

Comments on Arturo Hermann's paper, 'The Decline of the "Original Institutional Economics"'

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Keywords: Original Institutional Economics, behaviorism, pragmatism, positivism, W.D. Mitchell, M.A. Copeland, John Dewey, Thorstein B. Veblen

Arturo Hermann is to be commended for his attempt to identify the main reason for the decline in disciplinary power and prestige of Institutional Economics, by which he means the Original Institutional Economics, or OIE, as I will call it in this essay. Hermann notes that there is a strong case to be made that OIE was the dominant approach among economists in the U.S. in the first three plus decades of the 20th century. Why then did OIE decline in importance in the years after WWII? Why did a deductivist, non-evolutionary theory that stands quite apart from the development of other social sciences come to dominance, while a more scientific, evolutionary and interdisciplinary approach to study of the economy was marginalised in most of the academic world?

Hermann surveys the answers that have been given to this question. He quite correctly, in my view, identifies the following as among the most powerful answers: 1) the narrowness of approach into which Clarence Ayres and his followers forced the Veblenian branch of institutionalism; 2) a lack of agreement on 'fundamentals' among OIE practitioners; 3) the success of Keynesian and neoclassical economists in their claim of being more truly scientific than the 'naïve empiricist' Institutional economists.

The change in economic practice and the shift in prestige among the different practices is a complex story, as Hermann recognises, but it is also one that he does not recount quite accurately. In this brief comment I will focus on only one of the explanations for OIE's decline that Hermann discusses: the relationship between theoretical and empirical analysis and the failure of OI economists to articulate a satisfactory (to other economists) reconciliation of the two. On this point Hermann argues that a chief failure of the OI economists was that 'their philosophical background oscillated between pragmatism and positivism, and was never sufficiently clarified'. As Hermann explains it, to the extent that OI economists tended toward 'positivism', they considered statistical evidence to be the only truly scientific aspect of their work and relegated analyses of legislation and court decisions, as well as case studies, to less than truly scientific status. Hermann then argues that 'John Dewey's notion of behaviourism,' which is, he says, 'the pragmatist conception of behaviourism, requires that the "experience" of a person in its entirety' be analysed.

The words 'behaviourism', 'pragmatism', and 'scientific' are very tricky words and especially so when we in the early 21st century try to understand the arguments that engaged our intellectual ancestors in the late-19th and early-20th centuries. There were, in effect, several different discussions going on among pioneering sociologists, philosophers, emerging anthropologists, those would become psychologists and even a few economists, and each group was fighting against previously dominant ideas among their own ancestors. The ancestral groups did not entirely overlap, so that some of the ideas against which each separate group was fighting were of little or no importance to the other groups. And some of

the words for the emerging thought that was held to be superior among all of the pioneering social scientists were the same even though with shades of different meaning depending upon the discussion in which they were used.

This confused and confusing intellectual history has, unfortunately, led Hermann astray. He is simply wrong in saying that a fundamental problem for OI economics in the 1930s and 40s was an unreconciled tension between 'behaviourism' and 'pragmatism'. When Hermann says that this was the case he is arguing that OI economists fell victim to a belief that only the behaviour that could be 'measured' through the use of statistics could be considered 'scientific' and therefore worthwhile for purposes of understanding economic processes, and is thus assuming that an intellectual fight in the discipline of Psychology had equal relevance in Economics. Now, it was indeed the case that the Institutional economists of the 1920s and 30s were enthusiastic about the use of statistics as a way of describing and understanding socioeconomic processes. They did not, however, regard legal and political analyses or case studies as somehow less valuable or legitimate. A quick review of the Institutional literature of the period, as is provided by Malcolm Rutherford in Chapter 2 of his 2011 book, will show this to be the case. Valued OI work took many forms and as Rutherford wrote:

'Being "scientific" meant not being satisfied with speculative armchair theorizing based on unreal premises, including an outmoded psychology... Being scientific did mean being investigative, exposing hypotheses to critical empirical examination, and bring economic thinking into line with recent developments in related disciplines such as psychology, sociology, anthropology, law, and philosophy' (Rutherford, 2011, p. 26).

The work cited by Rutherford as meeting this requirement will be seen to involve a wide variety of analytical approaches.

