

## DISCUSSION

# Characterizing Institutional and Heterodox Economics—A Reply to Tony Lawson

Geoffrey M. HODGSON

The Business School, University of Hertfordshire, De Havilland Campus, Hatfield, Hertfordshire AL10 9AB, UK. E-mail: g.m.hodgson@herts.ac.uk

### Abstract

Lawson (2005) attempts to distinguish between heterodox and mainstream approaches in terms of their ontological presuppositions. By contrast he claims that different heterodox approaches are characterized not by their fundamental ontological presuppositions but more superficially by different ‘concerns or questions of interest’. First I argue here that this proposed distinction between heterodox and mainstream approaches is unconvincing. Second, I propose that the ontological communality claimed by Lawson for heterodoxy ignores the specific ontological outlook of a ‘Veblenian’ branch of institutional and evolutionary economics that focuses on algorithms and rule systems.

**Keywords:** institutional economics, heterodox economics, ontology, Thorstein Veblen.

### Introduction

In a recent issue of this journal, Tony Lawson (2005) responds to articles by Anne Mayhew (2000) and myself (Hodgson, 2000) that discuss the essential features of the old institutional economics<sup>1</sup>. I agree with much that he has to say. We also share a joint concern to develop a realist account of social and economic phenomena.

The old institutional economics emerged in the 1890s and was very strong in the United States in the first half of the twentieth century. It involved many prominent economists and inspired hundreds of books and articles. However, Lawson focuses rather narrowly on two brief accounts of institutionalism by Mayhew and myself and does not consider fuller descriptions of the nature or core approach of the old institutional economics (Bernstein, 2001; Rutherford, 1994, 2001, 2004; Yunay, 2001). The particular omission of any discussion of my full-length account of the evolutionary principles of the old institutionalism (Hodgson, 2004) is perplexing because it was

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**JEL:** B15, B25, B41, B50, B52.

<sup>1</sup> The author is grateful to Tony Lawson for discussions and comments on a preceding version of this essay.

published in a Routledge book series edited by Lawson himself.<sup>2)</sup>

Lawson's concentration on my 2000 essay is unfortunate because it is a very short article for a conference volume of the *Journal of Economic Issues*. The length of such conference pieces is severely constrained by the editor, and I was unable to elaborate my argument. However, I attempt such an elaboration in my 2004 book, which Lawson ignores.<sup>3)</sup>

I argue in my 2000 essay that endogenous preference formation is a defining characteristic of the old institutionalism. In an attempt to refute this, Lawson points out that Friedrich Hayek also acknowledges that individual purposes and preferences can be moulded by institutional circumstances. I agree with this characterization of Hayek's position.<sup>4)</sup> I also agree with Lawson that many other economists, including Marxists and Keynesians, recognize the possibility of endogenous preferences. In fact, in several works I have gone further, to argue that Alfred Marshall also accepted that individual wants and preferences could be moulded by circumstances (Hodgson, 2001, p. 99).

If Lawson had read further he would have encountered such acknowledgements by myself. Furthermore, these observations do not undermine the argument in my 2000 paper. I never attempted in my 2000 essay to lay out *all* the necessary features of the old institutionalism. The criteria I outlined therein were described as necessary but treated as insufficient to define the old institutionalism. I knew that I did not have the space in that short paper to consider all the sufficient criteria. This is a much more tricky task, for

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<sup>2)</sup> Another perplexing feature is that Lawson attributes the concept of 'downward reconstitutive causation' to me, whereas I always use the term 'reconstitutive downward causation'.

<sup>3)</sup> Contrary to an elliptical allusion in his article, if Lawson (2005, p. 10) had taken note of my book he would have found no attempt by me to define institutions in terms of reconstitutive downward causation, and no claim that any other author makes such an attempt. Instead he would have found that I define institutions therein as 'durable systems of established and embedded social rules that structure social interactions' (Hodgson, 2004, p. 424). I also define institutions in similar terms in Hodgson (2001, pp. 295–296). Lawson ignores this 2001 book as well, despite it also being published in his Routledge series.

<sup>4)</sup> However, a key difference with the old institutionalists is that Hayek (1948, p. 67)—contrary to Marx, Marshall, Keynes and Veblen—did not regard it as the task of 'economics... or any other social science' to explain individual preferences or behaviour. Hayek accepts endogenous preferences but does not see their explanation as within the scope of economics. I quoted Hayek on this in Hodgson (2004, p. 38) and contrasted it to the old institutionalist belief that the explanation of individual dispositions, purposes or preferences was partly within the scope of economics. Lawson ignores this discussion in my book and this major difference concerning the defined scope of economics.

reasons that should become clear below.

