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Michel Foucault's archaeology of knowledge and economic discourse

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Abstract: The literature in economic methodology has witnessed an increase in the number of studies which, drawing upon the postmodern turn in social sciences, pay serious attention to the non-epistemological-discursive elements of economic theorizing. This recent work on the "economic discourse" has thus added a new dimension to economic methodology by analyzing various discursive aspects of the construction of scientific meanings in economics. Taking a similar stance, this paper explores Michel Foucault's archaeological analysis of scientific discourses. It aims to show that his archaeological reading of the history of economic thought provides an articulate non-epistemological framework for the analysis of the discursive elements in the history of economics and contemporary economic theorizing.

Keywords: Michel Foucault, economic discourse, archaeology of knowledge, epistemology, postmodernism

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There seems to be a growing number of economists today who, against the dominance of the mainstream paradigm, make the case for pluralism in economics and show an awareness of different theoretical approaches in the discipline. This awareness, in the form of a philosophical self-reflection, has led in recent decades to a flourishing economic methodology literature. Methodologists of economic science have employed, for instance, criteria such as verification and falsification to assess the scientific status of various economic theories.[1] Others have taken a descriptive approach and used the Kuhnian notion

[1] For a defense of the use of falsification to assess economic theories see Blaug 1992; for a critique of the criteria of scientificity see McCloskey 1985.

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of "paradigm" and the Lakatosian framework of "scientific research program" to analyze and reveal norms of behavior, modes of theorizing, ways of formulating assumptions, and so on, which define, shape and characterize different schools of thought in economics.[2] The recent interest in ontology, moreover, has raised questions concerning the very nature of economic reality, such as: 'Are there any "real" economic forces or mechanisms at work beneath the surface of the appearances that empirical studies confine themselves to?'[3] This whole literature, outlined in dotted lines, has played a major role in keeping the critical stance in economics alive.

A recent development, which bears a close affinity to the main theme of this article, has further brought some other philosophical concerns and issues to the attention of historians and methodologists of economics. Drawing upon the theoretical and philosophical framework developed in poststructuralist theory, cultural studies, literary criticism, feminist theory, and so forth, economists such as Jack Amariglio, Antonio Callari, Stephen Cullenberg, Arjo Klamer, Deirdre McCloskey, David Ruccio have emphasized the role of literary and rhetorical practices in the production of scientific meanings in economics.[4] Consequently, the various linguistic devices economists use to produce and disseminate economic theories—the textual character of our knowledge of the economy—have become a locus of analysis. This literature has thus moved attention away from epistemological norms toward non-epistemological-discursive unities in the practice of economic science.

This emphasis on the non-epistemological-discursive elements of economic theorizing opens up a new field for research in economic methodology. This article aims to make a contribution to this new field by bringing Michel Foucault and his theory of discourse (or discursive formation as he also calls it) into the picture. Foucault uses the term discourse in a particular way, although one cannot find an explicit definition of it in Foucault's work. He rather lets the term develop in his concrete case-study-like analyses of the "rules and regularities" in different disciplines that confer to a given body of knowledge the status

[2] For a discussion of scientific research programs in economics see, for example, De Marchi and Blaug 1991.
[3] Recent years have witnessed an increasing number of studies devoted to economic ontology. See Lawson 1997; 2003; and Mäki 2001.
[4] See McCloskey 1985; Amariglio 1988; Samuels 1990; Callari 1996; Cullenberg, et al. 2001; Amariglio and Ruccio 2003; Klamer 2007.

of scientificity, i.e., the privileged position of being “the” scientific analysis of reality, in a historical time period. These rules and regularities constitute for Foucault a non-epistemological unity at the ‘archaeological’ level of knowledge, in the sense of imposing historical limits upon what we can say, write, or think about any given object of scientific analysis in a particular historical era. It is the task of the archaeologist of knowledge to unearth these historical discursive rules and thus the whole matrix of relations within which they define and constitute the unity of a discursive formation. Furthermore, it is within this network of discursive rules, concerning the construction of objects of analysis, the formulation of concepts, the articulation of theoretical structures, and the like, that the conditions of the truth/falsehood dichotomy are determined (Foucault 1972). Claims to true and scientific knowledge of reality, therefore, which take on in epistemology a universal and non-historical character, appear in archaeology to have historical and contingent discursive elements.[5]

Within this general framework the article sets itself two main objectives. First, it analyzes and compares—in the first section—the epistemological and archaeological approaches to the problem of knowledge in order to argue that Foucault's archaeology offers a substantially different way to think about the problem, even if epistemology is defined as *the* theory of knowledge in the classical taxonomy of philosophy. While Foucault does not explicitly target epistemology, his archaeology involves, I maintain, a substantial implicit critique of the epistemological approach to the problem of knowledge.

[5] Of three major themes in Foucault's work throughout his career (archaeology in the 1960s, genealogy in the 1970s, and technologies of the self in the 1980s), this article is confined to the first period where he develops his theory of discourse. This obviously does not mean that Foucault's later studies do not bear upon economics. In fact, Amariglio, in one of the very few pieces on Foucault in the economics literature, offers a general introduction to Foucault for economists, drawing upon both his early and later studies (Amariglio 1988). A recent article by Steiner, moreover, uses Foucault's lecture courses at the Collège de France during 1978 and 1979—roughly the period of transition from genealogy to the technologies of the self—to discuss Foucault's analysis of the birth of political economy, the rise of 18th century liberalism and neo-liberalism (Steiner 2008). However, whereas Amariglio's essay is mainly centered around Foucault's genealogical analysis of body and power, and Steiner refers to such concepts as governmentality and biopolitics that Foucault developed in his later studies in the 1970s and 1980s, this article approaches Foucault explicitly from the perspective of the theory of knowledge. As Foucault himself remarks (Foucault 1980), his early work on the archaeology of knowledge constitutes the basis for much that he did in his later studies. A close scrutiny of the implications of Foucault's archaeology for economics should therefore add an important dimension to Foucault's relevance for the study of economics.

The other objective is to present—in the second section—Foucault's own archaeological reading of the history of economics and to scrutinize his contribution to a non-epistemological theoretical space for historical and methodological analysis. In the last section I conclude with some remarks concerning the Foucault-postmodernism-economics nexus.

THE PROBLEM OF KNOWLEDGE: EPISTEMOLOGY VS. ARCHAEOLOGY

The problem of knowledge, which can briefly be formulated as "How do we know what we know?", arises, as all other inquiries concerning human understanding, when the human mind turns back upon itself and reflects on its own operations. The genesis of the problem, however, does not necessarily prescribe in itself the method for its inquiry. In other words, locating its origin in the reflexivity of the human mind on its own operations does not require that the problem of knowledge be analyzed within a framework that takes the human mind as one of its operative variables. This is the path taken by that subfield in philosophy known as epistemology, a path whose markers are set in accordance with a certain understanding of the problem of knowledge. The problem is posed there as a non-historical and universal correspondence relation between the epistemic subject, based on the Cartesian *cogito*, and objective reality, which exists out there independently of the ways of knowing it. Epistemology, therefore, is based on a fundamental ontological divide between the subject and the object of knowledge, where each exists independently of the other. The main problem for epistemology consists then in finding ways to close this ontological gap between the subject and the object so as to allow us to proclaim that we have acquired true knowledge of things.