That OI economists did think and often did write that numbers might be the best tool under many circumstances for understanding the experience of people in the U.S. economy can be illustrated by the work of Morris Copeland on money flows. (See Copeland, 1952; Dawson, 1996, Part II.) Copeland (following the lead, as he said, of J.R. Commons) recognised that transactions that were completed by money flows were basic provisioning actions in our modern economy and he set out to trace those money flows. As he did so he was not unaware of the full range of experiences that accompanied these transactions (Copeland, 1946). But, in an 'accounting economy' dealing with accounts is a good way to understand what is happening. Here is how Copeland put it,

'Because money plays a major role in organizing our economy, it has frequently been characterized as a money economy. In a significant sense it is also an accounting economy. Our system of moneyflows has become so complex that many transactors must keep detailed accounting records. Such records, and financial statements and reports derived from the, today help significantly to organize economic activity '(p. 8).

Copeland was not saying that behaviour must be quantified for analysis to be scientific. He was saying two things that set his work, and that of his mentor, Wesley C. Mitchell, apart from earlier work in Economics. First that the 20th-century U.S. economy was largely organised by money, so money flows were important although largely ignored in earlier economic analysis. And, he was saying that we should use the records that the subjects of enquiry used in order

to understand what they were doing. In stating the importance of his approach, Copeland was arguing with the classical and neoclassical ancestors of his chosen discipline.

So too was Wesley Mitchell arguing with economists when he wrote the passage that Hermann quotes from Mitchell's Presidential Address to the American Economic Association. What Mitchell was arguing was not that quantitative data was required for scientific exploration; he was quite specifically arguing that Alfred Marshall had conceived of 'economic behaviour as controlled by two opposing sets of motives, the motives that impel us toward consumption and the motives that repel us from labour and waiting' (Mitchell, 1925, p. 25.) Mitchell went on to say that he doubted that it would be possible to make more precise statements about these motives, just as he doubted that it was going to be possible to measure 'pleasure' or 'the strength of desire'. He went on to say that use of these terms 'are something that the theorist adds to the data' and that 'In the present state of knowledge of human nature, such interpretations smack more of metaphysics than of science' (1925, p. 25).

I can see why Hermann equates this position with the kind of positivism of mechanistic behaviourism advocated by J.B. Watson, but it is nonetheless a mistake to do so. Watson had rejected the 'functional psychology' that was part of the social science context in which Veblen, Dewey, Mitchell, Copeland and other OI economists were developing their ideas and arguments. (For more on this, see Rucker 1969, Chapter 3 entitled 'Psychology: Functionalism and Behaviorism'.) It may also be tempting to equate what Mitchell wrote in his Presidential address with later 'positivistic' forms of argument about the irrelevance of assumptions so long as predictions are shown to be correct. But that too would be a mistake. Mitchell sought realism in assumptions, in evidence, and in conclusions.

So, contrary to what Hermann argues, my reading of the literature does not lead me to the conclusion that the Institutional economists fell victim to a 'positivist attitude' by considering statistical analysis to be the only truly scientific form of evidence. The debate over the use of 'measurable' observations was of greater significance in some other disciplines and, arguably, in some other forms of economic analysis where the important variables did not emerge in numerical form from the human actions deemed important. Neither Mitchell nor Copeland had to, nor did they want to, 'impute' values; they wanted to use the values recorded by the subjects of their studies.

Not only do I think Hermann wrong in thinking that OIE was weakened by the adoption of a 'positivist attitude,' I also think that he is wrong in saying that this alleged adoption created a conflict with a 'pragmatic conception of behaviourism' in OIE. Behaviourism – as an analytical approach – was, as already noted, important in the development of modern Psychology, but actually had very little importance in the development of OIE. However, Pragmatism was, and continues to be, an important foundation for OIE. What the Pragmatic tradition, as established by C.S. Pierce, W.W. James, J. Dewey and others, means is that individuals and the groups of people who are the basic unit of OI analysis are thought to adapt 'means to ends that cumulatively change as the [life] process goes on' (Veblen, 1906, pp. 74-75). Pragmatism is not, as is often thought, simply a matter of expediency, nor is it in any sense a form of analysis that sets it apart from 'positivism'. Rather, it is a fundamental assumption that the ends or objectives of human action change as the means of achieving them change. This process is cumulative and interactive. The importance of this for OI economists was that it meant there was no one thing – such as maximisation of utility – that could be both taken as an unchanging goal and be given any specific meaning. The acceptance of the Pragmatists' view of humans and their evolution through time and space also meant that individuals took *and changed* their tastes and

preferences as part of an ongoing social process that was shaped by the means of achieving what was desired/needed.

