

Economics and Evolution: A Reply to Laurence Moss

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references

In [volume 4](#) of this Bulletin, Laurence Moss made a number of positive comments about my book *Economics and Evolution*. He concludes that the book is "worth reading" and for those interested in "the Mecca of economic biology [...] a must read". On the other hand, sections of his extended review were critical and raised some important questions. Without wishing to give the impression of ingratitude, the purpose of this rejoinder is to address these criticisms.

First, I must dispense with some errors of misrepresentation. Contrary to Moss I do not assert that "the economics profession is actively shopping for an escape from the baggage bequeathed by the neoclassical research program" (Moss 1994, p. 35) nor that "scientific progress cannot exist for a thoroughgoing Darwinian evolutionist" nor that "the switch [to a Darwinian version of economics] will merely end the crisis" (*ibidem*). I do not promise "that if economists accept the idea that natural selection occurs on different levels, and that whole societies can be selected for survival, including the entire market system, then the crisis in economics will pass" (p. 48). These assertions are Moss's invention. They are not mine.

Not only are such assertions absent from my book but also I make frequent warnings about the problems with the use of biological metaphors in economics. As I state in my book (Hodgson 1993, p. 24) "biology is not a panacea". Moss chooses to ignore the many passages in my book where I critically address the mechanistic aspects of Darwinism, and atomistic and reductionist variants of modern biology.

Further, contrary to Moss (p. 42n14) I do not suggest that the International Joseph Schumpeter Society is misnamed. Their journal – the *Journal of Evolutionary Economics* – may be so, but only if "evolutionary" is defined, contrary to Schumpeter, on biological lines. What I do assert is that much work using evolutionary selection mechanisms derived from biology – Richard Nelson and Sidney Winter's *Evolutionary Theory of Economic Change* (1982) is the most important – would be better described as "neo-Veblenian" or "neo-Hayekian" or "neo-Marshallian" than the chosen title of "neo-Schumpeterian". The reason – discussed at length in my book – is that Schumpeter (1954, p. 789) explicitly rejected any such appeals to biology.

On the subject of Friedrich Hayek, it is inaccurate to suggest without qualification that I have a "strong dislike" (Moss 1994, p. 47) for that man. For me, Hayek was one of the greatest economists of the twentieth century and his work has been a major source of influence and stimulation. According to all reports he was personally kind and generous, although his brand of politics is definitely not to my taste.

Apart from some blemishes the first dozen pages of Moss's review article are, on the whole, balanced and sometimes complimentary. Then, on page 46, there is a remarkable switch in tone and a shift into an ideological gear. Regrettably, most of it is based on misunderstanding and some of it on misquotation. In his critical mode, Moss makes two substantive charges against the book. The first is

my alleged support for "organicist explanations" and other supposedly associated ideas. The second is my alleged neglect of "later Marshallians". I shall deal with these allegations in turn.

Organicism and Organic Analogies

Moss (1994, p. 46) suggests that I support something called "organicist explanation". Given that this allegation is crucial for what follows I shall make the following points. First, neither the phrase "organicist explanation" nor "organic explanation" nor "organismic explanation" appear anywhere in my book. Second, the suggestion that the word "organicism" – as defined and used in my book – is to do with explanation is a misunderstanding and suggests a confusion on Moss's part between ontology and methodology.

Instead of "organicist explanation" the phrase "organicist ontology" does appear in my book. I consistently use the word "organicism" in an ontological sense. Ontology is about being, not explanation. Explanatory statements are found not in ontology but methodology. Hence if someone were to state that "society consists (only) of individuals" that could be appropriately described as ontological individualism. There is nothing about explanation here. By contrast, methodological individualism is properly associated with statements such as "social phenomena must be explained (only) in terms of their constituent individuals". As typically defined (e.g., Winslow 1989), organicism provides us with an ontology but not a methodology.

Moss's errors do not end there. He goes on to associate my (ontological) use of the word "organicism" with the "organismic approach" described by Karl Pribam (1983, pp. 224-44). To use Moss's words: "Organismic explanations are those in which appeals are made to holistic notions such as national spirits, racial logic, and so on". It seems that Moss is doing no less than attempting to tar me with a fascist brush. In doing so the scholarly tone of the first twelve pages is thrown to the wind. To repeat, neither the phrase "organicist explanation" nor "organic explanation" nor "organismic explanation" appear anywhere in my book. Furthermore, therein there is no appeal to holism, to "national spirits", or "racial logic". No evidence of nationalism or racism will be found. For the record, I have no sympathy whatsoever with either ethnic nationalism or racism. Further, in the book I explicitly reject the use of the word "holism" (Hodgson 1993, p. 269).