Beginning from the 17th and up until the early 20th century, i.e., until the time when the philosophy of language and logic appeared as the dominant paradigms in Western philosophy, the problem of knowledge was analyzed within two great traditions of epistemology: rationalism and empiricism. The Cartesian *cogito*, which Descartes set up in his *A discourse on method* (1934) and *Meditations on first philosophy* (1996), defined the fundamental problem with which not only rationalism but epistemology in general would grapple with for the centuries to come. Descartes's main concern in his philosophical investigations was the 'quest for certitude'; his method was to reject everything as false about which he could have the slightest doubt. Descartes finds this certitude in "the Self", the entity existing behind all doubt, because the act of doubting is self-referential and requires the existence of a thinker (Descartes 1934). In constructing his *cogito*, Descartes was not only giving an answer to the epistemological problem; he was also defining the very problem itself. The Cartesian *cogito*, in other words, laid down the terrain for epistemology within which both rationalism and empiricism, the latter even in its rejection of the rationalist solution, would seek their own solutions.

In order to make this argument more concrete we can look at the empiricist tradition. In his *Essay concerning human understanding* Locke, just as Descartes, looks upon the problem of knowledge as constituted by an abstract epistemological subject:

> *Every man* being conscious to himself that he *thinks*, and that which his mind is applied to about whilst thinking being the ideas that are there, it is past doubt that men have in their minds several ideas [...]. Whence has it [the mind] all the materials of reason and knowledge? To this I answer, in one word, from experience (Locke 1956, 19, emphasis added).

For Locke, knowledge can have no other source than experience. He rejects any account of knowledge which makes recourse to innate ideas or concepts that the human mind possesses of its own nature. But, in the midst of these differences, or rather negations, we encounter a fundamental similarity between rationalism and empiricism: the epistemological problem itself. What brings Descartes and Locke together is not that they both dealt with inquiries concerning human knowledge, but that they both conducted philosophical investigations within the same problematic issues, using as it were the same language, however much they may have differed in the answer they gave. Even Kant, with his synthesis as outlined in his *Critique of pure reason*, belongs to these problematic issues of classical epistemology. When he said "But though all our knowledge begins with experience, it does not follow that it arises out of experience" (Kant 1965, 41), he was attempting to put in their proper places the a priori and a posteriori elements of human knowledge within the main problematic issues of epistemology.

Foucault's archaeology

The philosophical framework adhered to by rationalism and empiricism, that which constitutes their common locus, characterizes classical epistemology in its understanding of the problem of knowledge. Once we emancipate our mode of thinking from this particular problematic issue—once we allow ourselves to see the problem of knowledge not as concerned with prescribing universal criteria to attain the true knowledge of things, but as revealing the regularities, rules, and practices which make scientificity itself possible in a particular discipline and at a particular time period—a different problematic set of issues reveals itself. At this *archaeological* level (Foucault 1972; 1988; 1994a; 1994b), as opposed to the *epistemological* one, the problem is not to prescribe how scientific analysis can reach the truth, but to understand how a particular discourse acquires the status of scientificity, how it creates in itself, so to speak, the conditions of what counts as truth. In *The order of things*, Foucault writes:

> I tried to explore scientific discourse not from the point of view of the individuals who are speaking, nor from the point of view of the formal structures of what they are saying, but from the point of view of the rules that come into play in the very existence of such discourse: what conditions did Linnaeus (or Petty, or Arnauld) have to fulfill, not to make his discourse coherent and true in general, but to give it, at the time when it was written and accepted, value and practical application as scientific discourse [...]? (Foucault 1994b, xiv).

Knowing things, therefore, cannot be pictured for Foucault as a neutral and innocent practice of the intellect, whose only concern is to get to the truth about reality. Scientific discourse is part of a broader social whole within which it finds, and if necessary creates, its own conditions of existence; that is, within which it is labeled as scientific. Hence the analysis at the archaeological level of knowledge of the rules and regularities which scientists of a particular historical period follow—perhaps unconsciously—when they define their objects, form their concepts, and build their theories to acquire the 'scientific' label:

> [In the classical period] unknown to themselves, the naturalists, economists, and grammarians employed the same rules to define the objects proper to their own study, to form their concepts, to build their theories. It is these rules of formation, which were never formulated in their own right, but are to be found only in widely differing theories, concepts, and objects of study, that I have tried to reveal, by isolating, as their specific locus, a level that I have called, somewhat arbitrarily perhaps, archaeological (Foucault 1994b, xi).

Thus, in Foucault's archaeological analysis, the main problem concerns the interrogation of those elements which allow scientific discourses to create their objects and to formulate their theories, but which also *constrain* them in their scientific investigations.[6] These "historical a priori" elements impose limits in the sense that they involve certain rules and regularities which confer to a body of knowledge the status of scientificity in a particular historical period. Moreover, in its historical development a discipline adheres to different rules of scientificity; the study of these transitions occupies a prominent place in Foucault's research. Foucault conceives of these changes not as a continuous progress in the development of scientific truth in which we get ever closer to the true knowledge of things, but as breaks, ruptures, or transformations at the archaeological level.

In the next section, I shall analyze Foucault's archaeological reading of the history of economic thought and discuss how he links those transformations at the archaeological level of knowledge to different discursive constructions of the economy as the object of economic science. But before, I would like to spend some time on Foucault's historical analysis of the construction of "madness" and "illness" as objects of medical and mental sciences, respectively. This will pave the way for the discussion on the construction of the economy in the history of economic discourse, because Foucault follows similar lines in his archaeological reading of the history of these different disciplines. In his *Madness and civilization*, to start with, Foucault traces the changes of the way the Western culture has understood "madness" and made it a discursive object of "scientific" investigation. From the perception of mad people as having peculiar relations with divinity and being part of daily life in the Renaissance, to that which put them to houses of confinement together with the criminal and the unemployed, with all the "idle" elements of the early capitalist society that constitute its other (Foucault 1988).

[6] Foucault's analysis of the historical a priori elements of scientific discourses bears a relation to Kant's main analysis in his *Critique of pure reason* of the conditions of possibility of human knowledge. For Kant, the a priori elements of reason have a dual character. They allow human minds to achieve the knowledge of things, i.e., they render knowledge *possible*; but at the same time they set the *limits* to our knowledge of things in the sense that things can only be known within the dimensions of time and space, and through a priori concepts of understanding (causality, unity, plurality, and so forth). Kant, in other words, analyzes the conditions of possibility of knowledge in terms of their positivity and negativity: what makes knowledge possible imposes at the same time its limits upon what and how we can know. This epistemological problem takes on a historical and discursive, i.e., archaeological, character in Foucault.

The discursive conception of madness further changed, Foucault explains, in the 19th century when madness constituted itself as the object of modern psychiatry and the mad person was defined as someone who was sick, and who should therefore be separated from other idle elements and subjected to medical treatment in the asylum. To the modern mind, this constitution of madness as an illness is nothing but the recognition of an objective reality which will eventually mitigate the sufferings of the mad through appropriate treatment in the asylum (Gutting 1989). For Foucault, however, the dissolution of the confinement system and the beginning of the asylum life for the mad was based upon the imperative of social control and manipulation of those who did not conform to morals and economic practices of modern bourgeois society.

In *The birth of clinic*, he explains, in a similar fashion, the transformations that occurred in the perception of illness at the turn of modernity. From having an ideal existence separate from the sick person's body, illness in the 19th century acquired a locality in the human body, making the modern clinical discourse possible as a new discursive formation about illness. This transformation in "medical gaze", which for Foucault was not an epistemological event, created the conditions of possibility for a new sensibility (the modern clinical discourse), and established a new relation between the patient and the doctor. In the 18th century it was believed that the sick should be treated at home, where the patient would be in "the natural environment of social life, the family" (Foucault 1994a, 39). This would allow the doctor to capture the nature of illness more easily; whereas in the hospital where different illnesses would intermingle with each other, the nature of the illness would change through this interaction, making treatment more difficult. All this changed, according to Foucault's archaeological analysis, with the transformation in the "medical gaze". Illness, as the object of modern medical science, was stripped of its ideal existence independent of the body and located in particular organs, tissues, and the like. This development gave rise to the establishment of modern clinical practice in which illness is treated at the hospital at its specific locality in the human body.