### **Can Heterodox and Mainstream Economics be Distinguished in Terms of Ontology?**

Lawson (2005, pp.11–12) repeats his earlier (Lawson, 2003) claim that heterodox economics in general (including the old institutionalism) can be distinguished from mainstream economics only on ontological grounds. In contrast, he argues that distinctions between different heterodox traditions are not primarily ontological but due to different ‘concerns or questions of interest’.

For Lawson, heterodoxy is united in its rejection of ‘mathematical-deductivist methods’. He argues by contrast that mainstream economics presumes that the world is closed, as a basis for adopting these methods. Elsewhere I make it clear that I do not regard this as an adequate criterion to distinguish mainstream from heterodox economics (Hodgson, 1999, 2006). All theorising in science involves abstractions (or isolations) that involve some partial or temporary closure in the theory (Chick and Dow, 2005; Mäki, 1992; Nash, 2004). Heterodox theory in economics is no exception. Consequently, Lawson’s attempt to divide mainstream from heterodox economics fails.

Does his criterion of ‘deductivism’ distinguish adequately between old institutionalist and mainstream approaches? Clearly, Lawson is on to something. Old institutionalists from Veblen to Galbraith have been generally very wary of deductivist and ahistorical methods in economics. A rejection of predominantly *a priori* theorising is a persistent feature of writings in the old institutionalist tradition.

But let us look a little closer. Lawson (1997, pp.16–17) sees ‘deductivism’ as presuming ‘event regularities’ or ‘constant conjunctions of events or states of affairs’ with regularities of the form ‘whenever event x then event y’. Philosophically, this is an atypical definition of deductivism, because it refers to empirical regularities concerning events rather than logical deductions concerning propositions.<sup>5)</sup>

The consequence of this atypical definition of ‘deductivism’ is that most prominent empirically-based methods, including those using econometrics, are also ‘deductivist’ by this definition because they too presume event regularities. Lawson’s (1997, 2003) extensive critique of econometrics confirms this stance. In general, he upholds that

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<sup>5)</sup> Hands (2001, p. 323 n.) points out that Lawson’s use of the term deductivism ‘is different from the way in which the term is generally used within the philosophical literature’. Hands (2001, p. 327) suggests and Wilson (2005) argues that neoclassical economics does not fit Lawson’s characterization of ‘deductivism’.

econometric methods are inappropriate unless system closure exists or is approximated in reality.

However, several prominent Post Keynesians have defended econometric methods (Downward 2000, 2003) and many Marxists have embraced mathematical models. Sraffian economics has for long claimed heterodox credentials. Lawson has (2003) responded that mathematical methods and models are not intrinsically inappropriate, but their legitimate use depends on whether closure obtains (or is approximated) in reality. But the degree of approximation required is so far unspecified in his writings. Consequently, it is not easy to use Lawson's criteria to identify a clear dividing line between mainstream and heterodox economics. Unless he refines his criteria, Lawson is obliged to describe all mathematical economics—including formal work in the evolutionary, institutional, Marxian, Sraffian and Post Keynesian traditions—as mainstream.

### **Can Mainstream and Old Institutional Economics be Distinguished in Terms of Ontology?**

Similar problems emerge with Lawson's particular claim that the old institutional economics can be distinguished from mainstream economics in the manner he describes. Problems result from the fact that the old institutionalism was highly varied in terms of its core philosophical and theoretical assumptions. Within this variety, some elements of the old institutionalism were close to what Lawson specifies as 'mainstream' positions on some points.

For instance, many old institutionalists favoured inductive methods. However, claims to establish inductive generalisations from an inevitably finite body of data would also presume event regularities and amount to 'deductivism' in Lawson's terms. Inductive and positivist methods were upheld by a number of leading old institutionalists, including Wesley Mitchell, Morris Copeland and John Maurice Clark (but not Thorstein Veblen). These methods were also proclaimed in the work of the National Bureau for Economic Research, founded by institutionalists Mitchell and Edwin Gay in 1920.

Following developments in the behavioural sciences as a whole, many old institutionalists took a strongly positivist turn in the 1920s and 1930s, abandoning some of their former Veblenian preconceptions. Leading institutionalists such as Mitchell, Copeland and Clark explicitly embraced positivism. Generally, a positivist spirit infused institutionalism in the interwar years, despite Veblen's earlier warnings on the limits of positivism and empiricism. My 2004 book examines in detail the consequences of this

adverse shift in the fundamental philosophical outlook of the old institutionalism. In practice this shift towards positivism involved presumptions of event regularities, which Lawson's defines as 'deductivism' and sees as characteristic of mainstream economics.