Contrary to Moss's allusions, organicist ontologies have nothing specifically to do with fascism, nationalism or racism. An organicist ontology was championed by Aristotle and held sway in Western thought for two millennia until the time of Galileo. After three hundred years of Galilean and Newtonian atomism, in the twentieth century there has been a return to organicism, notably in the work of philosophers such as Alfred Whitehead (1926) and influential physicists such as David Bohm (1980).

*The question of an organic analogy is quite different. Moss goes on to discuss the use of the organic analogy, particularly in some pre-1945 German sociological and economic writings. Strictly, an organic analogy is not a "methodology". In social science an organic analogy is the attribution to social collectives of the features of an organism. This idea is quite different from an organicist ontology. Moss rightly asserts that several pre-1945 German thinkers were fond of organic analogies. An association with fascism is dramatised by Moss with a reference to *Mein Kampf*.*

*I have two major points to note in response to Moss's allusions on organic analogies. First, I am fully aware of this episode in the history of economic thought (see Hutter 1994, for a good summary). However, it is not tackled in my book *Economics and Evolution*. This is because the scope of the latter is confined to the use of the evolutionary analogy in economics: an analogy concerned with process and change and addressing whole populations. German writers such as Knies, Schmoller and Spann are not central to my discourse because their own development of an evolutionary theory of process was highly limited. Further, I had to limit my analytical terrain if the book was not to*

explode in size.

Second, although there are dangers and errors in the organicist analogy, Moss fails to explain why it necessarily and exclusively points to fascism. He simply asserts that:

[...] organicist ideas, when cut loose of their moorings in individual action and calculation, have a tendency to lead their promoters away from political individualism and squarely into the arms of the political ideologues who espouse virulent nationalism and total war as the best method for "selecting out" inferior cultures (Moss 1994, p. 47).

This is an ideological argument and not a serious scientific one. A number of ideas are rejected by Moss purely and simply on the basis of their purported ideological leanings, not on the basis of their scientific, analytic or empirical validity.

Moss overlooks the fact that organic analogies were almost universal in social science in both the German and the Anglo-American world at the end of the nineteenth century. Far from being confined to imperialists, racists and proto-fascists they were also abundant amongst liberals and socialists. Consider two prominent examples of liberal or liberal-socialist writers who extensively used organic analogies. The first is a neglected but important economist and political theorist who had a major influence on both Lenin and Keynes: John Atkinson Hobson (1848-1940). The second also used organic analogies throughout his works and was profoundly influenced by German theorists who also employed the organic analogy, notably by Albert Schäffle. This man was Alfred Marshall. Marshall, however, differs from Hobson and from German writers such as Knies, Roscher, von Lilienfeld and Schäffle, in that he, unlike the others, did not see society or organisations as having wills of their own.

Moss writes: "Hodgson offers no reflections whatsoever on these organicist roots of the positions he is presenting" (ibid., p. 47). In response, I do not write on organic analogies because they are not the focus of Economics and Evolution. Second, the roots of an organicist ontology are Aristotle and not in the organic analogies of some nineteenth-century nationalists. Moss is confusing an organicist ontology – which is a topic in my book with other notions such as "organicist explanation" – with an organic analogy. These quite different ideas are lumped together by Moss. I do not know what is meant by an "organicist explanation" or "methodological organicism". Nevertheless, organic analogies are an important topic and I have written more extensively on them elsewhere (Hodgson, forthcoming).

Moss's criticisms here are not on the basis of theoretical argument but of ideological fear. Fear of a presumed ideological consequence is moulding his views. Such a practice is censurable but alas not rare. There are many extreme examples in totalitarian societies of science being compromised because of the need to conform to ideology – the efforts of both Hitler and Stalin being included. There is also a less extreme but prominent tradition in Western – particularly American – social science. As Dorothy Ross (1991, p. xiii) points out in her major study: "What is so marked about American social science is the degree to which it is modelled on the natural rather than the historical sciences and imbedded in the classical ideology of liberal individualism". In his very important discussion of the separation of biology from social science in the interwar period Carl Degler (1991, p. viii) also shows that liberal-individualistic ideology had a major effect on the development of American social science. Even in America, ideology has had an unwarranted influence on science. It seems that Moss's work is no exception.

