [*1984*, 64] has aptly summed up this situation by saying that the "...contributions of Peirce and Schröder got lost" because "neither Peirce nor Schröder had the services of such an excellent propagandist as Russell. The Peirce-Schröder calculus was portrayed as purely algebraic, without the variable-binding operators Peirce regarded as essential and to which Schröder usually resorted...," while Hawkins [*1992*, 1] speaks of Russell as merely a "crystallizer."

A few contemporary algebraic logicians likewise recognize the value of the work of their predecessors in the Boole-Schröder tradition, although most have little precise or direct knowledge of the details of their predecessors' work. In his survey of the contributions of Tarski to algebraic logic, Monk [*1986*], echoing Tarski [*1941*], recognizes the contributions of Peirce and Schröder in developing the theory of binary relations, but this is only a small part of algebraic logic. Tarski himself recognized in a few instances the role of Peirce. In [*1941*, 73], Tarski called Peirce "the creator of the theory of relations", which Schröder continued and systematically developed, while Halmos [*1962*, 10] calls Boole the "father of algebraic logic".

Peirce on Russell and Russell on Peirce. Russell studied Schröder's *Vorlesungen über die Algebra der Logik* [*1890-1895*] some time during September 1900 (the month during which he acquired that work); and soon thereafter he also studied Schröder's "Der Operationskreis des Logikkalkuls" [*1877*] and "Sur une extension de l'idée d'ordre" [*1901*]. His notes [Russell *1901a*] on the latter two works are far more extensive than his marginal notations in his copy of [Schröder *1890-1895*]. In Russell's copy of Schröder's *Algebra der Logik*, very few passages referring to Peirce are marked; on the whole, Russell's marginal comments are much more favorable to Peano than they are to Schröder, not surprisingly, since Russell considered his greatest intellectual debt to be to Peano (see [Kennedy *1973*] and [Russell *1967*, 217–219]). A detailed examination of the material in the Russell Archives related to Schröder shows that Russell was extremely critical not only of Schröder, but of the entire algebraic tradition from Boole onward, despite the fact that Peano, to whom Russell considered himself the most indebted, belonged to that tradition] initially, and despite the fact that contemporaries of Whitehead and Russell saw the *Principia* essentially as the apogee of that tradition (see [Anellis *1990/1991*, 244]). In [*1903*, 10], Russell does condescendingly admit that Schröder's *Algebra* is "the most complete account of non-Peanesque methods." At the same time (2 June 1903), Russell wrote to Louis Couturat ([Russell *1903b*]) that he read Schröder's work only after learning of Peano, and that "it is not therefore essential to go through him." A few days later (9 June 1903), Russell [*1903c*] told Couturat that Schröder speaks "prose" without knowing it. (In a comment reminiscent of Russell's remark to Couturat that Schröder spoke prose without knowing it, [Littlewood *1986*, 130] reported of Russell that "he said once, after some contact with the Chinese language, that he was horrified to find that the language of the *Principia Mathematica* was an Indo-European one.") To Peirce [*n.d. ca. 1897(a)*, MS 521:2], by contrast, unsurprisingly, "Prof. Schröder's work [*Algebra*] is, and must for many years remain, the standard treatise upon exact logic...."

There are remarkably few references to the work of the principal investigators in algebraic logic in Russell's early work, for example in his [*1900*] survey of work in logic and the foundations of mathematics and in his important paper "Sur la logique des relations" [*1901*], given his professed strong interest in the logic of relations. It is less surprising that there are so few references to Peirce or to Schröder in *The Principles of Mathematics* [Russell *1903*] or other mathematical writings of the same period, given his generally unfavorable comments in his manuscript notes on Schröder's *Algebra* ([Russell *1901a*]) and elsewhere. About Peirce, Russell [*1946*, xv] wrote that, although he had first heard of him in 1896, he "read nothing of him until 1900, when I became interested in extending symbolic logic to relations, and learnt from Schröder's *Algebra der Logik* that Peirce had treated of the subject."[1] Russell's remark is surprising when we realize that Peirce's logical writings were well known in England and on the continent, and that, long before 1900, both Peirce and Schröder had made significant progress in "extending symbolic logic to relations". By then, Peirce [*1883*; *1885*] had moved

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[1]This is the source of Fisch's recollection, in a letter of 23 April 1977 to Russell Archivist Kenneth Blackwell, that Feibleman told him [Fisch] that Russell had once met Peirce. In his letter, Fisch recollected that he understood from both Feibelman and Elizabeth Eames that when Russell was in the United States in 1896 and visited William James, he had also met Peirce. (On the separate question of whether Peirce and Frege knew of each other's work, see Hawkins [*1993*].)

on to work in quantification theory. This renders Russell's neglect all the more curious. (Lowe [*1985*, 231] asserts that beginning in 1898 Russell "became acquainted with...symbolic logic, through the works of Boole and later logicians, and Book II of Whitehead's *Universal Algebra*;" but does not identify the "later logicians.) It is also curious in view of the fact that, as the archival evidence shows, Russell knew of Peirce's [*1883a*] and mentioned it ([Russell *1899*], quoted in [Blackwell *1987*]) in his letter to Couturat of 11 February 1899, and in fact suggested that Couturat might be interested in reading it. We also know that Russell had read Peirce's [*1880*] and [*1885*] *American Journal of Mathematics* papers, since Russell had taken notes on both of these papers, some time around 1900-1901 [Russell *ca. 1900-01*] (i.e. at about the same time as reading Schröder), in preparation for his own work on the logic of relations. Hawkins [*1992*, 43–44], however, suggests that Russell actually read very little of Peirce's work, totalling the equivalent of approximately 14 pages in the third volume of Peirce's collected papers [*1933*], merely skimming [*1880*] and [*1885*]; this, Hawkins adduces, accounts for Russell's misunderstanding of Peirce's work on the logic of relations in general and on relative addition in particular.

While it is problematic how much knowledge Russell may have had of Peirce, he had in any event at least enough apparent enthusiasm for Peirce to lead Harvard University philosophy department chairman James Houghton Woods on 23 September 1916 to offer him a position editing Peirce's papers, for which Henry M. Sheffer was to be appointed Russell's assistant, and to teach a seminar on Peirce. In this case Russell was unable to accept the position because he was unable to obtain a visa because of his anti-war activities. The belief that Russell was interested in Peirce's work persisted, and led James K. Feibleman to suggest to Max Fisch ([Feibelman *1959*]) that Russell might wish to assist Fisch with his studies for the Peirce Edition Project and with his work towards a biography of Peirce (information from N. Houser, private communication) and to write [Russell *1946*]. In reply, however, Russell [*1959a*] wrote to Fisch in a letter of 4 July 1959 that he is unable to work on Peirce, since "there was very little relation between his work in logic and mine." Russell went on to explain that Peirce's "treatment of the logic of relations did not seem to me what was appropriate for mathematics and, apart from that, I read very little of his work until my own was finished."

(Against this background, it is ironic to recall that Russell found apparent reason to complain about the lack of interest in his work on relation-arithmetic, which he regarded as his "most important contribution" to the *Principia* ([Russell *1959*, 95), "not only as an interesting generalisation, but because it supplies a symbolic technique for dealing with structures," [Russell *1959*, 100]) that is, with the isomorphism type of a system of relations. His complaint, however, as [Solomon *1989-90*] notes, ignores Tarski's work on ordinal algebras ([*1956*]), which deals algebraically with Russell's "relation-numbers", as relation types. [Solomon *1989-90*, 170], citing Tarski's work as evidence of interest in relation-arithmetic, suggests that Russell's complaint may more accurately reflect Russell's own lack of interest in logic after completion of the *Principia*. We should also note that [Tarski *1956*, 1] explicitly expresses his debt to the *Principia*, where "the [arithmetic] operations [on order types and ordinals] were extended to relation types." There is moreover no question that Russell could not have known about this work (at least from 1963). We know that, in reply to Russell's [*1963*, 1–2] complaint to Leon Henkin that the *Principia* did not elicit the kind of response for which its authors had hoped, he ([Russell *1963*, 2]) told Henkin that he and Whitehead "had fondly imagined that we were making the kind of advance that Descartes made in geometry by the use of co-ordinates." In particular he and Whitehead had hoped, as Henkin [*1963*, 2] had expressed it, "to be leading mathematicians to a general and detailed study of relations of higher rank." Henkin [*1963*, 2] informed Russell that "Tarski and some of his students have undertaken an axiomatic-algebraic investigation of the theory of dyadic relations..." and briefly described their work.)

On the question of the logic of relations, Russell [*1901*] raises specific criticisms of Peirce's work. In [*1870*], Peirce introduced the same symbol ($-\!\!<$) for class inclusion and for implication. This was seen by Russell [*1901*] as a serious weakness; and he takes pains there to distinguish his own work from that of Peirce and Schröder. (His complaint is presumably based upon his distinction [*1903*, 187] between "Universal Mathematics", meaning universal algebra in what he understood to be Whitehead's sense, and the "Logical Calculus", the former "more formal" than the latter. In particular, for Universal mathematics, the signs of operations are variables, whereas for the Logical Calculus, as for every other branch of mathematics, the signs of operations have a constant meaning.) But Russell misses the point, inasmuch as he interprets Peirce's notation to be a conflation not of class inclusion with implication, but of class inclusion with set membership. (This erroneous interpretation is reiterated by [Kennedy *1973*, 367–368].) In a letter to Russell of 27 January 1901, Couturat [*1901*] expresses his agreement with the need to distinguish implication from set membership. In fact, however, Peirce made no distinction in his work prior to *[1885]* between sets and classes, and so the charge that he conflates the notation for class inclusion with the notation for set membership is moot for Peirce, if there is an issue here at all. Schröder too, following Peirce, used the same symbol, '$\mathrel{=\!\!\!\!\in}$', for class inclusion and implication, for which he, in turn, was criticized by Frege [*1895*]. The modern distinction between sets and classes about which Russell worried appears to have its historical roots in Cantor's 1897 distinction between complete and incomplete multiplicities, and arose as a result of the Cantor and Burali-Forti paradoxes. The distinction was clarified and formalized only by Russell and his successors (in particular von Neumann) in response to the Russell paradox. For Boole [*1854*], propositional calculus and the class calculus are two interpretations for the Boolean algebra; for us, set theory is another. In fact, Peirce intended quite deliberately that "—< " should be a basic and primitive relation subject to various interpretations, including, among others, class inclusion, the ordering relation, and material implication. Peirce was able, using his —< together with other logical apparatus such as his notion of logical dimension, to do without a special symbol for set membership (see [Dipert *1978*, 250]). In his *Notes on Cantor's "Beiträge..."* [MS 821, n.d.], Peirce wrote that Cantor "implies that the relation of the collection to a member is that of inclusion," and then asserts that Cantor should first have defined an order relation. After reading Cantor's "Beiträge zur Begrundung der transfiniten Mengenlehre" [*1895-1897*], Peirce obtained his own versions of the Cantor and Russell paradoxes, and in letters to Cantor dated 21 December 1900 and 23 December 1900 ([Peirce *1976*, III/2, 767–771; 772–779]), appears to recognize the need to distinguish between types of collections, such as sets and classes. Although of course Russell could not have had access to Peirce's unpublished work on set theory or to his correspondence with Cantor, his criticisms of Peirce and Schröder on this point of notation are nevertheless anachronistic, and thus unjustified, although Frege was on more solid ground in his criticism of Schröder. But the damage had been done. Meanwhile, Peirce, discussing Cantorian set theory specifically and set theory in general in the third of his lectures at the University of Lowell (MS 459:19–20 = *1976*, III/1, 347), noted that Schröder's presentation showed that Dedekind's ideas were similar to Peirce's, and that "...quite recently Mr. Whitehead and the Hon. Bertrand Russell have treated of the subject; but they seem merely to have pre[sented] truths already known into a uselessly technical and pedantic form."[2] More specific responses to Russell's criticisms of Peirce's alleged errors, and particularly to his "failure" to distinguish inclusion from set membership are to be found in Peirce's annotations to his copy of Russell's *Principles*. Victor Lenzen ([Lenzen *1965*, 7–8]) found that "there are a few critical remarks by Peirce in the margins" of Peirce's copy of Russell's *Principles*. Here, Peirce suggests that the difficulties which Russell had with the lack of distinctions in Peirce's (and Schröder's) work were grounded in Russell's own failure to distinguish material implication and truth-functional implication (conditionality), and in his erroneous attempt to treat classes, in function-theoretic terms, as individual entities.

