COMMENTS ON STEVENS' REVIEW OF *THE CAMBRIDGE COMPANION* AND ANELLIS ON TRUTH TABLES

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In his review[1] of the Griffin *Companion* on Russell, Stevens calls into question several aspects of my reading of the mathematical aspects of Russell's logic. Some clarifications are in order.

(1). One of the most innovative aspects of recent Russell studies is the upgrading by Landini of Russell's substitutional logic, in which Russell had dispensed with propositional functions and relations and worked only with propositions, their truth values, and individuals; the latter were substitutable into a proposition, and propositions into compound propositions, by means of a notion called "matrix". Russell developed the theory from about mid-1905 to early in 1907, in a large collection of manuscripts that we may be able to read one day complete in *Papers* 5. He came to see it as the means to achieve logicism; but then he found various difficulties with it, including a paradox.[2] Thereafter it was only a residue in his definitive theories of types, where propositional functions and relations were back on centre stage.

This is my view, recorded in my article in the book; but Stevens sees the

[1] Graham Stevens, review of Nicholas Griffin, ed., *The Cambridge Companion to Bertrand Russell*, in *Russell*, n.s. 24 (2004): 79–94.

[2] A main source of this paradox is a letter by Russell to R. G. Hawtrey. Both Stevens (p. 88) and Landini (p. 281 of the *Companion*) locate it in the Russell Archives; in fact, the original letter is among the Hawtrey Papers at Churchill College, Cambridge (Grattan-Guinness, *The Search for Mathematical Roots, 1870–1940. Logics, Set Theories and the Foundations of Mathematics from Cantor through Russell to Gödel* [Princeton: Princeton U. P., 2000], p. 579).

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matrix in the type theory of Russell's "Mathematical Logic as Based on the Theory of Types" (1908)[3] as "dramatically" (p. 82) exhibiting a difference from the version in *Principia Mathematica*, where a matrix is redefined as a propositional function with no quantifiers (1: 50–2). In a companion on Russell my aim was capture Russell's apparent position *in the 1900s and 1910s*: the treatment of substitution in 1908 covers just one paragraph in art. 4, immediately followed by a declaration that it is "technically inconvenient", while the redefinition of matrix in *Principia* does not deploy substitution at all. So maybe "residue" is a reasonable historical appraisal.

Similarly, Stevens wonders what mathematics Russell might not have been achieved with the substitutional theory, given Landini's revival of it (pp. 81–2). The table in my article shows the range of mathematics that *Principia* was intended to cover: much (but not all) of Cantorian set theory and transfinite arithmetic, and a rather mixed bag from the foundations of real-variable analysis (including a second definition of integers and numbers), some intended for the Volume 4 of *Principia* on geometry that Whitehead was in the end to abandon. Russell himself did not get that far into this mathematical mountain in his substitutional manuscripts, and I am not aware of any more detailed explorations in Landini's or anyone else's writings. Given the considerable technical and philosophical difficulties that attend the exposition in *Principia*, such as the issues surrounding the multiplicative axiom,[4] these claims for substitutional theories need further elaboration.

In recent years I have advocated, as a fundament of historiography, the distinction between two ways of reading past knowledge, each one legitimate but independent of and quite different from each other: history, where the effort is to reconstruct what the historical figures did and did not do, with careful handling and often avoidance of later theories; and heritage, where those later theories can be readily deployed.[5] In the passage cited, Stevens seems to read *Principia* in a heritage spirit inspired by Landini's excellent proposals; but this leads to a "temptation to assimilate Russell's views to more modern ones[, which] has been a cause of many misinterpretations of Russell's logic" (Stevens, p. 89). My article was meant to be historical about Russell (and Whitehead) at the time of their work.

(2). I am glad that Stevens accepts in part my reappraisal of the degree of the

[3] *American Journal of Mathematics*, 30 (1908): 222–62; reprinted in *LK*. To appear in *Papers* 5.

