



Discussion

Serious Matters: On Woodger, Positivism, and the Evolutionary Synthesis

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Anyone seriously wishing to document connections between *Woodger, Positivism, and the Evolutionary Synthesis* (the title of the piece in front of me), would begin with the excellent archival record left by Woodger himself and now known as “The Woodger Papers.” They might then proceed to examine the catalogued papers of the “Unity of Science Movement” and its many leaders and followers. Woodger’s influence or lack thereof might be determined first through an informed assessment of his correspondence with Rudolph Carnap, the doyen of logical positivism. It might then be profitable to explore his correspondence with his rival (and philosophical refutationist), Karl Popper or with W. V. Quine (another important and influential philosopher of science). Anyone seriously interested in Woodger’s intellectual relationships with biologists, might wish to consult his correspondence with Ludwig von Bertalanffy (a long neglected, and I wager misunderstood theoretical biologist) and the correspondence of both Haldanes, father and son, John Scott Haldane, and John Burdon Sanderson Haldane. They might then wish to examine the papers of Sir Peter Medawar, biologist-turned-inventive philosopher of biology. They might also wish to consult the papers of Sir Conrad Waddington, the so-called “maverick” of the evolutionary synthesis as well of course, as the excellent record of correspondence (beautifully catalogued) of Sir Julian Huxley, the author of *Evolution: The Modern Synthesis* (1942), who had early ties with the philosopher of science, Bertrand Russell. Given the English backgrounds of many of the protagonists, furthermore, it might prove especially interesting to compare Haldane and Huxley, with an examination of R. A. Fisher, whose papers should also be examined.

Any or all of these primary sources would be helpful *beginning* points to tracing the causal chains of influence that the author of *Woodger, Positivism, and the Evolutionary Synthesis*, seems determined to document.

The very last place to look for influences or discussion of historical connections between Woodger, positivism, and the evolutionary synthesis is my *Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology* (Princeton: Princeton University Press, 1996). It is quite strange indeed to be accused of failing to provide evidence of such connections (as the author of *Woodger, Positivism, and the Evolutionary Synthesis Charges*) when my book explicitly states that I am *not* adopting such historiographic models. My references to Woodger are in fact drawn from the work of another scholar, as I note in the footnotes, the text proper and the acknowledgements. Any insights gleaned from my book on this question would be derivative – an interpretation of an interpretation, in a nightmarish regress of interpretations. Historians who understand their craft make careful distinctions in their uses of primary and secondary sources, and the critical importance attached to these distinctions is reflected in the practice of referencing, in the self-conscious use of footnotes, and the acknowledgment of the work of others. Indeed, we all borrow ideas and draw on the insights of others, and such practices are fully sanctioned within a clear frame of credit and recognition for those whose work facilitated our own. My treatment of Woodger is relatively brief – about 14 pages or so, of 200 – plus pages of text, not only because of my own historiographic choices, but also because the subject has been ably covered already by the cultural historian Pnina Abir-Am. Any conversation on Woodger, really must directly engage her insights; that can only be done first by direct examination of the primary sources, and only then can it be determined whether or not I have put Pnina Abir-Am's insights to good use. It does seem to me, that she herself would make one especially good judge of the latter.

Anyone seriously interested in the historiography (meaning here specifically the scholarly approaches that have been presented) of evolution in the twentieth century, especially the evolutionary synthesis should also examine closely the work of intellectual historians like Ernst Mayr, William B. Provine (and all the contributors to their classic volume on the evolutionary synthesis), but also John C. Greene. If they do so, they will note that there are some interesting points of agreement as well as notable points of disagreement. One beautiful demonstration of the fundamental differences between Mayr and Greene, for example, emerged in a *Revue de synthèse* article written by Greene and commented on by Mayr (Greene 1986; Mayr 1986). Greene has recently revised and reproduced this piece along with accompanying correspondence with Mayr (Greene 1999). What emerges is striking: the dispute

is not so much about the science of evolution, but about their fundamental differences *in assumptions about what history is or can be*. As Greene makes clear in his first essay “I Discover Darwin and Darwinians” – a marvelous work of his own intellectual history – his interest in evolution stems from an underlying commitment to first American and then European intellectual history rather than the concerns of evolutionists. It is because Greene and Mayr approach the writing of history from fundamentally different *starting premises* that they end with a differing interpretation of the history of evolution. I note this not only because it explains the importance of historiographic assumptions, but because Greene’s original *Revue de synthèse* piece – which aroused Mayr’s ire – placed evolution in a positivist philosophical framework. His revised article, now available, only strengthens his original interpretation. (I have some interesting suspicions as to where Provine would fit into this dispute, but that is best left to another forum for expression.)

Historiography is *the* fundamental concern for historians. As students, we learn to distinguish between the different shades of meaning in use of the word, and how sources, methods, approaches, and the amorphous word “theory” (meaning something very different in history than in science or philosophy) become the tools to write our histories. We read book after book – instead of mere article after article – for good reason: it takes a book to lay it all out for the reader; and we are always trained to keep an eye on the historiographic scaffolding that can be either exposed for the reader or concealed for sometimes subtle or dramatic effect. At times there is sharp analysis, logical precision and rigorous argument combined with robust evidence in historical writing that would rival any of that found in a field like philosophy of science, but just as often there is what appears to be close reading, explication or interpretation of texts, or historical detail, or simply beautiful writing that makes dead people and forgotten events come alive. Sometimes history can even chasten us, or move us to political action or force us to see the world in a different way (though these are ambitious goals for the average historian, indeed). What is never taken for granted – at least by a serious historian – are those choices or starting premises that we all must make as we engage the writing of history. Suffice it to say, that historiography is vitally important to what we do and who we are as historians.