Russell's [*1901*] criticisms in "Sur la logique des relations avec des applications à la théorie des séries" of Peirce's failure to distinguish class inclusion from set membership are revived, in a somewhat different guise, in [*1903*]. After noting [Russell *1901*, 24] that Peirce and Schröder "realized the great importance of the subject" of the logic of relations, while at the same time asserting that "unfortunately their methods, being based, not on Peano, but on the older Symbolic Logic derived (with modifications) from Boole, are so cumbrous and difficult that most of the applications which ought to be made are practically not feasible," Russell [*1903*, 24] levels the criticism that

> In addition to the defects of the old Symbolic Logic, their method suffers technically...from the fact that they regard a relation essentially as a class of couples, thus requiring elaborate formulæ of summation for dealing with single relations.

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[2] Peirce may have been led to "implicate" Whitehead in the writing of the *Principles* by the strength of Russell's acknowledgement of Whitehead in the "Preface" [*1903*, xviii], according to which "At every stage of my work, I have been assisted more than I can express by the suggestions, the criticisms, and the generous encouragement of Mr. A.N. Whitehead," by a knowledge that Whitehead and Russell's collaboration went back at least as far as 1902, and by a presumption (evidenced by his various unflattering remarks on Russell's mathematical sophistication) that Russell would not have been mathematically capable of engaging on his own in such an enterprise. Another factor is likely to have been Whitehead's [*1898*, 3, 10, 37, 42, 115–116; *1901*, 139] remarks that Peirce's *relations* are "obscure" and his expression [*1902*, 367–368, 378–382] of preference to Russell's notation for relations to Peirce's.

Moreover, if we retrospectively examine Russell's own statements of the depth of Whitehead's contributions to his own work, we find that these frequently conflicting statements lead to scepticism about Russell's veracity.

(see also [Russell *1959*, 87], referring specifically to Peirce's treatment of a relation as a class of couples).

In reply to Russell's [*1903*, 26] assertion that

> Peirce and Schröder consider also what they call the relative sum of two relations R and S, which holds between x and z, when, if y be any other term whatever, either x has to y the relation R, or y has to z the relation S. This is a complicated notion, which I have found no occasion to employ, and which is introduced only in order to preserve the duality of addition and multiplication.

Peirce (at p. 30, letter to Lady Welby of 12 October 1904, in [Hardwick *1977*, 22–35) stated that

> As to my algebra of dyadic relations, Russell in his book which is superficial to nauseating to me, has some silly remarks about my "relative addition" etc., which are mere nonsense. He says, or Whitehead says, that the need for it *never* occurs if you bring in the same mode of connection in any other way. It is part of a system which does not bring in the mode of connection in any other way. In that system it is indispensable. But let us leave Russell and Whitehead to work out their own salvation.

This remark of Peirce's must have been included in the letter of Peirce (mentioned in [Russell *1904a*, 1]) which Lady Welby passed on to Russell, since Russell [*1904b*] claims that he "does not know where Whitehead or I have said that the need of Dr. Peirce's Algebra of dyadic relations seldom occurs." He adds that he thinks that "a symbolism based on Peano's is practically more convenient, but I hold it quite essential to have a method of expressing relations, & I have always thought very highly of Dr. Peirce for having introduced such a method." Peirce was certainly able, then, to reply in kind to Russell's attacks. In a letter to Christine Ladd-Franklin, Peirce ([Peirce & Ladd-Franklin *1891-1908*, MS L237: 27 July 1904]) stated that "...a year has passed since I agreed to notice Russell's vol. I [for the journal *Science*], and I feel its pretentiousness so strongly that I cannot well fail to express it in a notice." In a letter to F.W. Frankland of May 8, 1906, he adds that "In my opinion Russell and Whitehead are blunderers constantly confusing different questions" ([Peirce *1977*, III/2, 785–787: MS L148]), quotation on p. 785). One example which [Peicre *1903a*, MS 469:20 = *1977*, III/1, 371] observes that "puzzles the Hon. Bertrand Russell in his 'Principles of Mathematics' is whether a collection which has but a single individual member is identical with that individual," and he attributes Russell's puzzlement to his failure to make the appropriate terminological and conceptual distinctions between these. (Interestingly, [Hardy *1967*, 138] wrote that "it is not lack of understanding...which is the trouble in Whitehead's case; but he forgets, in his enthusiasm, distinctions with which he is quite familiar.") Elsewhere, Peirce goes so far as to suggest that Russell appropriated the ideas of others while taking credit for those ideas. Thus, for example, in the letter of 1 December 1903 letter to Lady Victoria Welby, Peirce [Peirce *1977*, 9] wrote that he concluded from a cursory reading of [Russell *1903*] that "whatever merit it may have as a digest of what others have done, it is pretentious & pedantic — attributing to its author merit that cannot be accorded him."

Peirce's suggestion that Russell expropriated the work of others without due credit seems to be confirmed by Russell in a letter to Jourdain of April 1910 (quoted in [Grattan-Guinness *1977*, 133]) in which Russell tells Jourdain that "[D]uring September 1900 I invented my Logic of Relations" and a [*1901c*] letter to Couturat, in which Russell asks Couturat if he has "observed that Schröder's extension of the concept of order (Rangstufenfolge) is nothing other than my series in relative position?" While this seems to suggest that he obtained his results in the logic of relations and on series independently of any study of Schröder, we know that Russell wrote a number of papers on relations and order, including his [*1901b*] paper "On the Notion of Order", about the time that he began his study of Schröder's work (recall that Russell studied Schröder's *Vorlesungen* [*1890-1895*] some time during September 1900 (the month during which he acquired that work); and soon thereafter [Russell *1901a*] he also studied Schröder's "Der Operationskreis des Logikkalkuls" [*1877*] and "Sur une extension de l'idée d'ordre" [*1901*]). It seems most likely, though that here he is referring to his paper "On the Notion of Order", which must have been written about the time that he began his study of Schröder's work. We cannot therefore assume that the equivalence of Schröder's extension of the concept of order and Russell's series in relative position is the "coincidence" that [Russell *1901c*] claims it to be. Our suspicion is supported by Russell's own [*1959*, 65–66, 72–73] admission that

> Boole had published his *Laws of Thought* in 1854, and had developed a whole calculus dealing mainly with class-inclusion. Pierce had developed a logic of relations, and Schröder had published a work in

> three big volumes summarizing all that had previously been done. Whitehead devoted the first portion of his *Universal Algebra* to Boole's calculus. Most of the above works were already familiar to me.... I still have the MS. of what I wrote on this subject just before my visit to [the International Congress of Philosophy] in Paris [in the year 1900].
>
> ...It was at the end of July that I met him [Peano], and it was in September that I wrote a paper on the logic of relations which was published in his journal. I spent October, November and December of that same year [1900] on *The Principles of Mathematics*. ...I finished this first draft of *The Principles of Mathematics* on the last day of the nineteenth century — i.e. December 31, 1900.

So it very unlikely that the equivalence could have been a mere "coincidence".

Another example of Russell's use of the work of others without giving due credit occurs in the *Principles*. The so-called "principle of reduction" given as Axiom 10 ([*1903*, §18, p. 17]), according to which $((p \supset q) \supset p) \supset q$ (when $p \supset p$ and $q \supset q$) is exactly the "fifth icon", which may be found in [Peirce *1885*, 189], what we now know more familiarly as "Peirce's Law". While nowhere in the presentation of the axioms for the propositional calculus does Russell explicitly say that the axioms presented are his, neither does he say anything to suggest that the particular set of axioms which he presents, and the form in which they are presented, are not his own. Moreover, while Russell treats it (as we noted) as an axiom, Peirce says that it is "hardly axiomatical", and provides an argument to show that it is true. And while Russell states ([*1903*, 17]) that it is "less self-evident" than the other axioms, he argues that it is equivalent to other propositions which are self-evident, and then gives as justification of its truth an argument which is exactly and in all details identical with the one which Peirce ([*1885*, 189–190]) gave. Moreover, both Peirce and Russell express the direct relevance of this principle in order to support a proof of the Law of Excluded Middle. One is led to ask why Russell would wish to prove an axiom that, while not obviously self-evident, is known to be equivalent to propositions which are self-evident, and why his proof of such an axiom should so closely duplicate Peirce's proof of the same, but non-axiomatical, proposition.

Russell himself admitted to Couturat in a letter of 25 June 1902 that he "found many things in it [Frege's *Grundgesetze*] which I believed I had invented" (quoted by [Griffin *1992*, 245]).

In addition to the question of Russell's appropriation without due acknowledgement of the work of others, here is the related question of the degree of Russell's competence to carry out original work without external assistance. We shall deal with this question later.