[4] Grattan-Guinness, *Search for Mathematical Roots*, Chap. 7 *passim.*

[5] Grattan-Guinness, "The Mathematics of the Past. Distinguishing Its History from Our Heritage", *Historia Mathematica*, 31 (2004): 161–85.

importance to Russell of Frege's work; in my view it is often grossly overstated while Peano and Cantor are undervalued. Frege's work started to impinge only when the main text of *The Principles of Mathematics* was written, and Stevens says that it led to the Appendix A of that book; in fact, a few passages in the text were also reworked.[6] Stevens quotes the fulsome praise given to Frege in *Principia* (1: viii); but I doubt that Frege's work had penetrated as deeply into Russell's logic as Russell seems to have thought. Had it done so, then various passages in *Principia* and related papers would surely have been less muddy than they are (the accounts of the type theories, for instance).

Take as an example Russell's 1906 paper on the propositional calculus (including propositional quantification and substitution): "the ideas [on that calculus] are more those of Frege" than of Peano, including the reading of implication.[7] Yet Russell's logic is notorious for its (Peanesque) conflations of implication with inference and consequence. Had he absorbed Frege's approach thoroughly, surely he would not have continued to let through sloppinesses such as "Anything implied by a true proposition is true",[8] or to merge axioms and rules of inference as "Primitive propositions". His manuscript notes on Frege show similar inclinations; as is already evident in the annotations transcribed by Linsky,[9] several of Frege's statements are converted into Peanese.

(3). Concerning the conversion of G. E. Moore and then Russell from neo-Hegelianism to realism, Stevens correctly points out (p. 81) that Russell had written on the new position in 1898. My remark on Moore's chronological priority was inspired by Russell's own recollection that "Moore led the way, but I followed closely in his footsteps" (*MPD*, p. 54), which seems to refer to conception as well as to publication (*cf. SLBR*, 1: 182). This remark is quoted by R. L. Cartwright on page 108 of the *Companion*.

(4). Anellis explores the history of truth tables and similar diagrams and tabulations.[10] When I credited Russell and Wittgenstein with creating the truth-table method, I meant very specifically the tabular forms for connectives that have become well known, not tabular or diagrammatic techniques in logics

[6] Grattan-Guinness, *Search for Mathematical Roots*, p. 324.

[7] Russell, "The Theory of Implication", *American Journal of Mathematics*, 28 (1906): 159–202. To appear in *Papers* 5.

[8] "The Theory of Implication", p. 164; *cf. PM*, 1: 13, 94.

[9] Bernard Linsky, "Russell's Marginalia in His Copies of Frege's Works", *Russell*, n.s. 24 (2004): 5–36.

[10] Irving Anellis, "The Genesis of the Truth-Table Method", *Russell*, n.s. 24 (2004): 55–70.

in general, which indeed have a quite a varied history. Neither Peirce's 1880 display of logical functions in quadrants, nor his listing of all connectives in 1902, fulfils quite the same purpose, though they are excellent pioneering contributions that Anellis rightly praises.

But Anellis's advocacy of an historical search for the line of influence from Peirce's fine though unpublicized work to Russell and Wittgenstein (pp. 66–7) seems to me redolent of the "Frege then Russell, therefore Russell because of Frege" non-history hinted at above. Russell "saw [Peirce] only once, so that I hardly know him", he told Louis Couturat in February 1899;[11] if they talked together at all, one can hardly assert that logical diagrams were a topic. The least unlikely possible link is Peirce's student Christine Ladd-Franklin; I saw nothing pertinent in my examination of her *Nachlass* at Columbia University,[12] but I did use only parts of that large and poorly organized collection. Nevertheless, I prefer to think that Russell and Wittgenstein thought up truth tables for themselves in broadly the way that Shosky describes.[13]

[11] A.-F. Schmid, ed., *Bertrand Russell. Correspondance … avec Louis Couturat*, 2 vols. (Paris: Kimé, 2001), 1: 103.

[12] Grattan-Guinness, *Search for Mathematical Roots*, pp. 375, 428.

[13] John Shosky, "Russell's Use of Truth Tables", *Russell*, n.s. 17 (1997): 11–26.