In his archaeological analysis of psychiatric and medical discourse, Foucault shows that the knowledge relation which the human mind establishes with reality is mediated through historical and discursive elements. His purpose, it should be emphasized, is not to evaluate the epistemological status of these disciplines; he does not, in other words, explicitly question whether what these disciplines say about their object of analysis is objective, true or scientific according to a universal benchmark of epistemology. He is rather concerned with understanding upon what historical and discursive a priori structures conditions of scientificity arise; i.e., within what network of discursive elements, however epistemologically authorized and justified, reality becomes the object of scientific analysis, concepts become part of a scientific nomenclature and theories become formulated and accepted as the scientific cast for the truth. It is within such set of problematic issues that the analysis of the discursive constitution of madness and illness acquires its significance. For, according to Foucault, the historical a priori structures of modern Western thought, while making modern psychiatry and medical science possible, allow only a particular conception of madness and illness as objects of "scientific" analysis.

But where exactly does the Foucauldian project of archaeology stand in relation to epistemology, especially when one considers that Foucault is rather reluctant to counterpose the two? Foucault's lack of lucidity in this regard makes it difficult to come up with a clear-cut answer; but at the same time, this ambiguity creates a space to further elaborate upon the problem through commentary and analysis. The tension between archaeology and epistemology can be best explored I suggest, along three different lines.

First, as argued above, epistemology's understanding of the problem of knowledge is predicated upon an ontological dichotomy between the subject and object of knowledge. Foucault, however, does not pose the problem of knowledge in reference to or from the perspective of an abstract epistemological subject. He is rather interested in understanding the discursive rules of scientificity that the practitioners of science unconsciously adhere to in different historical time periods. And, since these rules impose limits as to how the objects of scientific analysis are constituted, that is, discursively "constructed", the existence of an ontological gap between the epistemological subject and objective reality is seriously called into question by Foucault.

Second, whereas the disinterested search for the transcendental truth of the objective world is a constituent component of the epistemological framework, for Foucault there are only different "truth claims" which are historically situated and which find their justification and authorization (regarding the status of their scientificity) within the network of discursive rules. The idea of scientific progress where we get closer and closer to the true knowledge of objective reality is displaced, therefore, by the discourse-specificity of our knowledge of things.

Third, Foucault's archaeology allows him to introduce the concept of power into the problem of knowledge, which does not and cannot arise within the main problematic issues of epistemology. For Foucault, in other words, the operation of power in society—for example the social control of those who do not conform to the practices and values of bourgeois society, as mentioned above—is an integral element of claims to knowledge and of the historical production of truth.

Taken together this suggests that Foucault's archaeology entails a major critique of the underpinnings of epistemology. True, Foucault never problematizes his archaeology in its relation to and tension with epistemology. However, his account of the history of such disciplines as psychiatry and medicine, and economics as I shall try to explicate in the next section, demonstrates that there is much in the problem of knowledge and the actual practices of science that the epistemological framework fails to capture.[7]

FOUCAULT AND THE ECONOMIC DISCOURSE

In his *The order of things*, Foucault for the first time takes up economics as an explicit object of his archaeological analysis to point to the discursive elements at work in the construction of the economy as an object of scientific analysis in the history of Western thought. There, Foucault defines three different historical periods (*epistemes*) at the archaeological level of Western knowledge, with two breaks between

[7] In relation to this, I would like to add that Foucault's archaeology also entails a critique of the prescriptive frameworks of the philosophy of science (the principles of verification, Popperian falsification, and so on) as they derive directly from the same understanding of the problem of knowledge as in epistemology. The relation between archaeology and the descriptive frameworks of the philosophy of science (Lakatosian research program and Kuhnian paradigm) is, however, more complicated. Piaget (1970) argues, for example, that there are essential similarities between Foucault's archaeological analysis and Kuhn's notion of paradigm. It is beyond the scope of this paper to delve into this debate, but allow me to state very briefly that I see both important similarities, as well as differences between these two frameworks. Just like Kuhn, Foucault maintains that scientific practice includes elements that go beyond epistemologically-authorized norms of scientificity. However, it seems to me that Foucault is rather interested in understanding the assumptions and regularities at the "unconscious" of scientific practice than in the paradigm shifts that result from deliberate and conscious reaction to the accumulation of certain theoretical problems. This allows him, for example, to explicitly problematize how and with respect to what discursive rules reality is constructed as the object of scientific analysis, a problem that does not arise in the descriptive branch of the philosophy of science.

them: the first break in the 17th century between the Renaissance and the classical periods; the other at the beginning of the 19th century between the classical and modern periods.

In the Renaissance *episteme*, Foucault argues, "resemblance played a constructivist role in the knowledge of Western culture" (Foucault 1994b, 17). In Foucault's terminology, resemblance had a 'positivity' in making the knowledge of things possible, meaning that it was the defining archaeological principle that constituted the possibility of human knowledge. Knowing things in the Renaissance *episteme* consisted therefore in deciphering the signs imprinted into things which indicated the system of resemblance between them.

> There exists a sympathy between aconite and our eyes. This unexpected affinity would remain in obscurity if there were not some signature on the plant [its seeds], some word, as it were, telling us that it is good for diseases of the eye. [...] [The seeds] are tiny dark globes set in white skinlike coverings whose appearance is much like that of eyelids covering an eye (Foucault 1994b, 27).

The knowledge that aconite could be used to cure eye diseases was based upon the sympathy, as a form of affinity, between the plant and the eyes. This sympathy could be known because of another form of resemblance as its sign, whose explanatory power was justified within the discursive structure of the Renaissance *episteme* itself: the resemblance between eyes and the seeds of the plant.

There were no boundaries to the play of signs and resemblances in making the world, or rather the order of things, intelligible to us in the Renaissance. Resemblance might be found, for instance, in the principle of mobility (in the explanation of why things move at all): "[resemblance] attracts what is heavy to the heaviness of the world" or it makes "the great yellow disk of the sunflower turn to follow the curving path of the sun" (Foucault 1994b, 23). As far as economic discourse is concerned, the value of money and its role as the medium of exchange was based upon the intrinsic preciousness of the metal used. Money had a price and could function as the measure of all other prices because the monetary substance was of itself precious; and in its brightness the metal carried the sign of its own preciousness and worth. In the economic discourse in the Renaissance, "[f]ine metal was, of itself, a mark of wealth; its buried brightness was sufficient indication that it was at the same time a hidden presence and a visible signature of all the wealth of the world" (Foucault 1994b, 174).

Foucault identifies a rupture, or discontinuity, in the archaeological structure of Western knowledge at some time during the 17th century, when resemblance as the organizing principle of knowledge gave way to the "representation" of identities and differences on a table of classification. Consequently, the order of things for the classical *episteme* meant a taxonomy where things had their proper places in accordance not with their inherent signs, but with a representation of their identities and differences. These identities and differences, i.e., the presence or absence of common elements, also allowed the arrangement of things in a progressive manner from the simplest to the complex.

> [T]he Classical *episteme* can be defined in its most general arrangement in terms of the articulated system of a *mathesis*, a *taxinomia*, and a *genetic analysis*. The sciences always carry within themselves the project [...] of an exhaustive ordering of the world; they are always directed, too, towards the discovery of simple elements and their progressive combination [...] (Foucault 1994b, 74, emphasis in the original).