The scant treatment that has been afforded to the algebraic tradition is arguably due more to Russell than to anyone else, despite his admission that he had read the work of (some of) the algebraists, and despite his having begun his survey of "Recent Work on the Principles of Mathematics" [*1900*, 83] with the declaration that "pure mathematics was discovered by Boole, in a work which he called the 'Laws of Thought' (*1854*). ...His book was in fact concerned with formal logic, and this is the same thing as mathematics." Despite this promising beginning, [Russell *1900*] is concerned almost entirely with the development of set theory from attempts to deal with foundational problems arising from the infinitesimal analysis, and even the development of axiomatic deductive systems of logic, such as that of Peano, receive only secondary attention. Nowhere in his more serious technical writings did Russell credit Boole with the discovery of pure mathematics, or even with the discovery of mathematical logic, so that his remark to that effect in [*1900*] must be attributed, as G.H. Moore [*1989*] has noted, to the popular nature of the article in which it appeared and the intention to shock in the hope of convincing the reader to accept the logicist thesis that mathematics is logic by asserting the preposterous claim that Boole discovered pure mathematics. (G.H. Moore [*1989*, 1] cites, for example, Russell's letter of 31 December 1900 to Helen Thomas (see [Griffin *1992*, 207; letter 91]), stating that [*1900*] was written as a popular piece, for "filthy lucre," and an 18 July 1901 letter to Couturat, quoted by [Moore *1989*, 2], stating that [*1900*] was "a *popular* article" in which "there are one or two errors," going on to admit that he therefore wrote in it many things that are "not entirely correct.") This did not prevent him in the letter of 31 December 1900 to Helen Thomas (see [Griffin *1992*, 207; letter 91]), from boasting about the manuscript on which he had already begun working and which would become *The Principles of Mathematics* that"In October I invented a new subject, which turned out to be all mathematics for first time treated in its essence. Since then I have written 200,000 words, and I think they are all better than any I have written before."

The development of the "Algebra of Relatives", which Russell [*1900*, 85n] attributed to "Professor Peirce of Harvard," is portrayed as merely a means of deducing the properties of series "from the principles of symbolic logic" [*1900*, 98]. Much later, in a letter of 19 May 1954 directed to Mr. Hackett of the Royal Irish Academy on the centenary of the publication of Boole's *Laws of Thought*, Russell [*1954*] wrote retrospectively that "The remark that you quote from me to the effect that pure mathematics was discovered by Boole was of course not to be taken literally, but only as an emphatic statement of the importance of the subject which he inaugurated. This subject has now grown to vast proportions and has developed in directions that would have surprised Boole, but the developments have made his importance more evident." How is this remark to be reconciled with Russell's contrasting claims that the whole Boole-Schröder tradition was an unproductive dead-end? From the other logical writings which Russell undertook at the start of the twentieth century, we must conclude that Russell's view of the work of the Booleans at that time remained essentially dismissive. Admittedly, Peirce [*n.d., ca. 1897*, MS 520:3] readily conceded that "Boole's original algebra, as I first showed in 1867, is inadequate to express *particular* propositions." But then he recalls [Peirce *n.d., ca. 1897*, MS 520:3–4] that this is what led him, beginning around 1880, to develop indexed terms for individuals of the Boolean (i.e. propositional) parts of quantified formulae. Thus he certainly did not reject Boole's contributions out of hand, but deliberately built upon Boole's work.

Russell was more strongly critical of the algebraic tradition in *The Principles of Mathematics* [*1903*, 10], where he states that "since the publication of Boole's *Laws of Thought* (*1854*)," symbolic logic "has been pursued with a certain vigour, and has attained," mainly due to Schröder, "a very considerable technical development but nevertheless the subject achieved almost nothing of utility, ...until it was transformed by the new methods of Peano".

Concerning the specifics of the calculus of relations, Russell [*1903*, 24] asserted that, though Peirce and Schröder "realized the great importance of the subject, unfortunately their methods, being based, not on Peano, but on the older Symbolic Logic derived (without modification) from Boole, are so cumbrous and difficult" as to make them virtually useless. It is difficult to understand how Russell could possibly have suggested that Boole's successors, especially Peirce and Schröder, had left Boole's work entirely unaltered, had added nothing substantial of their own. We know, as mentioned above, that Russell, in his letter to Couturat ([Russell *1899*], quoted in [Blackwell *1987*]) discussed Peirce and suggested that *Studies in Logic* [Peirce *1883a*] might be of some interest to Couturat. We also know, as already mentioned, that Russell read several of Schröder's works before [Russell *1903*] went to press, probably while [*1903*] was being written. At another point in his [*1903*, 376], Russell is a bit more generous concerning the contributions of Boole, H. Grassmann, William Hamilton, De Morgan, Jevons, and Peirce to the development of universal algebra, in particular in regard to their work on the theory of imaginary numbers as that led to the extension of ordinary algebraic operators to fields of various kinds. But even here, Russell [*1903*, 377] gives the most credit to Whitehead. As a result, there are only a few passing mentions of the results of Peirce and Schröder as Russell looked at the details of the algebra of relations. Indeed, some of Peirce's students, such as Christine Ladd-Franklin, have taken *Principia* severely to task for the slight recognition of the work of Peirce and Schröder. In notes probably made for a Columbia University class lecture, Ladd-Franklin [n.d.] wrote: "It should now be clear how the logic of *Principia* is related to the logic we have presented, following the materials of Peirce and Schröder.... But Whitehead and Russell plainly 'imply' that P[eirce] and S[chröder] were absolutely non-existent!" Josiah Royce and Fergus Kernan [*1916*, 706–707] have likewise asserted, concerning "...Peirce's researches on the algebra of logic, and in particular in the logic of relatives," that "many of the most recent researches, including those of Bertrand Russell, are still due to his influence, although Russell, ...has a somewhat inadequate sense of his own generally indirect indebtedness to Peirce's work in this field". In his [*1898*, x], which Russell (according to his catalogue [*1891-1902*]) read in March 1898, Whitehead explicitly expressed his indebtedness "in regard to Symbolic Logic to Boole, Schröder and Venn." Thus, Russell might have based his "justification" for virtually ignoring Peirce's work on Whitehead's exclusion of Peirce from those to whom he was indebted; but he cannot have used this as an explanation for failing to take Schröder's work seriously. Ladd-Franklin [*1904*] sought to prod Peirce into replying to Russell; in her letter to Peirce of 24 July 1904, she therefore asked:

> Do tell me how it strikes you — all this recent work of Bertrand Russell, Peano, Couturat & their school, which they make so much of. Don't you think that they exaggerate both its originality & its importance? Are you not going to write some-thing on the subject?

When confronted directly by Norbert Wiener's [*1913*] comparison of Schröder's *Algebra* with the *Principia*, Russell dealt sharply with Wiener's positive assessment of Schröder (see [Grattan-Guinness *1975*]), claiming that Wiener had only treated "the more conventional parts of *Principia Mathematica*. I should rest its claims mainly upon three definitions, *14.01, *20.01, and *30.01. The rest is mainly working out these three. Can Schröder's methods express these?" ([Grattan-Guinness *1975*, 130]; quoting Russell's [*1913*] comments on [Wiener *1913*]). This is in response to Wiener's ([*1913*]; quoted in [Grattan-Guinness *1975*, 108]) statements that "Peirce first developed an algebra of relatives, which Schröder extended...." For Russell's part, Wiener was "disgusting, I don't know why; I hardly know how to be civil to him" (see Letter 216 to Ottoline Morrell, 9 October 1913; Griffin *1992*, 480]).

By contrast with Peirce and Schröder, Whitehead and Russell introduced a logic of relations following Peanesque principles. Russell claimed that the Peirce-Schröder approach was inferior with regard to...technical fluency.... Within the limits to which comparison is possible, the two systems are equivalent...", although "Schröder deals mainly with classes" and seeks "to develop...a branch of algebraic logic," while Russell seeks "to found the whole of mathematics from a few simple logical assumptions." These remarks were made despite the fact that, in a letter to Russell of 27 January 1901, Couturat [*1901*] warns Russell that "one must not deprecate the Boole-Schröder system and sacrifice it to Peano: it has its goal and its use in pure logic just as Peano's has its in Math. And the theory of logical equations has great importance since all logical problems amount to a system of equations and inequations."

Russell argued that his methods and symbolism were superior to Peirce's, not only in regard to its ability to express what Peirce's and Schröder's cannot, but in its ability to answer "many fundamental questions...(fundamental, I mean, to the foundations of mathematics & the principles of symbolism)... ," and he expressed the "hope that [volume 2 of the *Principles*], will do much to persuade such opponents as Mr. C. S. Peirce" of the correctness of his own views, as he said in his letter to Welby of 11 November 1904 ([Russell *1904*, 2–3]). This was certainly a crucial question for logic at the outset of the twentieth century. Thus, Eliakim Hastings Moore, in his capacity as editor of the American Mathematical Society's *Transactions*, wrote to Peirce on 14 October 1902, asking him to respond to Whitehead's [*1902*] paper "On Cardinal Numbers", and in particular to compare his work with the work of Whitehead and Russell; in his letter, E.H. Moore [*1902*] noted that Whitehead [*1902*] was written in Peano's notation and uses Russell's "additions on the algebra of relations in general. The Italian school believe in the new symbolism as a calculus in terms of which it is highly convenient to work, and not merely as an algebra of mathematical logic. It would give me much pleasure," Moore concluded, "if you would let me know what you think concerning Russell's work, especially in comparison with your own... ." Just over a year later, in a letter of 31 December 1903, Moore [*1903*, L299:45] asked Peirce what he thought of Russell's *Principles of Mathematics.*

Naturally, Russell could not be faulted for being ignorant of work, such as [Tarski and Givant *1987*], which appeared well after his death, which indeed produced a system bringing together Schröder's goal of developing the class calculus as a branch of algebraic logic with Russell's goal of founding all of mathematics on a small number of logical assumptions. Tarski and Givant achieved this unification by creating a branch of algebraic logic that founds set theory and number theory on a small number of logical assumptions, on the basis of the calculus of relations. But it should be noted that these two goals were also brought together by Peirce, who had already made a start at its realization in "Description of a Notation for the Logic of Relatives, Resulting from an Amplification of the Conceptions of Boole's Calculus of Logic" [Peirce *1870*], in his [*1881*] notes and addenda to [B. Peirce *1881*], and in other early papers ("On the Logic of Number" [*1881a*]; "The Logic of Relatives" [*1883*]; "On the Algebra of Logic: A Contribution to the Philosophy of Notation" [*1885*]). Russell's question, in his response to Wiener [Russell *1913*], as to whether Schröder's methods can express what Russell's methods express, is not really a question, but rather an exclamation, based upon an already established prejudice, as is evident from a letter to P.E.B. Jourdain of 15 April 1910, Russell ([*1910*]; quoted in [Grattan-Guinness *1977*, 134]), in which he called Schröder's methods "hopeless". But just as Russell can not be faulted for being ignorant of [Tarski and Givant *1987*], so he ought not to have in effect faulted Schröder for failing to create the mathematics that finally appeared in [Tarski and Givant *1987*]! Turning to the other side, we find that Schröder, in a letter to Klein, recently discovered in the Klein-*Nachlaß* by Volker Peckhaus [*1990-1991*], initially offered his [*1898*] paper "On Pasigraphy: Its Present state and the Pasigraphic Movement in Italy" to Klein for *Mathematische Annalen* and announced a shift in his attitude towards formal logic as a result of having read Peirce's "The Logic of Relatives" [*1883*]; Schröder was, says Peckhaus ([*1989*, 1]; [Peckhaus *1990-1991*]; see Schröder's letters to Klein, [Peckhaus *1990-1991*, 198–202]), led to abandon Peano's logical symbolism on the grounds that it did not appear to be capable of expressing relatives.