This proposition represented a fundamental challenge to economic thought. The introduction of active human agents who were changed by their environments even as they changed those environments, could not be reconciled with the 'globules of desire' that, in the famous Veblen passage, 'oscillate under the impulse of stimuli that shift about the area, but leave him intact' (Veblen, 1898, p. 389.) The problem was that the 'principles of economics' that Alfred Marshall had so meticulously set forth were among those aspects of 19th-century thought that were hard to test using newly-available statistics and were hard to reconcile with the economy as the OI economists were describing it. Mitchell thought, as he said in his Presidential Address, that economists would change their questions and would simply not try to prove the truth of those principles. But that is not how it went down.

Ragnar Frisch, whom Hermann quotes to illustrate the weakness of the neoclassical rejoinder to Mitchell's Presidential Address, gave the most direct statement about being unwilling to abandon the 'principles' that were inherited and were held to be both true and of greatest importance for economic analysis. The background is this: Henry Schultz, an agricultural economist who – as the US Department of Agriculture began collecting data on prices and purchases of goods such as sugar – realised that the observed price and purchase combinations, if translated into Marshallian terms, were points of intersection between a theoretical and unknown demand curve, with a theoretical and unknown supply curve. In order to construct a demand schedule you had to hold the supply schedule fixed or *vice versa*. The question was how to adjust for the many other things that, as textbooks briefly remind students, must be held constant for the common textbook demand-supply curves to look as they do.

It was at this point that Ragnar Frisch and his co-author Frederick Waugh in effect created modern econometric protocol and set neo-classical economics on its modern and somewhat awkward path. (For much more on this see J. Morgan, 2016 and the essays contained in that volume.) In a manner almost identical to the example that Hermann provides of how the truth is known about the tides, Frisch and Waugh declared that

'An empirically determined relation is "true" if it approximates fairly well a certain well-defined *theoretical* relationship, assumed to represent the nature of the phenomenon studied. There does not seem to be any other way of giving a meaning to the expression "a true relationship"' (Frisch and Waugh, 1933, p. 38).

Marshall's supply and demand schedules were thus declared, that to which statistical evidence had to bend.

The declaration of Frisch and Waugh should be taken as much more than a solution to a technical problem for statisticians. It was also a statement that Marshall's principles should be taken as definitive. By itself this would not have been sufficient to lead to the decline of OIE, an approach that involved a fundamental rejection of Marshallian economics as set forth in his *Principles*. Many other things were involved as well.

My view is that OIE declined, in part, because of the support and the prestige that Frisch and Waugh and their colleagues brought to the new field of econometrics, and to their adoption of Marshall's principles as fundamentally true. I have also written elsewhere about the narrowness of approach adopted by many followers of Ayres as a major reason for the decline of OIE (see Mayhew, 2008). And, as Hermann notes in his final remarks, the failure of OIE to offer deterministic results and its gradualist approach to socioeconomic change have

contributed to that failure. So too has the reformist role that Keynesianism (of an earlier sort) and then Post-Keynesianism have offered to academics eager to help create better economies. The story of the decline, and indeed, the failure of reformist and gradualist social science in the post-WWII era, remains to be fully told. I am pretty sure that when it is told, and told well, that the failure of OIE will not be explained as Hermann has in the essay under review.

Finally, I will note that one of the continuing strengths of OIE, a strength not usually so regarded in the mainstream of economics, is the continuing interdisciplinarity of work done by OI economists. Hermann takes the importance of Psychology to the 1920s OI economists to indicate that that is still the field of greatest interdisciplinary importance. Not so. Having abandoned the notion of a narrowly rational economic man/person, OI economists have moved on and found opportunities to work with legal scholars, sociologists, anthropologists, political scientists and many others. Psychology is where the action was back in the first two or three decades of the 20th century. For those interested in economic issues and policies, that is no longer the case, but OI economists have not, as a result, turned inward. That interdisciplinarity flourishes can be confirmed by looking at the affiliations of those who have published in the leading OIE journal, *The Journal of Economic Issues*, over the last several decades.

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SUGGESTED CITATION:

Mayhew, A. (2018) 'Comments on Arturo Hermann's paper, "The Decline of the 'Original Institutional Economics'"' *Economic Thought*, 7.1, pp. 87-91. <http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-7-1-Mayhew.pdf>