Furthermore, an even greater number of old institutionalists argued that institutionalism was compatible with neoclassical price theory of the Marshallian type. While much of their focus was on long-run technological and institutional changes, they sometimes used Marshallian price theory to analyse short-run developments (Hodgson, 2004, pp. 254–255, 332–333). As a result, Lawson's criteria do not identify a clear dividing line between mainstream economics and the old institutionalism.

Interestingly, there is significant evidence, particularly since the rise of experimental economics to mainstream prominence in the 1990s, that mainstream economics as a whole has become less *a prioristic* and more inductivist and evidence-based (Colander, 2005; Colander *et al.*, 2004a,b; Davis, 2006). Ironically, methodological approaches within modern mainstream economics are becoming closer to the positivist variety of institutionalism that prevailed in the 1920s and 1930s. Given that Lawson's definition of deductivism lumps together both empiricist and *a priori* approaches, it is insensitive to these prominent and important changes within the mainstream and some growing affinities with the positivist tradition within the old institutionalism.<sup>6)</sup>

### **Elements of a Distinctive Ontology for Veblenian Institutionalism**

In my 2004 book I stress that the old institutionalism involved a highly varied set of approaches, and leading tendencies within it differ greatly in terms of fundamental philosophical and psychological propositions. These differences are dramatized in a summary table on page 394 of my book. The approaches of Thorstein Veblen, John R. Commons, Wesley Mitchell, Morris Copeland and Clarence Ayres were very different from one another. While Veblen, for example, embraced instinct-habit psychology and rejected positivism, Ayres took precisely the reverse stance. This contrasts with the commonplace but untenable view that Ayres essentially continued the Veblenian

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<sup>6)</sup> I am not claiming that these current changes in mainstream economics are necessarily beneficial. The new positivist mainstream retains its hostility to both methodology and the history of economic thought, which in my view are essential for the clarification of conceptual underpinnings and the development of economics as a science.

<sup>7)</sup> I share Lawson's (2005) critical concerns about the so-called 'Veblenian dichotomy'. He does not refer to the long section in Hodgson (2004, pp. 365–375) where I argue in detail that Veblen never upheld a general dichotomy between institutions and technology and the source of this dichotomy is elsewhere.

tradition.<sup>7)</sup>

Indeed, it was the rise of positivism, the decline of instinct-habit psychology, and the rise of behaviourism in the 1910–1930 period that brought the old institutionalism to a crises concerning its very identity, meaning and fundamental presuppositions. As I elaborate in my book, this crisis of identity and conceptual foundations became apparent in two meetings on institutionalism held under the auspices of the American Economic Association in 1931 and 1932 (Hodgson, 2004, pp.277–279). This severe crisis prevented Commons and others from formulating adequate philosophical underpinnings for institutionalism. Consequently, institutionalism failed to provide a general and consensual statement of its overall approach. These deficiencies played a part in its subsequent decline.

In retrospect, however, Veblen’s position is strikingly modern in the context of recent developments in philosophy, psychology, sociology, anthropology and economics. Veblen was influenced by pragmatist philosophy. After being eclipsed for much of the twentieth century, pragmatism has re-emerged to become ‘if not the most influential, at least one of the fastest growing philosophical frameworks on the intellectual landscape’ (Hands, 2001, p. 214). In psychology, after the hegemony of behaviourism from the 1920s to the 1960s, evolutionary approaches inspired by William James and others are now enjoying a renaissance in psychology (Degler, 1991; Plotkin, 1994, 1997). The key Veblenian concept of habit—defined in terms of acquired dispositions—has also re-emerged in modern psychology (Ouellette and Wood, 1998; Wood *et al.*, 2002).

I argue in my 2004 volume that the position taken by Veblen himself is much more viable today, as long as suitable amendments are made, including the replacement of Veblen’s Kantian philosophy by a more suitable form of realism. To this end (Hodgson, 2004, pp. 246–247) I identify six negative propositions basic to Veblen’s institutionalism (including rejections of positivism, methodological individualism and methodological collectivism, suitably defined). I see Veblen as endorsing thirteen further basic positive propositions (including the principle of universal causation, the principle of evolutionary explanation, and the idea of reconstitutive downward causation). But together these nineteen propositions are not enough. Because key additional elements ‘were unclear, underdeveloped or absent in Veblen’s works’ I add a further eight propositions (including a layered ontology and an explicit focus on emergent properties) that are ‘vital for a Veblenian theoretical system rebuilt on emergentist foundations’ (Hodgson, 2004, p. 247).

In these twenty-seven propositions I sketch part of the philosophical foundations of

Veblenian institutionalism. The majority of these propositions are ontological and concern the nature of social being. They involve ontological stances concerning causality, evolution, Darwinian ‘population thinking’, a layered ontology, and much else. Lawson ignores all this.