Shearman, in discussing Russell's contributions to logic, gave two specific examples of results which the classical Boole-Schröder calculus did not appear to him capable of expressing were addition and multipliaction of integers. This, says [Shearman *1906*, 199*ff.*] is because it dealt with relations rather than classes, because "the older symbolist considered there is no need to make a distinction between th[e relation of an individual to the class of which it is a member] and that of a class to a wider class" ([Shearman *1906*, 201]). But recall that it was Peirce [*1903a*, MS 469:20 = *1977*, III/1, 371] who observed that what "puzzles the Hon. Bertrand Russell in his 'Principles of Mathematics' is whether a collection which has but a single individual member is identical with that individual." [Shearman *1906*, 207]), on the other hand, referring to [Russell *1903*, 113, 115], states that "Mr. Russell demonstrates that it is possible to define numbers in such a way that they are seen to be susceptible of being manipulated by the rules of Logic. A number," Shearman says,

> may be defined as a class of similar classes, *i.e.* of classes whose members are correlated one to one; and since classes may be logically treated, numbers are brought within the scope of Pure Logic. ...

And then he explains ([Shearman *1906*, 207–209; quotation from p. 209]) how Russell shows how "just as addition of integers may be expressed as a logical sum of terms of two or more classes, so multiplication may be expressed as a sum of terms of a single class." The algebraic logician C.I. Lewis [*1960*, 102], however, reminds us that

> It is worthy of remark that, in respect both to addition and to multiplication, Peirce has here [*1867*] hit upon the same fundamental ideas by means of which arithmetical operations are defined in *Principia Mathematica* [vol. II, §A]. The "second intention" of a class term is, in *Principia*, Nc'α; $a + b$, in Peirce's discussion, corresponds to what is there called the "arithmetical sum of two logical classes, and $a \times b$ to what is called the "arithmetical product".

If Lewis is correct, as he is, then Shearman has selected a poor example of something being expressible in Russellian terms which is not expressible in Peircean terms. Naturally not all problems that may arise were dealt with by Peirce in his short [*1867*] paper. Nevertheless, Russell was clearly wrong to have excluded Peirce's work in his [*1903*, 111] list of contributors to the "theory of Arithmetic", along with Weierstrass, Dedekind, Cantor, Frege, and Peano. Moreover, Dürr [*1968*, 112] reminds us that in his exposition of the *Principia* in his *Survey of*

Symbolic Logic, Lewis [*1960*] helps make the reader "aware of the close connection between the older and newer form of logisitic."

In contrast to Russell's negative appraisal, most contemporary logicians found considerable merit in the achievements of the algebraists. Despite the unusually strong language which Ladd-Franklin employed in her remarks on Whitehead and Russell's treatment — or, more accurately, lack of treatment — in their *Principia* of the work of Peirce and Schröder, her view was quite typical. Indeed, logicians at the turn of the century found no clear distinction at all between algebraic logic in the Boole-Peirce-Schröder tradition and the quantification-theoretic "mathematical" logic in the Frege-Peano-Russell tradition. This was clearly Peirce's own view, judging by his remark [Peirce *1903*, MS 549:20 = 1976, III/1, 347] that Russell and Whitehead simply reformulated, in a particularly technical and formal way, results in logic that had already been established. E.B. Wilson [*1904*, 76] noted that "Boole had freed us from Aristotelianism and that C.S. Peirce and Schröder had carried the technique of logic much farther", while Maxime Bôcher [*1904*, 119] declared that, "[F]ortunately, the mathematical logicians from Boole down to C.S. Peirce, Schröder, and Peano and his followers [including, independently, Frege], have been able to make a rather short list of logical conceptions and principles upon which it would seem that all exact reasoning depends." Josiah Royce (as quoted by [Ketner *1987*, 18]) went so far as to assert that "Mr. Charles Peirce has now been for many years the principal representative in this country of a type of investigation in Logic which seems to me, as a student of the subject, to be of very great importance." Among non-mathematical philosophers, Peirce's friend William James (as quoted by [Ketner *1987*, 20]) expressed his opinion that Peirce "is in the very front rank of American thinkers...and his Logic when published will unquestionably...be recognized all over the world as an epoch-making work." William Clifford went even further, going to the extent (according to Edward L. Youmans (as reported by [Fisch *1986*, 129]) of calling Peirce "the greatest living logician, and the second man since Aristotle who has added to the subject [of logic] something material, the other man being George Boole...." This judgment is echoed by Jan Łukasiewicz, in whose Inaugural lecture of 1922 Peirce is listed as one of the most prominent representatives of mathematical logic of the day. Indeed, even Couturat [*1904*, 129–130] thought of Russell's *Principles* [*1903*] essentially as simply "*une systématisation et une synthèse*" of the work of Russell's predecessors, most notably of Peano, Whitehead, Schröder, and Russell himself. And in a letter to Russell of 27 January 1901, as we have already noted, Couturat [*1901*] warns Russell that "one must not deprecate the Boole-Schröder system and sacrifice it to Peano: it has its goal and its use in pure logic just as Peano's has its in Math. And the theory of logical equations has great importance since all logical problems amount to a system of equations and inequations."

Similarly, Russell's appraisal aside, Peano's own work had strong roots in the work of Boole, Peirce, Schröder, and the algebraic tradition. In fact, Peano, who, as we have seen, Russell regarded as the first original and important logician of modern times, began his own work [*1888*] by summarizing Boole's *Investigation of the Laws of Thought* [*1854*], Peirce's "On the Algebra of Logic" [*1880*], Jevons's *The Principles of Logic* [*1883*], MacColl's "The Calculus of Equivalent Statements" [*1877-1879*], and Schröder's "Der Operationskreis des Logikkalkuls" [*1877*]; and, as a consequence, Frege [*1897*, 370–371] described Peano as a follower of Boole (see [G.H. Moore *1988*, 109]). And, as already suggested, Peano at this time, before, and even while, launching his own project [*1889*], was foresquarely working in the algebraic tradition (see, e.g., [Peano *1889*, 102, footnote 1] of [Peano *1973*]; see also [G.H. Moore *1986*, 26] and [Kennedy *1973*, 75]). Peano made this point himself quite clearly in a letter to Russell of 19 March 1901 (quoted in [Kennedy *1975*, 206]), declaring that Russell's paper on the logic of relations [*1901*] "fills a gap between the work of Peirce and Schröder on the one hand and the *Formulaire* on the other." In a later letter, Peano writes, not of Russell's *contributions to* logic, but of his contributions to the *promotion of* logic (Peano, letter to Russell, 16 February 1906, quoted in [Kennedy *1975*, 207]). Peirce himself may have been referring specifically to Russell (e.g., [Russell *1903*, 10]) when he wrote ([Peirce *1933a*, 514]) that "[S]uch ridiculously exaggerated claims have been made for Peano's system, though not, as far as I am aware, by its author, that I shall prefer to refrain from expressing my opinion of its value." The philosopher-logician-mathematician-publisher Paul Carus, who had contacts with Peirce, Schröder, and Russell and whose journal *The Monist* served as a vehicle of public communication between these logicians, wrote ([Carus *1910*, 43]) that Schröder, Peirce, Peano, Russell, and Couturat all belonged to the line of workers who sought to broaden traditional logic by their attempts to "transfer the accomplishments of mathematics upon logic," and in particular that "[A]mong modern mathematicians Professor Peano distinguished himself by an application of the algebraic method..." ([Carus *1910*, 54]).

Especially impressive and significant in the context of appraisals of Russell's contributions by his pre-*Principia* contemporaries is the judgment of Whitehead, who, in his [*1901*, 139], in the years when Russell was strongly denigrating the work of Peirce and Schröder, declared that, "[A]s a matter of history, this algebra [of symbolic logic] has only been continuously studied since the publication of Boole's 'Laws of Thought' (*1854*), and to C.S. Peirce and to Schröder must be assigned the credit of perfecting its laws of operation. But as a question of logical priority, this algebra must be considered as the first object of mathematical study" by the two-fold right of its being "concerned with the fundamental conceptions of classes" and as "the simplest of all algebraic systems".

A few post-*Principia* historians of logic, less well known than G.H. Moore or van Heijenoort, continued to argue the continuity of the algebraic and the "quantification-theoretic" traditions. Thus, for example, J. Encinas del Pando [*1940*, 101] declared, exaggeratedly but not falsely, that the "logic of Bertrand Russell is also called the 'logic of relations'." In this, he was merely quoting Carnap [*1933*, 17]. More circumspectly, Davenport [*1952*, 159], writing under the influence of Russell's logistic version of history, stated that "relational logic finds its culmination in the *Principia*." Similarly, in his "Foreword to the German edition" of Novikov's [*1973*] logic textbook, Karl Schröter wrote that it was "in the second half of the 19th century that mathematics received a new foundation. Hereby, mathematics succeeded, essentially through the work of *C.S. Peirce, E. Schröder, R. Dedekind, G. Frege*, in being brought back to the theory of relations. A summarizing presentation was given by the well-known work "Principia Mathematica" by B. Russell and A.N. Whitehead." Recently the older view has been readopted even by Quine [*1985*; *1995*; *1995a*].

For Whitehead [*1901*, 139–140], the defects in algebraic logic were due to its simplicity, that is, to its generality and the small number of its basic principles and properties by which it provides a "practical means for the exact expression of deductive reasoning, especially in regard to the foundations of the various branches of mathematics" (see [Whitehead *1901*, 140]). What was wanted, according to Whitehead, was a systematic development of these branches of mathematics within the framework of algebraic logic. Accordingly, Whitehead devoted the greater part of his "Memoir on the Algebra of Symbolic Logic" [*1901*] to the task of applying algebraic logic to the theory of invariants and to function theory, to the theory of substitutions and the theory of prime numbers. He based his work on the earlier work of Peirce and Schröder, as well as on his own *Treatise on Universal Algebra* [Whitehead *1898*]. In fact, however, Peirce's [*1870*] "Description of a Notation for the

Logic of Relatives" is devoted in part to a discussion of the so-called "logical quaternion" as a matrix defined by the logic of relatives, and Peirce's notes and addenda to his father's *Linear Associative Algebra* [B. Peirce *1881*] are devoted to defining the various algebras presented there precisely as subsystems of the logic of relatives; this work clearly paved the way for attainment of Whitehead's avowed goals for his "Memoir on the Algebra of Symbolic Logic" [*1901*]. In light of the preliminary steps taken by Peirce and thehe defects which [Whitehead *1901*] detected were due, not to a limitation of algebraic logic, but to a need to carry out a comprehensive and systematic development of the axiomatization of universal algebra on the basis of algebraic logic. The Peirces and others, Whitehead among them, had already taken steps in this direction. The full realization of this intent, however, could only come later.