This ordering, however, need not be quantitative. Foucault disagrees with the traditional account of the classical period as engaged in the mathematization of nature. The classical *episteme* was rather based on a mathesis, a general order of things which involved both quantitative and qualitative elements. The fundamental principle was not mathematization, but an ordering of things on a non-historical table through the representation of their commonalities and dissimilarities.[8]

Having defined the basic framework of the classical *episteme*, Foucault investigates three disciplines of human sciences in the classical period: general grammar, natural history, and analysis of wealth, the predecessors of philology, biology, and political economy, respectively. He argues that in their investigations these three disciplines adhered to the main rules and regularities of the classical *episteme*. Natural history,

[8] The metaphor "table" that Foucault uses frequently in his discussion on the classical period allows him to emphasize his idea that knowing things in this period of Western thought meant representing them in their appropriate places within a static (non-historical) scheme of order. As we shall discuss below, Foucault uses the same metaphor in his analysis of the realm of exchange in the classical period. In particular, he argues that the realm of exchange constitutes an order (in reference to the exchange of equivalences) where things are represented through the monetary substance in accordance to their identities and differences in economic value.

for instance, confined itself to the ordering of living beings into a classification scheme. It was their proper places in this classification according to the common elements they possessed which constituted knowledge of living beings. Thus, "if biology was unknown, there was a very simple reason for it: that life itself did not exist. All that existed was living beings, which were viewed through a grid of knowledge constituted by *natural history*" (Foucault 1994b, 127-128, emphasis in the original).

Foucault's archaeological analysis has important implications for a reading, or rather a re-reading, of mercantilist economic thought. In this period, "in the order of knowledge, production does not exist. [...] the ground and object of 'economy' in the classical age, is that of *wealth*" (Foucault 1994b, 166, emphasis in the original). The mercantilist literature analyzed wealth in its relation to money as the representation of wealth within the sphere of exchange, and this was, for Foucault, in line with the general characteristics of the classical *episteme* based on the representation of identities (equivalences) and differences. And since money was the universal representation of wealth in the realm of exchange—on this table of equivalences—it is not surprising to Foucault that mercantilists identified money with wealth:

> If it was possible to believe that mercantilism confused wealth and money, this is probably because money for the mercantilists had the power of representing all possible wealth, because it was the universal instrument for the analysis and representation of wealth [...]. All wealth is *coinable*; and it is by this means that it enters into *circulation*—in the same way that any natural being was *characterizable*, and could thereby find its place in a *taxonomy* [...] in a *system of identities and differences* (Foucault 1994b, 175, emphasis in the original).

If the mercantilists did not analyze wealth within a conception of the economy based on the realm of production, this was not because they were not aware of this realm, nor was it because they thought production was not significant enough to merit a place in the analysis of wealth. The reason, to Foucault, was that they conducted their analysis with respect to a particular discursive construction of the economy that rested upon the realm of exchange, upon a non-historical table of equivalences, where wealth circulated in the form of money as the universal *representation* of wealth. Unlike in the Renaissance *episteme*, however, the representative power of money (its function as a sign) was not linked to the intrinsic preciousness and value of gold and silver. The relation was reversed in the classical period: whereas in the Renaissance *episteme* gold and silver could represent wealth due to their intrinsic value, in the classical period they had value as monetary instruments due to their function in the realm of exchange to represent wealth.

Modern economic discourse

There was another break, Foucault claims, at the archaeological level of Western knowledge at the turn of the 19th century. "[T]he theory of representation disappears as the universal foundation of all possible orders; [...] a profound historicity penetrates into the heart of things [...] [and] imposes upon them the forms of order implied by the continuity of time" (Foucault 1994b, xxiii). In the modern period, knowing things was not directed towards their representation in a non-historical table of classification, but upon their existence in real historical time. This is how knowledge of things became linked in the modern *episteme* to our understanding of their historical laws of development. It was as a result of this archaeological transition that "the analysis of exchange and money [gave] way to the study of production, that of the organism [took] precedence over the search for taxonomic characteristics" (Foucault 1994b, xxiii). It was the same change, according to Foucault, that consequently allowed biology to introduce life and historicity into the understanding of living beings, to study both the development of organisms and the origin of species.

In economics, the sphere of production eclipsed that of exchange, with all its accompanying elements of labor, capital, division of labor, accumulation, and the like. All economic categories and problems, that is to say, came to be defined and investigated in terms of their relation to the realm of production. Whereas in the classical period value was determined within the system of exchange—within a non-historical cycle of equivalences—where money functioned as the universal representation of wealth, in modern economics value was linked to the productive activity of the human being, i.e., to labor. The laboring activity, moreover, was dependent upon the means of production, division of labor, the amount of capital invested, and so on, which themselves were related to past labor and to its historical productive organization (Gutting 1989). In Foucault's account, "[t]he mode of being of economics [was] no longer linked to a simultaneous space of differences and identities, but to the time of successive productions" (Foucault 1994b, 256).

The break Foucault locates between the classical and modern periods provides us with some new insights into classical political economy. In Adam Smith, labor occupies a prominent place, consistent with the ascendancy of the realm of production over the sphere of exchange in economic analysis. But Smith's break from the classical *episteme* was not complete according to Foucault, for even though Smith established a link between labor and the value of things, this link was possible only if the quantity of labor necessary for the production of things was equal to the quantity of labor that they would command in the process of exchange (Foucault, 1994b)—the so-called labor-embodied-vs.-labor-commanded problem in the history of economic thought. In other words, labor in Smith's analysis still had a representative element as a constant *measure* of value; it represented wealth in the sphere of circulation; or rather, wealth circulated in the form of labor, which necessitated the equality of labor embodied to labor commanded. The classical discursive principle of representation was still decisive in Smith's economics as for him "all merchandise represented a certain labor, and all labor could represent a certain quantity of merchandise" (Foucault 1994b, 253).

It was Ricardo, Foucault claims, who initiated the decisive break from the classical *episteme* in economic discourse. Ricardo was not the first to give labor a prominent place in economic analysis, but he was the one who first "single[d] out in a radical fashion [...] the activity that is at the origin of the value of things" (Foucault 1994b, 253). For him, the quantity of labor still determined the value of things, but this was not because labor *represents* wealth, but because labor, as an activity, is the *source* of value (Foucault, 1994b).[9] In Smith's discussion of the division of labor, the market, i.e., the sphere of exchange, retains a central importance, as the division of labor depends on the extent of the market. Wealth, which circulates in the sphere of exchange in the form of labor, determines the division of labor and hence has its effect on the realm of production. In Ricardo, production proclaims its superiority, and labor as the value producing activity becomes the central element that makes economic discourse possible:

[9] Could this be the reason why Marx called Ricardo "the economist of production par excellence"?

> Whereas in Classical thought trade and exchange serve as an indispensable basis for the analysis of wealth (and this is still true of Smith's analysis, in which the division of labor is governed by the criteria of barter), after Ricardo, the possibility of exchange is based upon labor; and henceforth the theory of production must always precede that of circulation (Foucault 1994b, 254).

It has been a common criticism against Smith to suggest that he confused the amount of labor embodied in the production of a commodity and the amount that it can command in exchange, and Ricardo's contrasting approach has doubtless been very influential in this particular reading. In a similar fashion, mercantilists have been accused of confusing money with wealth; the popularity of that critique being largely driven by Smith himself. No matter what the final judgment be on these controversies, Foucault's interpretation provides a different avenue to approach them and to think about the discourse-specificity of theoretical problems and their solutions in economics.