Consider just one proposition among the twenty-seven. I refer to what is widely known as the principle of determinacy, meaning that every event has a cause, or everything is determined in accordance with laws by something else (Hodgson, 2004, pp. 37, 58–62, 65, 88–90 etc.). I argue that Darwin and Veblen share this ontological principle, despite its rejection by both William James and Charles Sanders Peirce. Many mainstream economists also embrace it. However, it is not adopted by some Austrian-inspired heterodox economics, such as George Shackle (1976), who instead embrace the notion of an ‘uncaused cause’. Hence this highly important ontological principle divides some heterodox approaches and fails to separate heterodox from mainstream economists.

This principle also divides old institutionalists. Veblen insisted that human intentions and intentionality are causally determined, even if such causes are unknown or unknowable. Veblen (1909, p. 625) wrote:

The modern scheme of knowledge, on the whole, rests, for its definitive ground, on the relation of cause and effect; the relation of sufficient reason [or intention] being admitted only provisionally and as a proximate factor in that analysis, always with the unambiguous reservation that the analysis must ultimately come to rest in terms of cause and effect.

While Commons (1934, p. 739) also rejected a ‘capricious and undetermined’ human will, in contrast he saw this principle as irrelevant to economics, as he explicitly abandoned any discussion of the determination of human volition (Commons, 1924, p. 82). Furthermore Frank Knight, who repeatedly described himself as an institutionalist, rejected the principle of determinacy.

Veblenian institutionalism, in both its original and revived forms, can lay claim to a particular ontology. This ontology is in contrast to many other approaches found in heterodox or mainstream economics. For example, Veblen’s views concerning the instinctive foundation of intelligence contrast with prevalent views among heterodox and orthodox social scientists that regard instinct and intelligence as being in opposition. Furthermore, Veblen’s insistence that assumptions concerning human rational capacities have to be consistent with evolutionary explanations in Darwinian terms is also absent from much heterodox and mainstream thinking. I do not argue that Veblen himself

worked out an adequate social ontology. Far from it. But his general evolutionary and Darwinian stance points to a potentially fertile ontological grounding for future theoretical work.

In part, this revived Veblenian approach focuses on the algorithmic and incremental nature of decision-making. Its orientation is towards process rather than equilibrium, as Lawson (2005, p. 16) himself emphasizes. Both social reality and individual capabilities are conceived in terms of various systems of processual rules. Following Veblen, these rule systems are placed within a generalized Darwinian framework where the principles of variation, retention and selection are used to help explain their evolution.<sup>8)</sup>

These and similar elements elsewhere coincide with a possible *gestalt* shift in the social sciences, away from conceptions of incremental change and equilibria, where everything potentially impinges on everything else, to a notion of limited interconnectedness within social systems essentially composed of structures and algorithmic processes of rules (Potts, 2000; Hodgson, 2006). More particularly, due to recent developments within the new institutionalism as well as the old, what emerges as ‘institutional economics’ in the next few decades may turn out to be very different from what was prominent in the 1980s and 1990s, and it may trace its genealogy from the old as well as the new institutionalism. Consequently, attempts to draw a sharp line between mainstream and heterodox approaches may be counterproductive as well as being unconvincing.<sup>9)</sup>

## **Conclusion**

In this response I have regretted Lawson’s (2005) failure to engage with my recent two books on the history and nature of institutionalism. Had he done so, it might have been possible to advance the argument further. Lawson (2005) repeats his earlier arguments that attempt to (a) distinguish between heterodox and mainstream approaches in terms of their ontological presuppositions, and (b) claim that different heterodox approaches are characterized not by their ontological presuppositions but by different ‘concerns or questions of interest’.

I argue here that the attempted distinction in (a) is inadequate and unconvincing

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<sup>8)</sup> This Veblenian approach is illustrated in the simulation and discussion in Hodgson and Knudsen (2004). See Potts (2000), Dopfer (2004), Dopfer *et al.* (2004), Parra (2005) and Arthur (2006) for related discussions of rules, knowledge and algorithms.

<sup>9)</sup> See Dequech (2002) and Hodgson (2002, 2004) on the convergence of some elements of the ‘new institutional economics’ with older traditions of institutionalism.

because it overlooks the fact that all theorising — heterodox or mainstream — inevitably involves some form of theoretical closure. It is thus impossible to make a clear distinction on the basis that Lawson suggests.

Furthermore, the ontological conflation in (b) ignores huge diversity of fundamental assumptions both within heterodox economics and the old institutionalism. It also overlooks the specific ontological focus on algorithms and rule systems that characterizes an institutional and evolutionary approach under contemporary revival. This modern approach has strong Veblenian roots.

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