The Later Marshallians

Moss criticises Hodgson for neglecting the later Marshallians. In this context he mentions Edith Penrose and Brian Loasby. It is said that Hodgson "erroneously insisted that later Marshallians

*abandoned Marshallian biology for mechanism" (Moss 1994, p. 41n12). Moss also states that I am incorrect when I assert (Hodgson 1993 p. 107) that "later Marshallians neglected the biological aspects of Marshall's thinking, and abandoned any attempt to recast economics along biological or evolutionary lines". To counter these views of mine, Moss asserts that in his book *Equilibrium and Evolution* Loasby is said to continue "Marshallian themes in the context of a modern Darwinian understanding of biology" (p. 33n2). Indeed, Moss asserts: "Loasby's neo-Marshallian construct seems every bit as Darwinian as Hodgson wants these concepts to be" (p. 49).*

There are a number of allegations and assertions here, namely: (a) Hodgson has neglected the later Marshallians, (b) Hodgson is allegedly in error when he asserts that "later Marshallians neglected the biological aspects of Marshall's thinking, and abandoned any attempt to recast economics along biological or evolutionary lines", and (c) Loasby's (Marshallian) analysis is Darwinian and thereby refutes Hodgson's latter assertion. I shall respond to these points in turn.

*In part, to charge (a) I plead guilty. The later Marshallians are not discussed at length in the book. This is because, as explained above, the scope had to be limited to prevent the book from exploding in size. These self-imposed constraints are made explicit in the book in the second chapter. There the "arbitrary cut-off date of 1980" is mentioned. Loasby's *Equilibrium and Evolution* was published in 1991. Even more seriously, there is no extended discussion in my book of recent major works in "economic biology" such as Nelson and Winter's *Evolutionary Theory of Economic Change* (1982). I even make an excuse for failing to discuss extensively Nicholas Georgescu-Roegen's magisterial *Entropy Law and the Economic Process* which was published before 1980. However, all three of these works are mentioned several times in the book.*

Moss fails to mention that Penrose, who could plausibly be described as a "Marshallian", was an acute critic of biological analogies in economics (Penrose 1952). I refer to Penrose in my book, along with another important postwar "Marshallian", Jack Downie. I freely admit, however, that the postwar developments of Loasby, Penrose, Downie, Andrews and others are not discussed extensively. My book could not cover everything.

Turning to charge (b), in the book I state that "later Marshallians neglected the biological aspects of Marshall's thinking, and abandoned any attempt to recast economics along biological or evolutionary lines". However, this quotation is taken out of context. First, Moss ignores the fact that this statement rests in part on the scholarship of others as well as myself, notably Nicolai Foss (1991) and Neil Niman (1991). Second, the context in my book is a discussion of the decline of "economic biology" after Marshall's death in 1924. This reversal is indisputable. As I point out (Hodgson 1993, p. 108), in his famous article on Marshall on the centenary of his birth, Gerald Shove (1942, p. 323) noted "a return to the mechanical as against the biological approach" in mainstream economics. Despite its Marshallian pedigree, the theory of the firm in the postwar microeconomic textbooks showed little trace of biology (Gee 1983; Newman 1960). I go on to state that "it was not until the 1950s that orthodoxy [in which category I would include most Marshallians] rediscovered the evolutionary analogy, but then only in an inferior, half-hearted and equilibrium orientated form" (Hodgson 1993, p. 108).

I stand firmly by all these assertions. I have already noted that Penrose was an important critic of biological analogies in economics. Like Penrose, other important postwar "Marshallians" such as Downie and Andrews made little use of biological analogies in their works (Andrews 1949; 1964; Downie 1955; Penrose 1959). Similar remarks can be made regarding Loasby's (1976) classic work – no significant use is made there of biological metaphors either.

The scarcity of biological metaphors in economics in the 1924-1970 period is hardly surprising. Degler's (1991) book shows how social sciences severed their links with biology around the time of the First World War. Economics is no exception. I have done a bibliographic search and discovered only 7 relevant works in economics from 1914 to 1970 inclusive with "evolution" or "evolutionary" in

their title or subtitle. The number appearing since is well into three-figures. From 1914 to 1970 the very idea of "economic biology" was – with peripheral exceptions – virtually dead.

I now turn to charge (c). The focus here – of which Moss makes great play – that in his 1991 book Loasby is said to continue "Marshallian themes in the context of a modern Darwinian understanding of biology" (p. 33n) and "Loasby's neo-Marshallian construct seems every bit as Darwinian as Hodgson wants these concepts to be" (p. 49).