Foucault's argument that modern economics starts with Ricardo has further repercussions for the study of 19th century economics in the sense of a new interpretation of Marxian economic discourse. Karl Marx, though acknowledging his debt to the important figures in classical political economy, argues that there are elements in his own theoretical structure that constitute a decisive break from classical political economy. In his *The poverty of philosophy*, for example, he emphasizes that classical political economy takes the relations of capitalist production as given and therefore cannot explain the historicity of these relations: "The economists explain to us how production is carried on in the relation given, but what they do not explain is how these relations are produced, that is to say the historical movement which has created them" (Marx 1995, 114). And since "[t]he economic categories are only the theoretical expressions, the abstractions, of the social relations of production" (Marx 1995, 119), concepts of political economy are devoid of historicity. Unlike his own analysis, Marx therefore argues, political economy studies the historical economic relations of capitalism as if they were the natural and eternal conditions of human existence.[10]

[10] Marx's own analysis of capital, however, not as a mere thing used in the process of production but as a historical social relation that defines capitalism is for him a case in point that shows the fundamental difference between his analysis and that of classical political economy. Furthermore, according to Marx the distinctions he introduces between abstract and concrete labor on the one hand and between labor and labor power on the other—main theoretical elements that he uses to develop his theory of

For Foucault, however, Marxian economics operates within the same archaeological field as Ricardo's. To make his point, Foucault draws our attention to three important consequences of the conception of labor in Ricardian discourse. The first, already mentioned, is the determination of value through a series of historical events where both past and current labor play their respective parts within the historical organization of production. The second concerns the notion of scarcity and the position of the human being in the face of scarcity. This position calls forth for Foucault a new conception of "man" as an economic agent in the modern period. Whereas in the classical period human beings entered into economic discourse only in terms of "their capacity to form representations of things they needed and desired" (Gutting 1989, 188), modern economic discourse constructs a human being which has to labor to satisfy its needs in its confrontation, or rather struggle, with scarcity: "*Homo economicus* is not the human being who represents his own needs to himself, and the objects capable of satisfying them; he is the human being who spends, wears out and wastes his life in evading the imminence of death" (Foucault 1994b, 257, emphasis in the original).

The third consequence concerns the relation of this human finitude to history. The "modern" history of human kind is the history of increasing wants and diminishing resources; it is a history during the course of which human kind increasingly feels the limitations of its being, i.e., its finitude. And this history will lead for Ricardo to a stationary state where there is no prospect for further development. The finitude of the human being, however, has a positive aspect for Foucault in the Kantian sense that what limits our knowledge of things makes at the same time this knowledge possible. It is the discursive construction of the human being in its finitude, in its limitation by scarcity, Foucault emphasizes, that makes modern economic discourse possible. Human finitude creates, therefore, the conditions of possibility of modern economics: in its finitude the modern human being establishes itself as a unified, centered, and rational subject, thereby creating a space where modern economics becomes possible as a human science.

What separates Marx from Ricardo in this regard is that whereas Ricardo the pessimist sees history unfolding toward a stationary state where the human being will face the unavoidable consequences of its

surplus value and exploitation—clearly differentiate his own account from "bourgeois economics" (Marx 1990).

finitude, Marx envisions a future where the human being, as the laboring subject, develops an awareness—when faced with the imminence of its finitude—that is supposed to initiate a radical change in the economic and social organization of society. Whatever their future projections, however, Foucault argues that both Ricardo and Marx see history as the struggle of the laboring subject to survive under the conditions of fundamental scarcity. In Ricardo, scarcity, hence human finitude, presents itself in historical time as increasing quantities of labor become necessary to produce the same amount of output due to diminishing returns. In Marx, on the other hand, scarcity finds its existence historically within the capitalist relations of production as capital accumulates through the exploitation of labor, and as the number of those who get no more than subsistence-level wages increases (Foucault 1994b). But despite such differences the scarcity-labor combination (together with the corresponding constitution of "modern" history) represents, for Foucault, a common locus in Ricardo and Marx at the archaeological level of knowledge: "At the deepest level of Western knowledge, Marxism introduced no real discontinuity" (Foucault 1994b, 261).[11]

Foucault's archaeological reading of the history of economic thought suggests that what counts as scientific knowledge of the economy is determined within a network of historical and discursive elements that elude the main problematic of epistemology. Foucault rejects, furthermore, the presupposition that the same conception of the economy exists in historically distinct theoretical structures, thereby dispensing with the established continuities in the history of economic thought. But besides its significance for the historian, Foucault's archaeology also has implications for the theorist and the methodologist of economics today within the general setting of "postmodernism and

[11] Even the marginalist school of the 19th century is not immune to Foucault's restructuring of the history of modern economic discourse. For the difference between labor and utility theory of value, Foucault very briefly suggests, is only a surface phenomenon (Amariglio 1988); they both are predicated upon the constitution of a finite human being in its confrontation with scarcity as its fundamental condition of existence. Whereas the labor theory of value puts the laboring activity of the human being at the center of its theoretical framework, the utility theory of value chooses to structure its theoretical analysis in the subjective sphere around need and desire (Foucault 1994b). They differ, in other words, only in the choice of the bodily function of finite "man" around which they articulate their respective theoretical structure: the laboring vs. the desiring subject in its confrontation with scarcity. Both subjectivities, however, belong to the same discursive formation for Foucault: the same discursive construction of modern man can be found, therefore, in various theories within modern economic discourse.

economics". The final section, therefore, will be devoted to a brief discussion of how Foucault's work might be important, not only for the history, but also for contemporary economics.

FOUCAULT, POSTMODERNISM, AND ECONOMICS

Foucault's relation to postmodernism is a complicated one, not least because Foucault himself never associated his work, method of analysis or way of thinking with postmodernism. Additionally, there is such a variety of usages of the notions of modernism and postmodernism that it seems virtually impossible to come up with an overarching definition of postmodernism today. Sometimes postmodernism is defined as the cultural form or expression of late capitalism, characterized by mass commodification, globalization of production, widespread use of information technologies, and so on (Jameson 1991). Others use the term in reference to a certain "style" of creativity and interpretation in architecture, art, literature, philosophy, and the like, that includes such stances as deconstruction and self-reflexivity, and that celebrates the instability of meaning, the presence of indeterminacy, the play of plurality and chaos and the impossibility of representation (Amariglio and Ruccio 2003). Still others look upon postmodernism as "a *discursive formation* that signifies a different relation to modernism that arose within and alongside modernism itself" (Ruccio 1991, 499, emphasis in the original). It is not my intention here to systematically analyze these or other definitions of postmodernism. But the third definition would seem to offer a congenial space in which to elaborate upon the theme of "Foucault, postmodernism, and economics".

Now, postmodernism in this sense entails a (critical) relation to and an attitude toward modernism that aims to uncover and call into question, in a deconstructivist sense, the hidden assumptions and underlying metaphysical underpinnings of modernism (Screpanti 2000). In this (postmodern) sensibility toward modernism, the main critique is leveled at the modernist assumption that the exercise of "human reason" in its pure, abstract, and non-historical form is able to achieve universal goals such as truth, freedom, democracy, emancipation, and development (Peet 1999). All these "metanarratives" (Lyotard 1984) of the modernist discourse are criticized in postmodernism, especially on two fronts.

First, postmodernism rejects the modernist construction of the human being as an abstract, centered, and unified entity with an inherent essence and rationality (theoretical humanism) and argues that our subjectivities are constituted within a play of systems of signs, discourse, desire, the unconscious, cultural norms, institutions, and so forth (Best and Kellner 1991). In thus 'decentering' the subject, postmodernism aims to show that the modernist quest to reach universal goals through the exercise of human reason, which neglects the various mechanisms through which individuals are regulated and subjectivized, is ill-founded.

Second, postmodern thought tries to demolish the strict modernist separation between science and rhetoric by denying the existence of universal and objective criteria of truth (Ruccio 1991). It argues rather that there are only different interpretations of reality, based upon different social structures of thought, which may or may not count as the true account of reality in different "regimes of truth". This critique implies that scientific rationality leads to a state of affairs where alternative interpretations of the world are cast aside and silenced in the name of universal norms of scientificity which are themselves historically, geographically, and culturally situated according to postmodernism thought.