Peirce himself [*1912*, 1] severely criticized Russell's *Principles* for its shallowness, arguing that the book is sufficient merely to apprise outsiders of the basic concepts of the subject. Writing to Victoria Welby on 1 December 1903, Peirce [*1977*, 9] again turned the tables on Russell, and declared of the *Principles*, as we already noted, that "whatever merit it may have as a digest of what others have done, it is pretentious & pedantic, — attributing to its author merit that cannot be accorded to him". Peirce used the opportunity in his [*1903b*, 308 = Hardwick *1977*, 157] joint review of Welby's [*1903*] book *What is Meaning?* and Russell's *Principles* to dismiss the *Principles*, while at the same time admitting there that "the severe and scholastic labors" which went into its preparation "bespeaks a grit and industry, as well as a high intelligence," adding that "[W]hoever wishes a convenient introduction to the remarkable researches into the logic of mathematics that have been made during the last sixty years...will do well to take up this book." But these remarks, as he wrote to Lady Welby [Hardwick *1977*, 9], were intended to serve as a contrast between the two books and to hint that the *Principles* was quite unoriginal. Nevertheless, Peirce remained sufficiently interested in Russell's work at this time to write directly to Cambridge University Press on 6 February 1912 to ask for a copy of the *Principia* (information from N. Houser, personal communication). Peirce's marginal annotations in his copy of the *Principles* is littered with comments such as "not so" (p. 12, ll. 12–13), "utterly false" (p. 13, end ¶ 13), and "Ridiculous modes of formulation" (p. 16, beginning of ¶ 18). Russell must have been fully aware of the published criticisms of his *Principles*, including perhaps as well those unpublished criticisms of the work by Peirce, which we can with confidence speculate were conveyed to Russell by Lady Welby. Even prior to its publication, Russell privately realized of the *Principles* that he "cannot, in the time and present condition, finish it in style, but can patch up something that will do for publication" (as he wrote in a letter to his wife Alys of 30 April 1902; quoted in [Garciadiego *1992*, 141–142]), and (letter to Alys of 16 May 1902, as quoted in [Garciadiego *1992*, 142]) that it

> will be full of imperfections, and will raise innumerable questions that I don't know how to answer. There is a great deal of good thinking in it, but the final product is not a work of art, as I hoped it would be.

Russell's self-ctiticism with regard to the *Principles* continued unabated after the book actually appeared in print. In a letter to Russell of 4 June 1903, Couturat [*1903*] goes much further, expressing his deep dislike for the *Principles*, especially Part I. He was likewise critical of it in letters to the French historian Élie Halévy, writing on 19 July 1903 that "I am dissatisfied with it, and it only remains to hope that Vol. II, in which Whitehead and I are collaborating, will contain fewer errors and fewer unsolved difficulties" (see Letter 122 in [Griffin *1992*, 267]). A few days after the *Principles* was published, on 13 May 1903, Russell wrote to Helen Thomas of the *Principles* that "it seems to me a foolish book, and I am ashamed to think that I have spent the best part of six years upon it. Now that it is done, I can allow myself to believe that it was not worth doing — an odd luxury!" (see Letter 120 in [Griffin *1992*, 263]).

Norbert Wiener, who knew Russell personally, shared Peirce's appraisal of the man and his work. Between 1913 and 1915, Wiener was a visiting student at Cambridge, and during the Autumn of 1913 attended Russell's lectures on logic (and lectures by G.H. Hardy). In his youthful appraisal of Russell in letters home to his parents, Wiener wrote (quoted by [Grattan-Guinness *1975*, 104]) at that time that Russell's two logic courses, one for students well prepared mathematically, one for students training primarily in philosophy, "attain the acme of superficiality." Russell reciprocated these views: Wiener, we saw, was, to Russell, "disgusting" and had an inflated view of his own genius (see Letter 216 to Ottoline Morrell, 9 October 1913 in [Griffin *1992*, 480]). In contrast with his estimation of Russell, Wiener [*1953*, 190] wrote of Hardy that "in all my years listening to lectures in mathematics, I have never heard the equal of Hardy for clarity, for interest, or for intellectual power." It is therefore not clear that Wiener's views can be easily dismissed as youthful arrogance, as Russell would have it (see [Griffin *1992*, 479, n. 3]).

It is also clear that Russell's contemporaries did not unanimously share his views on the alleged inferiority of the classical Boole-Schröder algebra. Whitehead's [*1901*] "Memoir on the Algebra of Symbolic Logic" in particular is tantamount to a refutation of Russell's [*1913*] claim in reply to Wiener's [*1913*] Harvard doctoral thesis, *A Comparison Between the Treatment of the Algebra of Relatives by Schröder and that by Whitehead and Russell*, that the Boole-Schröder algebra cannot express what Russell's method expresses.

Win respect to the question of definitions *14.01, *20.01, and *30.01, which deal respectively with definite descriptions as function-theoretically defined terms for individuals, with extensional functions as predicative functions over classes, and with descriptive functions for defining relations between terms which are either individuals or classes, as we have seen,t Peirce [*1885*] had a theory of quantification, with concepts of individuality and a first-order calculus which he had developed explicitly within the logic of relations. Moreover, Peirce [*1885*] used his connective (—<) for material implication and had introduced truth-functional analysis. As early as 1870, Peirce abandoned equational logic in favor of — or, more accurately perhaps, transformed it into — a quantification-theoretic logic defined by his relational calculus. It was Schröder who took a small part of Peirce's work and developed it as the comprehensive and complete, final version of classical Boole-Schröder algebra; equational logic was of more interest to Schröder than it was to Peirce after 1870, and Peirce went so far as to judge that, in this narrowing of his range of interest, Schröder was mistaken. Thus, Russell's [*1913*] criticism of Wiener's thesis must be seen to apply specifically to Schröder's *Algebra* and not, in general, to the Boole-Schröder logic. At the same time, Schröder's system of algebraic logic must be understood as a fragment of the symbolic logic of algebra being developed by Peirce. We can say that Schröder worked out the algebra of relations and the class calculus that had been developed by Peirce, and that Schröder's system, the classical Boole-Schröder algebra, was a fragment of the broader algebraic logic developed by Peirce.

In a letter of 25 December 1909 to William James, Peirce described his own work in logic, and in particular his work "in the Logic of Relatives" as a work that "simply revolutionizes Logic" (see [Peirce *1976*, III/2, 867–877, especially pp. 873–874]). Following an enumeration of his results in the calculus of relations, quantification theory, truth-functional logic, and universal algebra, he expresses his belief that all of these, taken in their unity as the algebra of logic, "ought to be the Logic of the Future" ([Peirce *1976*, III/2, 874]). Peirce was certainly therefore no more reticent about taking credit for his own contributions than Russell was about surreptitiously taking credit for the contributions of others. On the other hand, Peirce appears to have been more willing to credit others for their contributions than was Russell. A salient example of the latter (which we shall examine momentarily) was Russell's assumption of credit for more work on *Principia* than the evidence suggests was his rightful due.

Despite all of Russell's denigrations of the Boole-Peirce-Schröder tradition, G.H. Moore [*1988*, 110] reminds us that Russell [*1903*] retained a division of logic — into propositional calculus, the class calculus, and the relational calculus — "that was much more in the tradition of Boole, Peirce, and Schröder than in Frege's". For as Sluga [*1987*] points out, Frege, whose primary target was Schröder, was another vigorous opponent of the algebraic tradition (see [Frege *1895*]). But Frege [*1880-1881*; *1882*] also argued that Boole's work was actually a step *backward* from the work of Leibniz. At least Russell was not that radical.

Nevertheless, it was Russell's antagonism to the Boolean tradition that led to the separation of the algebraic tradition from the mainstream of logical development during the first decade of the twentieth century and to the denigration of the algebraic tradition, first by those of Russell's contemporaries who stood outside of the algebraic tradition, and later by historians of logic for whom Russell's overpowering influence and authority proved, if not inescapable, then certainly presumptive.[3] To those belonging to the so-called "algebraic" tradition, it was clear that "modern logic is really due to Boole and De Morgan" ([Jourdain *1914*, iv]), and that "the algebra of logic was founded by George Boole (1815 – 1864) [and that] it was developed and perfected by Ernst Schröder (1841 – 1902)" ([Couturat *1914*, 3]). This is in sharp

[3] Carl Spadoni (*1977*) has argued that Russell never entirely abandoned his youthful neo-Hegelianism, and Garciadiego [*1992*, 55] suggests that Russell's hostility to algebraic logic was an aftereffect or residue of this neo-Hegelianism, noting that "neo-Hegelians were strongly opposed to what they called 'logic', which developed into algebraic logic or Boolean algebra." And Rodríguez-Consuegra [*1991*, 75] notes that, even in his unpublished (*1899a*] paper "The Classification of Relations", Russell ended by admitting that if one accepts relations, one is, unfortunately, doomed to accept a Bradleyan regress, i.e. an infinite relational regress, and states that Russell never overcame this problem. (For a discussion of the neo-Hegelian logic of Francis Herbert Bradley (1846 – 1924) and its influences, including its influence on Russell, see [Manser *1983*].)

contrast to Russell's claims that Peano was the founder of modern logic and that algebraic logic faced a dead-end until salvaged by incorporation into the quantification-theoretic tradition by Whitehead and Russell in the *Principia*.