*Like Moss, I am an admirer of Loasby's work. But I reject the description of either of his 1991 or 1976 books as "Darwinian". Further, despite its title, the 1991 book fails to define "evolution" clearly and makes no sustained appeal to biological metaphors. Therein Loasby (1991, pp. 12-13) does indeed make a brief mention of Charles Darwin. However, there is no recognition of the contention, supported by both Moss (1990; 1994, p. 4) and myself (Hodgson 1993, pp. 100-1), that Marshall leaned much more heavily on the biology of Herbert Spencer rather than on Darwin. Indeed, I would go so far as to suggest that no-one can fully understand or interpret Marshall unless he or she has read key works of Spencer, such as his *First Principles*. Note only did Marshall (1890) himself refer to Spencer more copiously, but also during the crucial 1880-1900 period Spencer was a much more persuasive influence on Western thought than Darwin (Bowler 1983).*

What criteria could reasonably be invoked to evaluate Moss's claim that Loasby's (1991) work is permeated with modern Darwinism? Among biologists there is fortunately a degree of consensus on the identification of the core principles of Darwinian biology. Richard Lewontin (1978, p. 220), for instance, identifies four basic principles of Darwinian natural selection theory:

1. The Principle of Variation. There must be variations among the members of a species or population. Variations may be blind, random, or purposive.

2. The Principle of Heredity. Offspring have to resemble their parents more than they resemble other members of their species.

3. The Principle of Natural Selection. Better-adapted organisms leave increased numbers of offspring either immediately or in the future.

4. The Principle of the Struggle for Existence. The variations or gene combinations that are preserved are those bestowing advantage in struggling to survive.

Similar statements are made by other leading biologists. In some cases the four principles are reduced to three by combining the third and fourth. That is of no matter here. The question is this: are these principles replicated in Loasby's (1976; 1991) work? The answer is "no". Moss makes no case that they are. By comparison with others such as Veblen, Hayek, Nelson and Winter, the Darwinian influence on Loasby is virtually insignificant. I challenge Moss to show otherwise.

Conclusion

*Moss's article on my book *Economics and Evolution* could be an archetypal "mixed review". Praise is mixed with criticism; accurate appraisal is mixed with misrepresentation; penetrating insight is mixed with misunderstanding; accuracy is mixed with error; truth is mixed with falsehood; and scientific detachment is mixed with ideology. I can do no more here but to ask that readers turn to my book and judge for themselves.*

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Winslow E.A. (1989), "Organic Interdependence, Uncertainty and Economic Analysis", *Economic Journal*, 99(4), December, pp. 1173-82.

LAURENCE S. MOSS Charlottesville, Virginia October 1975 Author's Preface The present essay is an attempt to explore with some thoroughness an extremely narrow area within the field of the history of economic thought. Although this area is narrow, it merits a scrutiny quite out of proportion to its extension, relating as it does to fundamental ideas around which the entire corpus of economic thought has revolved for some two centuries. Laurence S. Moss 1944-2009: Academic Iconoclast, Economist, and Magician. Published by: American Journal of Economics and Sociology, Inc. <https://www.jstor.org/stable/i40026651>. The American Journal of Economics and Sociology (AJES) was founded in 1941, with support from the Robert Schalkenbach Foundation, to provide a forum for continuing discussion of issues emphasized by the American political economist, social philosopher, and activist, Henry George (1839-1897). Today, the exciting encounters between sociology and economics remain a natural subject to explore, and AJES continues to publish carefully crafted essays in the social sciences. Coverage: 1941-2015 (Vol. 1, No. 1 - Vol. 74, No. 5). Laurence S. Moss (1944-2009) was an American economist who specialized in history of economic thought and economics of entrepreneurship. He earned his PhD in economics at Columbia University, and later earned a law degree from Suffolk University. Moss was the leading expert on the economics of Mountiford Longfield. His doctoral dissertation was published as *Mountiford Longfield: Ireland's First Professor of Political Economy*. He served as the editor-in-chief of the *American Journal of Economics and Sociology*. "Professor Hollander and Ricardian Economics": A Reply to Professor Moss," *Eastern Economic Journal*, Eastern Economic Association, vol. 8(3), pages 237-241, Jul-Sep. Handle: RePEc:eej:eeconj:v:8:y:1982:i:3:p:237-241. Laurence S. Moss, 2010. "Ricardian Economics: Reasoning About Counterintuitive Tendencies When System Constraints Are Present," *American Journal of Economics and Sociology*, Wiley Blackwell, vol. 69(1), pages 461-498, January. Samuel Hollander, 2010. "Continuing a Conversation with Larry Moss," *American Journal of Economics and Sociology*, Wiley Blackwell, vol. 69(1), pages 51-57, January. More about this item. Statistics.