From this perspective, it seems clear that Foucault's work has a significant affinity with postmodernism, even though one cannot easily extend this affinity to a close correspondence. Many have remarked for instance that Foucault's archaeology of knowledge includes elements of structuralism, which aspires to arrive at the universal laws between the constituent elements of a social phenomenon conceptualized as a structure, and thereby becomes the target of the postmodern critique. With respect to two specific points, however, there seems to be a close relation between Foucault and postmodern thought.

First, his analysis of Western rationality through an historical account of scientific discourses—in other words, his willingness to approach the problem of knowledge, not in reference to an abstract and centered epistemological subject, but from the perspective of the discursive rules and regularities that determine what can be thought and said within the confines of scientific rationality—fits with the postmodern critique of theoretical humanism. Scientific practice for Foucault entails a process of subjectification through the historical rules of a discursive formation, a process that cannot be explained by recourse to the autonomous subject of epistemology. The historical aspect of Foucault's archaeology also deconstructs the modernist notion of progress of knowledge, in line with the postmodern idea that there are only different interpretations of the world and that there is actually no basis for claiming one of them to be superior to others.

The second aspect of Foucault's relation to postmodernism lies in how his archaeology of knowledge provides us with a theoretical framework to make sense of the distinction (or tension) between modernism and postmodernism. This refers to his articulation of the modern *episteme*, its essential principles such as historicity, continuity and the birth of man, and to his anticipation of a new discursive formation that is characterized by the death of man as it is understood in modernism. For Foucault, in such "countersciences" as psychoanalysis and ethnology (and today perhaps we can also add cultural studies, feminist theory, postcolonial studies, postmodern Marxism, and the like), man loses its essential position as a unified and centered being (Foucault 1994b). These disciplines are paying ever more attention to the decentered subjectivities, i.e., the multiple rationalities and 'I' positions, which arise through the complex interactions of the unconscious, desire, taboos, culture, institutions, and so on (Ruccio 1991). Even though this new discursive formation that Foucault describes may not completely define for many the general milieu called postmodernism, it surely illuminates one central aspect of the postmodern critique of modernism.

Based on this, one can argue that Foucault's archaeology also helps us put the recent debate about "modernism vs. postmodernism" in economics into some perspective (Cullenberg, et al. 2001; Amariglio and Ruccio 2003). This debate has many facets, ranging from ontological premises to the problem of scientificity in economics. According to Screpanti, for instance, the ontological aspect of modern economics is characterized by its adherence to theoretical humanism, to "a humanist ontology of the social being" (Screpanti 2000, 88). Amariglio and Ruccio (1994) see the main tension as revolving around such dichotomies as order/disorder, certainty/uncertainty and centering/decentering. McCloskey (1985) and Klamer (2007) call into question the claim of economics to scientificity by showing the rhetorical and conversational elements of modern economic theorizing. I do not have space here to delve into the intricacies of this debate; therefore, I shall confine the discussion in this concluding part to a few examples that show in what ways Foucault's archaeological framework bears upon the issue of 'postmodernism in economics'.

The postmodern critique of theoretical humanism serves here as our entry point. If, as Foucault argues, modern economics is discursively predicated upon the construction of human finitude, upon the bodily wills, desires and functions of man as a unified, centered and rational subject, then a postmodern discourse in economics, characterized by the death of man *à la* Foucault, would go beyond this theoretical humanism to construct a human subjectivity that is fragmented, decentered, indeterminate, and unstable (Amariglio and Ruccio 2003). Resnick and Wolff (1987), for example, in their rethinking of the Marxian notion of class move toward a postmodern stance when they conceptualize class, not as a stable and unified entity, but rather as a process in which people are involved in various ways. An individual may therefore partake in different class positions, and hence embody different subjectivities, in the processes of production, appropriation, and distribution of surplus value. Salaried employees, for example, such as managers, state officials and supervisors, get a share from total surplus in many complex and overdetermined ways, Resnick and Wolff argue, in so far as they contribute to the conditions of existence of the capitalist system. Hence the existence of unified class positions and subjectivities (the laboring subject of classical Marxism) is rejected in their postmodern Marxian analysis of capitalist relations.

To illustrate further, some recent feminist research, and feminist economics in particular, criticizes the idea that feminist movements should seek to construct a stable feminine identity in its struggle against gender-based inequalities in society. This approach argues that since subjectivities and identities cannot be stable, gender (whether biological or a cultural construction) cannot establish unified and unambiguous subject positions (Butler 1999). Instead of creating, therefore, a construct of the modern human being with a bi-polar gender identity, such feminists take a postmodern position when they find gender differences in certain "acts" that individuals perform through their body. In their understanding of gender as performative, postmodern feminists argue that disciplinary techniques in society force subjects to perform specific bodily acts and thus create the appearance, or rather the illusion, of an essential, centered, and unified gender (Butler 1999; Hewitson 2001).

Amariglio and Ruccio (2003) in their analysis of the postmodern "moments" of modern economics argue that in the (modernist) economics texts of Knight, Shackle, and Keynes the notion of uncertainty as irreducible to probabilistic calculations undermines the construct of the knowing economic subject as it appears in modern economic discourse. For Amariglio and Ruccio, the distinction Knight introduces between risk and uncertainty (where the former lends itself to a priori calculations, but the latter not); Shackle's treatment of uncertainty as creating a space for creative and imaginative processes to enter into the decision making of the economic subject; and Keynes's notions of animal spirits and spontaneous optimism in his theory of investment are all postmodern moments in the sense that they all demonstrate that the rationality of economic agents can be overdetermined by a multiplicity of "psychological drives, hidden motivations and desires" as well as "conscious or unconscious forms of decision making" (Amariglio and Ruccio 2003, 87-88). What needs emphasis here perhaps is that the postmodern moments that Amariglio and Ruccio point to reveal the possibility of an economic theorizing that does not make recourse to a centered and unified subjectivity with a singular rationality. This point is important because the decentering of the unified economic subject of modern economic theorizing has influenced various schools of thought in economics, even though one cannot always find explicit references to Foucault or postmodernism in these literatures. Screpanti (2000) calls our attention, for instance, to the role that uncertainty plays in the post-Keynesian literature as a postmodern element. Amariglio and Ruccio (2003) further emphasize that the notion of general equilibrium as a state of order created through the rational and orderly behavior of economic agents becomes problematic once we allow for decentered subjectivities in economic theorizing. According to Screpanti (2000), evolutionary ways of thinking in institutional economics that maintain that economic processes cannot be explained in reference to an equilibrium ontology bear further testimony to this.

To conclude, I would like to emphasize that Foucault's work still awaits a close consideration by economists, including historians and methodologists of economic thought. The possibilities and challenges that Foucault offers for reading the history of economics from an unorthodox perspective and for moving beyond modernist theorizing in economics deserve more serious attention than they have received so far. This paper is an invitation for economists to take Foucault more seriously; an invitation based upon my belief that the incorporation of the Foucauldian framework into various conversations in economics (historical, theoretical or methodological) would fill an important gap.

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Extensionalism and intensionalism in the realist-SSK 'debate'

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Abstract: The 'strong programme' in the sociology of scientific knowledge (SSK) is based upon finitism and extensionalism. This article examines a critique of these bases. It is argued that David Tyfield's (2008; 2009) realist critique and his alternative intensionalist account of meaning face problems at least as serious as those he identifies in the strong programme's finitism. This is not to say that the strong programme is problem-free: it fails to give sufficient acknowledgement to non-conventional constraints on meaning formation and change. It is also suggested that, as they are currently conceived, realism's intensionalism and the strong programme's extensionalism are irreconcilably incompatible at such a basic level that the 'debate' between them reduces to an exchange of 'assurances'.