Russell's personality and style, and what we can conclude therefrom. Although, as we have seen, Peirce was capable of occasional engagement in *tu quoque* arguments against Russell, Russell had gone out of his way to belittle the achievements of Peirce and indeed of the entire algebraic tradition from Boole to Schröder. At the same time, he exhibited great pride in his own work. A.J. Ayer, a close friend of Russell's, assuredly expressed the truth about Russell's view when he said [Ayer *1972/1988*, 17] that Russell "no doubt with good reason, attached the greatest value to the work which he did on mathematical logic... ." But perhaps the reason that he found it so essential to downplay the contributions of the entire Boole-Peirce-Schröder tradition was precisely to enhance the perception of his own works, for, as [Henkin *1962*, 789] has noted,

> Boole demonstrated that through the use of...algebraic notation one can effect a great saving in the effort to collate and apply basic laws of logic. Later his work was extended and deepened by the American C. S. Peirce and the German mathematician E. Schröder. And Russell himself, *working within this tradition*, found it a convenient basis for a systematic development of all mathematics from logic. By combining the symbolic formulation of logical laws with the reduction of mathematical concepts to a logical core, he was able to conceive a unified development such as was attempted in the *Principia Mathematica.* [my emphasis]

[Henkin *1962*, 788] certainly agreed that "Russell was a great popularizer" of ideas. He was arguably the most influential popularizer of the so-called Frege-Peano-Russell quantification-theoretic logic. But if the *Principia* was, after all, essentially a systematization, as Whitehead had sought it to be and as Henkin saw it to be, and in the tradition of Boole, Peirce, and Schröder, as Henkin claimed, and given the the various difficulties which Russell had with mathematics (some of which we enumerate momentarily), then it is not difficult to comprehend Russell's extreme sensitivity to questions concerning the originality and extent of his contributions to logic, and why he might wish to belittle the contributions of others to correspondingly enhance his own contributions. Russell provides some of his own evidence of insincerity in his appraisal of Peirce and Peirce's work, saying at one point ([Russell *1946*, xvi]) that Peirce appeared to him to be "a volcano spouting vast masses of rock, of which some, on examination, turn out to be nuggets of pure gold."

Peirce was also quite capable of praising himself extravagantly on occasion, comparing himself, for example, to Leibniz, and even to Aristotle (as quoted by [Fisch *1972*, 486]). But he is merely echoing Schröder (letter to Peirce, MS L392: 16 February 1896, and letter to Paul Carus, MS L421: 6 March 1893; quoted by [Fisch *1972*, 487, 488]). On the other hand, he was able to say ([*1903*, MS 459:20 = 1977, III/1, 347]) that "I am not so in love with my own system as the late Professor Schröder was," adding in his letter to Lady Welby of October 12, 1904 ([Peirce *1977*, 29]) that "my friend Schröder fell in love with my algebra of dyadic relations. The few pages I gave to it in my [1883] were proportionate to its importance." We might add that he was not so much in love with his own system as Russell was with his and Whitehead's. Is this Peirce's justifiable confidence in the merit of his work, compared with Russell's insecurity?

G.H. Hardy, the Cambridge mathematician who was close to Russell during the period when the *Principia* was being written, wrote ([Hardy *1967*, 83]) that "I can remember Bertrand Russell telling me of a horrible dream," in which

> He was in the top floor of the University Library, about A.D. 2100. A library assistant was going round the shelves carrying an enormous bucket, taking down book after book, glancing at them, restoring them to the shelves or dumping them into the bucket. At last he came to three large volumes which Russell could recognize as the last surviving copy of *Principia mathematica*. He took down one of the volumes, turned over a few pages, seemed puzzled for a moment by the curious symbolism, closed the volume, balanced it in his hand and hesitated....

Similarly, Hardy's collaborator, the Cambridge mathematician J.E. Littlewood, also an acquaintance of Russell's in those years, suspected ([Littlewood *1986*, 128]) that Russell "had a secret craving to have proved *some* straight mathematical theorem. ...(This weakness is very common with people who take the Mathematical Tripos and then switch... .)"

An objective analysis of his criticisms of Peirce and Schröder suggests that Russell did not always completely understand the ideas of others. It has similarly been shown (by [Anellis *1984*; *1986* & *1987*; *1987*; *1987a*]) that Russell thoroughly misapprehended the Cantorian conceptions of infinity and the theory of real numbers and the continuum. This appraisal has recently been reenforced by Alasdair Urquhart, who, upon reading the chapter on pure mathematics in Nicholas Griffin's book *Russell's Idealist Apprenticeship*, wrote in his review [Urquhart *1993*, 107] of that book that "the chapter on pure mathematics"

> makes depressing reading for admirers of Russell. Russell's thinking on measurement, quantity and continuity at this time was extremely muddled, and showed his mathematical ignorance in its starkest form. We are presented with "antinomies" of quantity (p. 260) which are no more convincing than the corresponding geometrical "antinomies". Worse yet, Russell's criticism of Cantor's theory of transfinite numbers rests on elementary howlers (p. 242).

Moreover, a comparison of Russell's published statements and some of his more formal but unpublished remarks with his informal private remarks shows that there exist discrepancies that are not easily dismissed — for example in Russell's published disparagement of Peirce and the private admission to Lady Welby ([Russell *1904b*, 1]) that he has "a great respect" for Peirce's work, so that "it would be the greatest interest & pleasure for me to meet Mr. C.S. Peirce," that "the little [Peirce] has published is tantalizing" ([Russell *1904b*, 3]), that he ([Russell *1905*, esp. p. 2]) did not perhaps understand Peirce's work, and ([Russell *1904b*, 3]) that "the number of readers who will profit by work such as his, without the benefit of his personal explanations, is necessarily small," although ([Russell *1904a*, 1–2]) he takes exception to Peirce's views concerning triadic relations. Except for this type of admission, it might be possible to discharge to faulty memory or minor chronological confusion Russell's published claim that he did not become aware of Peirce's work until 1900 in the face of his private suggestion to Couturat in 1899 that the latter might find Peirce's *Studies in Logic* to be of interest. Russell's [*1904a*] claim he "always thought very highly of Dr. Peirce for having introduced such a method" seems under the circumstances to be somewhat disingenuous. It is more credible that Russell indeed regarded Peirce as an "opponent" to be persuaded, through volume two of the *Principles*, of the correctness of his own views. Russell admitted as much in his letter to Welby of 11 November 1904 ([Russell *1904*, 3]).

Peirce's suggestions that Russell sought in the *Principles* to assume credit for work which had done by others and which he merely summarized or expounded may appear at first sight, as Peirce himself ([Peirce/

Ladd-Franklin *1891-1908*; MS L237: July 27, 1904]) thought to an impartial observer it might, to be "simply the resentment of the old man who is getting laid upon the shelf." However, this methodology of expropriation would readily account, for example, for Russell's total failure (see [Anellis *1984*; *1986 & 1987*; *1987*; *1987a*]) to make distinctions between natural numbers, cardinal numbers, rational, irrational, and real numbers all through the period 1896-1899 in his first attempts to discuss Cantorian set theory and his sudden and otherwise unaccountable expertise in set theory after 1900. The "turning point" in this instance, as [Anellis *1986*; *1987a*, 317–318] noted, seems to have been Russell's discovery of the textbook *Introduction to the Theory of Analytic Functions* of [Harkness and Morley *1898*] and Russell's verbatim adoption (in his [*1899?*] note "On Number") of its number-theoretic schema. Thus, before Russell had access to Harkness and Morley's text, his own repeated attempts to classify numbers were based on outmoded classical Greek concepts; but after finding Harkness and Morley's text, he followed the modern Cantorian-Dedekindian-Weierstrassian conception that was advanced by Harkness and Morley.

It is also difficult to dismiss the many conflicting claims which Russell made concerning the nature and extent of his collaboration with Whitehead as anything less than obfuscation in an attempt for self-aggrandizement. Russell's various statements about his collaboration with Whitehead, particularly as related to the *Principia*, raises questions about Russell's veracity as well as questions concerning the relative contributions of the two men to the enterprise. In a discussion of the Whitehead-Russell correspondence, Lackey [*1972*, 14] outlines the methodology used in writing the *Principia.* Russell began by writing a first draft, which was then sent to Whitehead, who critiqued it and made any corrections which he felt necessary, then returned the corrected draft, with his comments, to Russell, who prepared the final draft. Lackey quotes Whitehead's praise of Russell's work; but if we examine their context, we find that nearly all of these refer to the specifically philosophical contributions, notably the theory of types and the theory of descriptions. Moreover, we notice that Whitehead and Russell disagreed on the nature and purpose of their book — Russell, says [Lackey *1972*, 15] "viewed **Principia** more as a **criticism** of the whole of mathematics; Whitehead saw it more as a **systematization** of that whole." (In light of the fact that there are, as [Gandy *1973*, 346] has pointed out, few *new* results in the *Principia* that could not already have been found elsewhere, Whitehead's perception is the more accurate one.) One point of technical disagreement between Russell and Whitehead concerned the need for an explicit statement of the axiom of infinity. [Lackey *1972*, 15] concludes from the correspondence that Whitehead felt the need for a separate axiom, while Russell initially did not, but that Whitehead finally persuaded Russell of the need. Grattan-Guinness [*1986*, 63], discussing Lowe's [*1985*, 279–280] consideration of the correspondence between Russell and Whitehead relating to the axiom, says that Whitehead used the axiom with great frequency, and was forced by his own use of the axiom to finally state it explicitly. Lowe [*1985*, 121–294] himself personally studied the surviving Russell-Whitehead correspondence in detail, as well as their collaboration. His remarks make it clear that Whitehead argued in favor of making the axiom explicit. He also shows that Whitehead was frequently critical of Russell's mathematical efforts, finding it necessary to correct or revise Russell's incorrect, incomplete, or sketchy proofs.

Lowe [*1974*, 23] noted that Russell destroyed some of the correspondence which he had received from Whitehead that had been written during the period of work on the *Principia*, correspondence which Russell himself admitted contained harsh judgments of some of his work for *Principia*.[4] With respect to one such letter, only two sentences were saved: "Everything, even the purpose of the book, has been sacrificed to making the proofs look short and neat. It is *essential*, especially in the early parts, that the proofs be written out fully." When Lowe asked Russell why only those two particular sentences were saved and the remainder of the letter destroyed, Russell replied that it was "because they show that the fullness of 'Principia' is due to Whitehead" ([Lowe *1974*, 23; *1985*, 263]). But these lines were never used by Russell to give the greater credit to Whitehead for the proofs in the *Principia*. Lowe [*1974*, 23; *cf. 1985*, 294] concluded that Whitehead and Russell's "teacher-pupil relationship had not wholly disappeared" during these collaborative years.

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[4] That Russell should have deliberately destroyed any of his correspondence is certainly peculiar, since Russell habitually saved almost every scrap of paper that he could, including those with the most ordinary or worthless writings. Among the correspondence preserved among his papers at the Russell Archives, for example, is an exchange between Russell and his local distillery, in which he requests a new case of their "Red Hackle" brand — Russell's favorite — whiskey to replace the one that had been stolen, along with the reply from the distiller expressing regret for the theft along with satisfaction that the thief had the good taste to steal their brand.