Given all this, it would seem to follow logically that any serious examination of a historical work should be made in tandem with its historiographic set of assumptions. It is all the more imperative if the work involves some historiographic revision under some specific theoretical view of history. But I can well understand why non-historians might balk at the professional concerns of historians. And I can well understand why it might seem abstruse, arcane, or just plain boring to non-historians to engage in historiographic

discussion. I hate to listen to stock or economic reports for the same reasons; but then again, I would not choose to make public pronouncements on Alan Greenspan's latest successful attempt to keep the economy running smoothly.

I do feel compelled, however, to comment on the author's peculiar reconstruction of my own intellectual development – a sort of commentary that is quite unusual in a scholarly paper and really quite remarkable. Anyone seriously interested in my own intellectual history (see especially the reference in note 3, but also 1 and 2, and others) might begin by reading the introductory matter in my book. Then it would help to read my book. They should then definitely watch for Volume 14 of *Osiris*, which has a long, heavily documented article that builds on previous work. This one extends the history of the synthesis to the 1959 Darwin Centennial Celebration.

They could also follow my published – and unpublished work – which has circulated widely since 1988, and which the author collected energetically since they were a graduate student. They could check my own correspondence, some of which has already made its way into the archives. Since I am still alive (I ain't quite dead yet, as the purported plague victim says in *Monty Python's Holy Grail*), and since I am not yet quite that famous or inaccessible or intimidating, it might be easiest to just place a phone call or send an e-mail and put some good oral history methods or interview techniques to use. In this way, they could spare themselves public embarrassment if not a serious loss of professional status and credibility by making silly, and virtually irrelevant statements in print. I could then tell them the following: that my interest in the "Unity of Science Movement" began with an interest in the language of the historical actors which I picked up from Cornell University's writing program, and specifically the language of unification and the synthesis, which I happily relayed to all who would listen in the late 1980s.

My introduction to the "Unity of Science Movement" came after a conversation with Gerald Holton in the spring of 1989, who gave me the reference to "Encyclopedia of Unified Science," on the back of a luggage tag as I drove him to the airport. My "claims about Woodger, The Unity of Science Movement" draw not only on the work of Abir-Am, as noted earlier, but also Gerald Holton's important early work on synthesis and analysis as methodological themata, but especially his absolutely beautiful paper which eventually appeared in *Isis* titled "Ernst Mach and the Fortunes of Positivism in America." This paper, which echoed with the voices of the logical positivists of the Vienna Circle, literally moved me to tears.

The author's suggestion that my ideas were "most likely to have roots in a conference on logical positivism and the unity of science" are literally incoherent. The conference was not on logical positivism and the unity of science, but instead on the quite different topic of "disunity and contextualism" and

what they meant for science studies and cultural studies; and what I got from the conference was an especially bad case of bronchitis. I need also note that the conference took place in March 1992 – long after I had been exploring this topic publicly for years – and was comprised of a different set of papers than those that appeared in the published volume that it inspired. It is not historically defensible to see Galison and Stump (1996) for the contents of the conference, the list of invitees, and definitely not my own intellectual development. And that is just one correction to Note 3 of the essay in front of me.

But enough of all this scholarly discourse on intellectual history and historiography. It is all a waste of time and paper and ink; the fact of the matter is, that the author of *Woodger, Positivism, and the Evolutionary Synthesis* is not seriously interested in any of these things. If they were, they would be deeply engaged with the fundamental intellectual questions raised by the topic and exploring all available sources. Instead, they are deeply engaged with a sort of tedious gamesmanship, in which warmed over scraps of stale research are presented and a minor interaction between Woodger and Raymond Pearl becomes crucial evidence of some sort. Historiographic explication is beneath this critic and there is evidently no time to devote to the remaining 200 or so pages of my book or the event it examines. There does seem to be time for, amateur pronouncements and judgments, crude historical readings, and atrocious writing. The exaggerated use of scare quotes, repetitive, offensive metaphors, and general tone of hostility and rage suggest much more is at stake here for the author than the resolution of Woodger's status. I suspect someone else's status is at issue here. I am reminded of the wit and wisdom of Cornell biochemist Efraim Racker – no stranger to the perverse tactics of competitive individuals – as cited by historian and biochemist Joseph Fruton: "Rejoice when other scientists do not believe what you know to be true. It will give you extra time to work on it in peace. When they start claiming that they have discovered it before you, look for a new project."¹

All of this is why no serious historical journal *would* accept *Woodger, Positivism, and the Evolutionary Synthesis* for publication (it was summarily rejected). It found its way (and not by accident) to a forum where it will presumably get as much attention from historical scholars as it deserves. Only from a naive, ill-informed perspective could such a text pass as serious history or serious historical critique. This is about as seriously as I can take it. I do hope that other more serious scholars will return to the subject of Woodger, positivism, and the evolutionary synthesis.

Note

¹ As cited on page 5, of Joseph Fruton (1999).

References

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