Keywords: strong programme, sociology of scientific knowledge (SSK), meaning finitism, extensionalism, intensionalism, transcendental realism

JEL Classification: B40, Z13

In a previous issue of this journal, David Tyfield (2008) offered a two-pronged critique of the 'strong programme' in the sociology of scientific knowledge (hereafter SP).[1] The first prong is the well-known claim that the SP is logically flawed because it entails a self-refuting relativism.[2]

[1] This article restricts itself to the Edinburgh School's 'strong programme' in the sociology of scientific knowledge, as this is the focus of Tyfield's (2008) critique. David Bloor's work, and especially his *Knowledge and social imagery* (1976), is taken to be exemplary of this School's approach and doctrine. The Paris and Bath Schools in the sociology of scientific knowledge are exempted from this analysis.

[2] Yann Giraud and E. Roy Weintraub's (2009) reply to Tyfield (2008) focuses upon this criticism. Their counter-argument, in summary, is this: Tyfield presupposes a conception of truth that prejudices his conclusion and is not accepted by advocates of the SP. In essence, if proposition P is judged to be true by standard S (presupposing, say, a pragmatist theory of truth as Giraud and Weintraub say the strong programme

The second prong is an attack on the SP's finitism and extensionalism. Tyfield argues that the SP's finitism entails a false dichotomy, that it results in incoherence, and that it neglects a superior alternative account of meaning, namely intensionalism. In the present article, the relationship between finitism and the SP will be outlined, and then Tyfield's critique and his proposed alternative will be examined and shown to be problematic. It is then argued that the flaws in the SP are not due to its lack of an intensionalist theory of meaning. Finally, it is suggested that the realist-SP debate over meaning reflects the much older essentialist-nominalist dispute.

THE STRONG PROGRAMME'S CONVENTIONALISM AND FINITISM

The SP has been characterised by Bloor (1976, 4-5) as possessing the following defining features. (1) It is concerned with discovering the causes of scientific beliefs and knowledge-claims, and especially (but not only) the causal social conditions that contribute to their coming about. (2) It seeks to explain the content of scientific claims irrespective of whether they are taken to be true or false, rational or irrational, successful or unsuccessful. That is, it does not seek to explain *only* false, irrational, or unsuccessful claims as does the 'traditional' Mertonian (1973) sociology of scientific knowledge. (3) The same types of causes attributed to true, rational, and successful scientific claims are to be attributed to false, irrational, and unsuccessful ones. This amounts to combining (1) and (2), viz., that the same kinds of causal explanations, especially involving social factors and communal interests, should be attributed to both true *and* false claims. (4) SP is itself a scientific enterprise, and thus it can be investigated on the basis of (1), (2), and (3). The mode of explanation that the SP uses to account for scientific claims should be applicable to the SP itself.

We might reasonably summarise the above key features in the following way: the SP is concerned with discovering the conditions, and especially the social forces and group interests, which causally explain all scientific beliefs or knowledge-claims (including those made by the SP itself), be they true or false, rational or irrational, successful or unsuccessful.

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does) and false by an incompatible standard S* (presupposing, say, a realist correspondence theory of truth as they say Tyfield does), it is illegitimate for Tyfield to say that P is 'in fact' false just because he *assumes* S* to be correct. In what follows, insofar as it is possible, I seek to avoid the 'relativism and self-reflectivity debate' in which Giraud/Weintraub and Tyfield (2009) engage.

The SP has gained a reputation for being radical and innovative, because it presents itself as breaking with what it takes to be the foundationalism of traditional epistemology, which is said to hold that knowledge consists in the guaranteed justification of true beliefs (or propositions). Bloor rejects the view that we can justifiably judge whether statements about the (mind-independent) world are true, where, *as per* the traditional correspondence theory of truth, 'true' stands for a strict one-to-one matching-relation between the terms of a proposition and the elements of some 'real object' which it is held to identify and represent: "At no stage is this correspondence [between a theory and reality] ever perceived, known or, consequently, put to any use" (Bloor 1976, 34).[3] Bloor's rejection of the correspondence theory is made on two grounds. First, the concept of correspondence is "very vague" and "difficult to characterise in an illuminating way" (Bloor 1976, 32). Second, it is not possible to know whether the correspondence relation holds because "[w]e never have the independent access to reality that would be necessary if it were to be matched up against our theories" (Bloor 1976, 34).[4]

Bloor invites controversy with his alternative conventionalist epistemology. He argues that:

> [T]here is one sort of correspondence that we do indeed use. This is not the correspondence of the theory with reality but the correspondence of the theory with itself. Experience as interpreted by the theory is monitored for such internal consistency as is felt important (Bloor 1976, 33).

Scientific theoretical and empirical developments are regulated by similarly "internal principles of assessment" such as predictive success and accuracy, scope and coherence; and the trajectory of development is determined and motivated by "our theories, purposes, interests, problems and standards" (Bloor 1976, 34).

Since scientific claims are developed and assessed by the internal, self-imposed "standards" or methodological "requirements" of our theories and experience—rather than by correspondence with reality—and since historical analysis is said to reveal that there are numerous

[3] Bloor says this in the context of commenting on Priestley's experimental testing of the theory of phlogiston.
[4] Bloor supports his claim by noting that as theories have failed on their own terms and have been subsequently revised, apparently known truths have been rejected and revised over time.

such standards and requirements, then "[i]t should be possible to see theories entirely as conventional instruments for coping with and adapting our environment" (Bloor 1976, 35).[5]

How does all this relate to finitism? According to Bloor (1983), finitism "is probably the most important single idea in the sociological vision of knowledge. It shows the social character of that most basic of all cognitive processes: the move from one instance of concept application to the next" (Bloor 1991, 165).[6] So, the SP's sociological theory of knowledge, including scientific knowledge, is built upon a sociological theory of *meaning*.

Finitism presupposes an extensionalist theory of meaning. And extensionalism holds that universal terms are used in context-specific ways to denote classes of particulars. In its modern formulation—following Wittgenstein—it is 'use that determines meaning', not the other way around; and it is rules about the use of particular and universal terms in a variety of context-specific 'language games', learned in an iterative (and initially ostensive) fashion, that give those terms their specific meanings. Importantly, however, the past application of a given word to *finite* cases does not determine how that word will be used in future cases (hence the term 'finitism'). There is no logically necessary reason why the rules of use in themselves would prevent *any* kind of new extension, be it to new particulars of an existing class, or to entirely new uses for entirely different classes.[7] Although the formation and extension of terms to new particulars have no *logical* constraints, there must be some kind of constraint on existing and new extensions lest conceptual chaos ensue.

According to Bloor (2007), Wittgenstein held that narrowly 'philosophical' attempts to constrain meanings with formal and abstract conditions must founder on the rocks of an infinite regress: "if a rule

[5] Despite this, Bloor argues that the notion of—or better, the term—'truth' still has some use-value and so should not be discarded. It serves a "discriminatory function" used to indicate which theories are currently assessed to 'work' and which do not; a "rhetorical function" by which a claim is authoritatively recommended as more than "mere belief"; and a "materialist function" by which "we mean just this: how the world stands" (Bloor 1976, 35-36). Obviously, these functions have no truck with the correspondence notion of truth.

[6] Bloor claims that finitism can be traced back to Ludwig Wittgenstein's (1973 [1953]) *Philosophical investigations*.