Russell in his published [*1948*] account of his collaboration with Whitehead, seems to waver between suggesting that the bulk of the work of *Principia* was his, with some minor assistance from Whitehead, although he does not explicitly say so and indicating that the work was fairly evenly shared between the two. He told Littlewood (there is no indication of when, see [Littlewood *1986*, 128]) that "he did all the dirty work [for *Principia Mathematica*], since Whitehead was a hard-working lecturer." But Griffin [*1992*, 275; 277] points out that work on the *Principia* "went on mainly in the summers when Whitehead was free from teaching;" that, on 9 August 1904, for example, he spent an entire day talking about how to analyze "The present King of France is bald." Littlewood's [*1986*, 128] statement that Russell "had a secret craving to have proved *some* straight mathematical theorem" was followed up immediately by the assert that "as a matter of fact there is one: '$2^{2^a} > \aleph_0$ if a is infinite'. Perfectly good mathematics." Boolos [*1993*, 757] has gone so far as to argue, not only that this theorem is Russell's, but that it is the mathematical core of *Principia* and that, more generally, Russell's work cannot be improved upon. In that case, we must ask *why* Russell would gave had, or felt any need of, a "secret craving" to have proved a mathematical theorem if he had already proven one such as this, and many more as well, scattered throughout the *Principia*? The evidence suggests that the proof was most likely Whitehead's, not Russell's.

Many years later, in his intellectual autobiography [*1959*, 74], Russell asserts that Whitehead did all of the mathematics for the *Principia*, other than the section on series, without, however, disavowing the claim that the work was otherwise fairly evenly shared. In a private 1968 interview with D.G. King-Hele, published by King-Hele after Russell's death ([King-Hele *1974-75*, 23], Russell is reported to have admitted that Whitehead provided proper mathematical proofs and did all the polishing." Examining the surviving correspondence, [Lowe *1985*] concluded that Whitehead all of did the mathematics for the *Principia* and Russell did the philosophy. In particular, [Lowe *1985*, 291–292] concluded that "Whitehead was the critical mathematician who developed and consolidated the ideas involved in Russell's logicist thesis and (except for the theory of descriptions and the theory of types) took the lead in giving them accurate symbolic expression." In a review of [Lowe *1985*], Quinton [*1986*] therefore summarizes Lowe's conclusion as "mak[ing] it clear that although Whitehead and Russell were in constant and fruitful touch during the composition of *Principia Mathematica*, a pronounced division of labor prevailed. Whitehead did the mathematics;

Russell did the philosophy." The not altogether satisfying explanation for Russell's having attained the dignity of being perceived by many as the "primary" author of this book is that it "excited philosophers but left mathematicians cold" ([Quinton *1986*]).

Grattan-Guinness [*1992*, 3] tells us that Popper and Russell "held each other in high esteem." In 1947, Popper (quoted by [Grattan-Guinness *1992*, 13] in the original German, and in English translation in [Popper *1992*, 21]), analyzed Russell's "greatness" in terms of Russell's willingness not merely to alter his opinion, but to do so "openly and without beating further about the bush," adding that Russell was

> The only philosopher who *did not pose as infallible*, but who openly admitted that he could *err*; who through this act proved that to him only one thing was important: *to learn, and to seek, the truth*. I do not know *how often* Russell has altered his opinion, but I know: every time *when* he does it, it signifies *progress* in philosophy. He never altered his opinion without bringing forward good, very good reasons for the modification. And he would always give his reasons with great openness and simplicity. This sincerity and intellectual incorruptibility, this selfless devotion to truth and to reason, the simple humanity, that is the man.

Popper's analysis of Russell's "openness" indubitably applies to the published writings; but this Popperian pronouncement of Russell's "sincerity and intellectual incorruptibility" does not accord well with the rest of the evidence, and in particular with the disparity between Russell's private admissions and his public pronouncements that we have enumerated. A perhaps typical example of differences between the public and private ways in which Russell dealt with depictions of his professional relationship with Whitehead (and thereby more generally to the history of logic) comes from a description of an item owned by Main Street Fine Books & Manuscripts (Galena, Illinois). Item #62 in their *Catalog* No. 3: "Russell, Bertrand: Printed DS, 1p., 7" × 10 $^1/_4$", Cambridge, England, 1948 May 16" (as described in [Russell Society News *1993*]) is an "Updating form for *Who's Who in America*, to which Russell's 2" × 3" biographical entry from the previous edition has been affixed." In the form, Russell crossed out the line reading "(with Prof. A.N. Whitehead) *Principia Mathematica*, 1910-13" and written in "*Human Knowledge:Its Scope and Limits*, 1947" and signed the form. The item comes with a second, similar sheet, to which Russell added the same information. This single incident is insufficient by itself to allow us to conclude that there was any attempt at deception on Russell's part; it may by itself rather reflect a mere change in his opinion about the relative importance of the two works. We may just as easily account for Russell's ambiguous references to the *Principia* specifically and to his collaboration with Whitehead generally in his the first of his [*1952*] BBC radio interviews ("Portraits from Memory), devoted to Whitehead, by noting that these interviews were intended for a general audience.

In the BBC interview, Russell recalled that he first met Whitehead in 1890 when as a Cambridge freshman he attended Whitehead's lectures on statics. According to Russell, Whitehead told the class to study article number 35 of the textbook, then turned to Russell to advise him that he need not study the article inasmuch as he already knew it, having quoted it by number in his scholarship examination. Russell then ambiguously asserts that, throughout his "gradual transition from student to independent writer," he "benefitted from Whitehead's guidance," without, however, making clear either the nature or extent of his transition from student to independent thinker, or the nature and extent of the benefits he derived from Whitehead's guidance, or of the role of Whitehead in his transition. Russell remembered the pivotal episode of this transition to be the fellowship examination in 1895; Whitehead had severely criticized Russell's examination, but had done so because it would be the last time he could speak to Whitehead within the context of a teacher speaking to a student. Russell testified that, in 1900, when he began to think independently, he was able to convince Whitehead that his ideas were "not without value," and they became the basis of the ten-year collaboration between the two "on a big book, no part of which is wholly due to either". Nowhere in the interview did Russell mention the *Principia* by name. Moreover, Russell failed, during the course of his interview, to delineate the nature, length or extent of Whitehead's guidance through the transition from student to independent scholar or the delineation of the division of labor in their collaboration. This does not accord well with Lowe's [*1974*, 23] conclusion that Whitehead and Russell's "teacher-pupil relationship had not wholly disappeared" during these years. The importance of these examples comes from the pattern of [seeming] duplicity in Russell's treatment of his descriptions relating to Whitehead.

It is difficult to find another explanation for Russell's numerous waf-flings, and especially for the differences between his private and his public statements, than either intellectual dishonesty, or a desire for popularity.[5] One explanation that is less unflattering to Russell's honor but more damaging to his intellectual reputation is that Russell simply did not have the abilities in mathematics generally or in logic specifically that he is believed to have had and which he claimed for himself. Some Russell scholars such as [Anellis *1987b*, esp. pp. 151, 153–154, 169–170] and [Griffin & Lewis *1990*] have attempted to account for Russell's difficulties with mathematics and to salvage his reputation by focusing upon the negative aspects of the Victorian education in mathematics that he received at Cambridge. But then one is left to explain how such of his senior classmates and teachers as Hardy and Whitehead had managed, in the teeth of that very same atrocious pedagogical system, to understand the contemporary German mathematics in a way that Russell did not. We must also explain how Russell could have done so well mathematically in the *Principia* when he had the assistance of Whitehead, but conveniently found that the effort of writing the *Principia* was so great that he had to abandon mathematics for once and all after he no longer could rely upon the assistance of a Whitehead. In fact he frequently excused himself from doing work in logic after work on the *Principia* was completed by saying that the effort of writing the *Principia* had exhausted him and left him incapable of doing any more mathematical work. Littlewood [*1986*, 128], for example, recalled that Russell "said that *Principia Mathematica*... took so much out of him that he had never been quite the same again." (Thus [Ayer *1972/1988*, 19] is accurately expressing Russell's explanation by stating that "after the years of labor which he expended on 'Principia Mathematica', he became impatient with minutiæ.") This is the explanation which Russell

[5] Russell's intellectual career being a long one, his ideas matured and his knowledge grew; so it is not surprising that he should have changed his mind and his philosophical positions many times throughout his life. One may even point to the various alterations, some quite fundamental, in successive drafts of the *Principles* (see, e.g., [Blackwell *1984-85*; Byrd *1987*; Byrd *1994*]). The changeability of Russell's positions has frequently been noted by numerous scholars, to the extent that some Russell scholars (e.g. Weitz *1944*) found it worthwhile, even necessary, to account for Russell's philosophical shifts and point to the underlying unity of Russell's thought despite those shifts. One may find such fluctuations in many writers. But I would argue that the changes in Russell's philosophical positions throughout his long scholarly career are neither of the same magnitude nor type as the shifts between his statements for public and private comsumption, in which he presents the important and often pivotal events in the statements for history of his intellectual life and the influences exerted in his intellectual life by various thinkers or acquaintances.

gave to Henkin when in 1963 Henkin requested Russell's comments on [Henkin *1962*], much of it dealing with Russell's conception of the nature of logic and the impact which Gödel's incompleteness results had on that conception. Russell's explanation has obtained popular acceptance and is given credence by Russell scholars and historians of philosophy generally. In his review of Lee Eisler's new book *The Quotable Bertrand Russell* in the pages of the September 28, 1993 number of the newspaper *The Intelligencer* of Doylestown, Pennsylvania, Daniel C. Church [*1993*] writes, for example: 'The landmark "Principia Mathematica," Eisler says, exhausted Russell. "He thought it hurt his mind," he adds.'

Russell's [*1963*] reply to Henkin suggests another explanation for his reluctance, even outright unwilligness, to carry out further work in logic after completing work on the *Principia*. There, Russell's reply was that Gödel's theorem showed, not that (primitive recursive) arithmetic is incomplete, but that it is inconsistent, that it permitted "school-boy" arithmetic to allow that "2 + 2 = 4.001". This reply (and its "April Fool's" date) prompted Henkin to ask me ([Henkin *1983*]) whether Russell was joking; but the entire tenor of the letter, together with the philosophical background on which Russell drew to conclude that Gödel's results allowed school-boy arithmeticians to have 2 + 2 = 4.001, shows that Russell was in earnest. In his reply to [Russell *1963*, 2], Henkin [*1963*] therefore found it necessary to explain to Russell that Gödel's results did not say that arithmetic is inconsistent, but that it is incomplete. (Rodríguez-Consuegra [*1993*] admits that there is evidence that Russell did not fully comprehend Gödel's results, but did understand the gist of it. At the same time, Rodríguez-Consuegra admits that Russell sometimes failed to distinguish between a theory and a metatheory or between inference rules and axioms, and thought that Gödel's incompleteness results could be overcome by a judicious application of the theory of types.)

It is not strictly true, however, despite statements by Russell to Henkin, Littlewood, and others, to the contrary, that Russell did not attempt work in logic after completing work on the *Principia* — even if he really meant to say that he made no further efforts to work in logic after completing work on the second edition of the *Principia*. For example, Russell planned a series of private lectures on mathematical logic to be conducted during the Winter of 1917-1918. The lectures were to be held on Tuesday evenings, in a hired room in Dr. Williams's Library, on Gordon Square in London. Of the planned series, the first two lectures were actually held. They first met on 30 October and 22

December 1917; the second was held on 22 January and 12 March 1918 (see [Slater *1986*, 157–158]). The series was disrupted only by Russell's imprisonment for anti-war activities soon after the second lecture was completed. The lectures were rewritten in May 1918, while Russell was in prison, and published on 4 March 1919 as *Introduction to Mathematical Philosophy*. Moreover, Russell first raised with Cambridge University Press the question of a reprinting or new edition of the *Principia* in early 1920, and worked on the second edition in the Summer of 1923 — entirely without Whitehead's collaboration, ignoring Whitehead's criticisms of the theory of types (see, e.g. [Lowe *1990*, 273–77]). Nor did Russell ever entirely give up all work in logic. [Blackwell *1976*, 18] reported that Russell continually made alterations in the *Introduction* until as late as 1967. One might make a case in support of Russell that the *Introduction* was after all merely a "semi-popular" summary of the principal ideas of the *Principia*; but this argument does not alter the fact that Russell was still working on logic well after he had supposedly been exhausted and incapable of doing any more mathematical work by the efforts of working on the *Principia*.

We must also consider that, if Whitehead's biographer Victor Lowe is to be believed, Russell frequently relied upon Whitehead whevever he encountered difficulties. In a letter to Lowe of 18 June 1941 (quoted in [Lowe *1985*, 195n), Russell admitted that in 1898 his ideas [in this particular instance, presumably on the nature of mathematics and its relation to logic] "were still in confusion." Lowe [*1985*, 229] averred that Russell would always ask for Whitehead's help when he was "baffled" by a mathematical problem or concept, and Lowe cites a letter from Russell to his wife Alys written in early 1897 as just one instance in many that Russell declared in private that he needed Whitehead's assistance. In his account of Russell's mutilation of Whitehead's letter to him (identified by Lowe as concerning *1 – *5 of *Principia*) in which only two sentences were preserved, in which Whitehead there tells Russell that "everything, even the purpose of the book, has been sacrificed to making the proofs look short and neat. It is *essential*, especially in the early parts, that the proofs be written out fully," Lowe [*1985*, 263] that Russell later admitted that "Whitehead was entirely right." Whitehad's comments (as quoted by [Lowe *1985*, 263–264] on Russell's draft of *9 of *Principia* are that:

> The impression I have from a careful study is of very eleaborate definitions which are not used, some proofs very careful, others equally important carried out by common sense in the style of Euclid.

> The *proofs* are the *only* interest of this subject — no one will read this unless the *proofs* are careful and minute.

Lowe argues throughout his discussion of the collaboration between Whitehead and Russell that Whitehead had to couch his specific criticisms of Russell's work within a framework of general praise, in order to avoid damaging Russell's tender ego.

Before finally condemning Russell's acume and abilities outright, however, we ought to consider whether there exists the possibility that Lowe himself had an axe to grind in restoring greater credit to Whitehead than is commonly given to him for his contributions to the *Principia*. To start, let us note that Russell [*1948*, 138] himself, speaking of his collaboration with Whitehead on the *Principia*, confessed that:

> Whitehead was more patient and accurate and careful than I was, and saved me often from a hasty and superficial treatment of difficulties that I found interesting. I, on the other hand, sometimes thought his treatment needlessly complicated, and found ways of simplifying his drafts.

Russell is not here exactly admitting incompetence, of course, so much as apparently confessing that he allowed his enthusiasm to give rise to simple carelessness. But Lowe can easily be concluded, from Whitehead's seeming need to continuously admonish Russell to repair or provide more detailed proofs, to have implied that Russell simply was frequently simply incapable of writing correct proofs. For his part, Whitehead once told Quine that "Bertie thinks I'm muddle-headed, but I think Bertie's simpleminded" (quoted in [Quine *1985a*, 79).

Lowe's implication would seem to be further substantiated by Quine's [*1941*, 157; *1966*, 31] complaint that (in §3 of [Whitehead *1902*], which was actually written solely by Russell) he found the proofs of theorems to be "sketchy" and otherwise incomplete, or to have other errors and omissions, and that he was altogether "unable to decipher the purported proof that $n \neq n + 1$ for finite n" given there.

It is for others to determine whether Russell's methodology sprang from ineptitude or deceit, from a mixture of both, or (if I am wrong) neither. However, it is evident that Russell's treatment even of logicians whose work he admired have to some extent suffered from treatment at his hands. The evidence cited by [G.H. Moore *1989*] suggests that Russell was not on occasion adverse to falsifying history even for the sake of "filthy lucre." It would appear, then, that Russell would deliberately distort the history of logic in order to play up his own role in that history — or even for greed — while attempting to cover his tracks in case anyone, like Couturat, might otherwise find him out. [Geach *1959*, 72] has also accused Russell of distortion of the ideas of others, and Leśniewski is reported (without substantiation; see [Skolimowski *1967*, 245, n. 5] to have become exasperated with Russell's "oversimplifications and distortions" of Frege's ideas and with mistakes made in the name of Frege that Frege did not make.

Something of Russell's personality may be gained from William James, who expressed a much harsher judgment of Russell when in [1909] he told Peirce (William James, letter to C.S. Peirce, 24 December 1909; as quoted by Ralph Barton Perry [*1935*, 680]) that "I am a-logical, if not illogical, and glad to be so when I find Bertie Russell trying to excogitate what true knowledge means, in the absence of any concrete universe surrounding the knower and the known. Ass!" A similar attitude towards Russell's personality is evident in Norbert Wiener's impressions, made while Wiener was a visiting student at Cambridge and in attendance on Russell's logic course lectures. In a letter home to his parents in the autumn of 1913, Wiener wrote (quoted by [Grattan-Guinness *1975*, 104]) that

> I have a great dislike for Russell; I cannot explain it completely, but I feel a detestation for the man. As far as any sympathy with me, or with anyone else, I believe, he is is an iceberg. His mind impresses one as a keen, cold narrow logical machine... ,

from which it would appear that Russell exhibited little respect for colleagues, particularly those who did not share his views.[6] And as Josiah Royce had noted (as recalled by [Lewis *1968*, 6]) — I think correctly — the one marked characteristic of Russell is "a certain cheery dogmatism." Wittgenstein's acquaintance F.R. Leavis [*1981*, 69] (admittedly not the best character witness, since he knew neither Russell nor Wittgenstein very well) gave an even more unflattering description of Russell's personality in contrasting Russell with Wittgenstein, claiming

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[6] The late Henry David Aiken once recalled his experience meeting Russell at Harvard. Aiken recalled there being considerable excitement among Harvard's philosophy department graduate students surrounding Russell's impending visit, and an expectation that Russell would speak about his work in logic. Russell arrived late for his lecture, and instead of speaking on logic, delivered a tirade on the benefits of free love. Aiken remembered Russell as pompous and arrogant, and his most lasting impression of this encounter was of "Russell's great horse face."

that Wittgenstein, unlike Russell, "was a complete human being, subtle, self-critical and un-self-exacting" in support of the assertion [Leavis [*1981*, 67] that Wittgenstein was immensely superior to Russell as a person both with respect to intelligence and morality. However similar portraits of Russell were given by those who had made his acquaintance. In a review of Caroline Moorehead's biography of Russell, Hermoine Lee [*1993*, 22] quotes one of Russell's fellow pacifists saying in 1918 that Russell was "very childlike in his engrossment with his own emotions, virtues, vices, and the effect he has on other people. The oddest mixture of candour and mystery, cruelty and affection." This quote is followed by a quotation from Colette O'Neil, whom Lee (*ibid*) describes as Russell's "lover, on and off, for more than thirty years": "When BR really wants anything, he lets NOTHING WHATEVER stand in the way of getting it. He has always been like that."

Conclusions and queries. I conclude that there is a good deal of truth in the assertion of Dieudonné [*1983*, 107] that Russell "claims to have the reputation of being a mathematician and succeeded in doing so in the eyes of contemporaries (and even today in the eyes of a number of philosophers)," providing the limitation is added that he succeeded largely in the eyes of his younger contemporaries and in those of the following generation, but not in the eyes of his elders. Victor Lenzen, for example, sent on a mission by Harvard to the home of Peirce's widow in order to retrieve Perice's library for Harvard as the widow's gift to Peirce's *alma mater*, cites his own selection of Peirce's copy of Russell's *Principles* as a "demonstration of the commanding influence of Bertrand Russell upon graduate students" ([Lenzen *1965*, 7]). We also conclude that Russell's reputation as a mathematician is not wholly deserved, attained as it was at least in part at the unacknowledged expense of others, through an admitted desire to popularize and impress, and by selective memory as evidenced in the disparities between private admissions and public assertions. As Gandy [*1973*, 342] has said regarding Russell's intellectual exertions, Russell's contributions to mathematics "appears a poor return" on the ten years (from 1900 to 1910) that he devoted to the subject. And we may at the same time point to Russell's own private admission (quoted, without citation by [Garciadiego *1982*, xi]), that "The love of power is terribly strong in me — I can't help reflecting that all these math[ematic]'al philosophers have different thoughts from what they w[oul]d. have had if I had not existed."

The final questions would therefore seem to be:

(a) to what extent would the thoughts of mathematical philosophers be different if Russell had not existed?

(b) how much of the difference is owed to Russell's skills as promoter and popularizer rather than to his own mathematical abilities and discoveries?

(c) to what extent did Russell really contribute to something new and important to mathematics over and above the unfication and systematization that he carried out on the work of his predecessors and colleagues?

and, finally,

(d) how much of the unfication and systematization is directly attributable to Whitehead rather than to Russell himself?, that is, how far, realistically, could Russell have gone in working alone on the *Principia* without the aid of Whitehead and still attained equivalent results?

These questions are raised by the disparities which we have noted between Russell's private and public pronouncements, by the lapses and shifts or modifications in his memory over time as recorded even in his public testimony that we have traced, by the assessment of his work by colleagues and contemporaries such as Peirce that we have examined, and by the level of difference which appears to occur between his accomplishments in mathematics made by his own efforts and those which he carried out under Whitehead's explicit guidance or assistance, in particular if we accept the presumption that not all of the technical problems in Russell's earlier writings on logic and mathematics were resultant specifically or exclusively from his early neo-Hegelianism. The answers to these questions should go a long way towards illuminating both the history and the historiography of logic for much of this century.

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