[7] Take, for example, the term 'unemployed'. In the former case (extension to new particulars of an existing class) one may extend the term to cover, say, those who have lost hope in finding work and have thus taken early retirement ('hidden unemployed'). Yet one may use the term in an entirely different way to denote an entirely new class—say, to cynically denote legislatively powerless Heads of State.

depends on an interpretation then the interpretation demands an interpretation" and so on. Instead, the limits—the particular (and changeable) rules of meaning-formation and meaning-extension—are determined by a whole way of life. In Bloor's words, for Wittgenstein:

> [T]he real determinants of the next application, and the real sources of the discrimination between correct and incorrect steps and applications, were not to be found in the realm of formal specifications and justifications but amongst the totality of contingencies that impinge on the episode. He was not saying that the move to the next case was undetermined. Rather, the determinants lie around or behind the formal specifications but do not appear in or amongst them (Bloor 2007, 212).

Needless to say, for Bloor, the "totality of contingencies" includes enforced, contested and "negotiated" conventions that are in large part caused by social, institutional forces, and the particular "needs" and "interests" of a community. Thus, the linguistic chaos that finitism (understood abstractly) threatens is prevented by the normativity inherent in communal agreement over conventional rules of "right" word-use.[8] And so it is with the communal activity of science. One can thus sensibly characterise theories, methods, and facts as being 'inherently social' and 'socially constructed', since the extensional semantic properties and the formal structures of scientific theories as well as their empirical findings are—and for Bloor must be—limited and determined by normative social conventions.

This account of normative, conventional, socially influenced scientific knowledge is said to draw its force, on the one hand, from numerous empirical case-studies of meaning-formation and change in science, and on the other hand from the inadequacy of the dominant alternative account of meaning.[9] The dominant alternative, in very

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[8] Apropos of this point, Bloor says:

> The group collectively 'decide' on the norms of proper usage of their concepts and classifications and they create the norms of their correct use in the course of invoking them. They do not collectively discover the norms, as if they were a further feature of the world, even if it may occasionally seem like this to the individual concept user (Bloor 2007, 215).

[9] The present article avoids an analysis of any case-studies for two related reasons: First, there are now very many and varied case-studies in the literature. Any case-study analysis then would have to be highly selective, and in so doing, would inescapably open itself up to the charge of 'cherry picking'. Second, some realist critics of the SP and other modern schools of the sociology and ethnography of science dispute the veracity of *all* such case-studies. For example, Peter Slezak argues as follows:

general terms, is intensionalism. This position holds that the uses of words are determined by their meanings, where meanings are due to the common or essential properties of entities either *ante rem* in the realm of ideas (à la Platonism) or *in re* in the realm of the material (à la Aristotelianism). The SP's conventionalism undermines intensionalism because, with different "internal principles of assessment" motivated by different "theories, purposes, interests, problems and standards", there is logical space for different so-called 'essential' properties at different historical junctures or in contemporaneous conflict. By the SP's lights, this is a *reductio* of intensionalism.

A REALIST CRITIQUE OF FINITISM

Tyfield (2008; and 2009) draws upon a robust ontological realism to attack the SP's account of meaning. He asserts that the SP sets up a dilemma that forces us to choose between either logically pre-determined particular future uses of terms or logically undetermined particular future uses (2008, 76). In presupposing extensionalism we are then seemingly compelled to choose the latter option—since logically and universally pre-set usages are taken to be manifestly false—and thus to commit ourselves to a logically indeterminate 'negotiation' of rules over extensions that are causally influenced by social forces.

For Tyfield, this dilemma is illusory because (i) finitism entails the fatal problem of making the SP 'unintelligible' thereby rendering it an

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> The extensive body of case studies repeatedly invoked by sociologists to answer their critics has been taken to establish the thesis that the contents of scientific theories and beliefs have social causes, in contradistinction to psychological ones. [...] [T]he claims of social determination of beliefs are all the more extraordinary in view of the utter failure of these case studies to support them. Critics have challenged precisely the bearing of these studies on the causal claims, and so repeatedly citing the burgeoning literature is to entirely miss the point. [...] [T]o the extent that social factors are indeed ubiquitous, establishing a causal connection requires more than merely characterizing in detail the social milieu which must have existed. These more stringent demands have not been met anywhere in the voluminous case studies in the SSK literature. [...] Thus, it is a truism to assert, as Shapin does, merely that "Culture [taken to include science] is developed and evaluated in particular historical situations". Shapin undertakes to refute the accusations of empirical sterility by a lengthy recounting of the "considerable empirical achievements" of the sociology of scientific knowledge. But he is simply begging the question with his advice that "one can either debate the possibility of the sociology of scientific knowledge or one can do it" (Slezak 2000, 7-8).

This indicates that the status of case-studies, and what they demonstrate or not, is the subject of a debate all of its own. To merely gloss over this debate would be to do an injustice to all sides, and to engage with it would consume an entire article. The prudent path taken here is to leave the matter to another time and place.

unacceptable option in the first place, and (ii) intensionalism is not given its due consideration as a viable account of meaning. Let me stress three aspects of Tyfield's position:

(A) With respect to (i), Tyfield argues that the SP is shown to be unintelligible once it is realised that the 'social forces' governing rules of term-use *are themselves said to be rule-governed*. Since finitism's rules do not analytically tell us about future states of affairs, social forces themselves are analytically indeterminate and so the grounds for stable term-use, and thus stable meanings, are undermined. If rules really did have no logically determinate implications (that is, if finitism were true), then current rules allow one to say (mean) anything at all and nothing in particular about the future—even the immediate future. Thus, the rules governing statements *in* the SP (and everywhere else) mean anything at all and nothing in particular. If statements can mean anything at all and nothing in particular, then statements are unintelligible. Therefore, if finitism, and so the SP, is true, the SP is unintelligible.

(B) In order to establish (ii), Tyfield argues that because rules—both theoretic (relating proximately to meaning) and extra-theoretic (relating proximately to behaviour)—do indeed exist, do play a causal role in the determination of meanings, and are intelligible to us, they must "*not*, at any given time, [be] totally unlimited in application", and so "do have intrinsic, determinate content, i.e., they are intensional and not just extensional" (Tyfield 2008, 78), where "intensionality is understood here as the possibility of a proposition or term to have a determinate meaning in a given sociohistorical context and not a fixed, complete and perfect essence" (Tyfield 2009, 65).

In particular, Tyfield asks that a crucial distinction be acknowledged: words have determin*ate* current content but this does not uniquely determin*e* future content for all-time. Accepting this implies "internal relations of necessity between different meanings, hence rendering meaning relatively resistant to our use of it so that we cannot simply do as we please—*even collectively*—with meaning, *pace* [SP]" (2008, 80; emphasis in the original). For example, in our society at the present time, there is a necessary semantic relation between the terms 'water' and 'H2O'. We cannot simply choose to mean, say, 'electricity' when we speak of water. For we know that there is something about an entity *being* water—i.e., its molecular structure—over and above the application of some other terms that gives the word its meaning. That said, naming-rules at a particular time in a particular community do not universally determin*e* meaning for at least two reasons. First, intensions depend on knowledge: it is possible, say, in a community unaware of molecules, that 'water' just means 'liquid that falls from the sky'. Note that this latter case is still intensional because it is crucially about something *being* water. Second, the same words can be extended to quite new cases with new meanings—for example, 'watery eyes', 'watery grave', 'an explosion at the Water Works'. Nonetheless, these extensions are ultimately parasitic upon the original 'essential' or 'basic' intensional meanings which lie at the base of the commonality between all conceivable uses of the term 'water'. Again, without this kind of anchor, linguistic chaos would ensue.

In his rival account of meaning, Tyfield implicitly draws upon Roy Bhaskar's transcendental realism,[10] so for a more complete exposition let us turn to the latter's quite explicit statements on the matter. In the course of his discussion of what science seeks to discover, Bhaskar (1975) argues that "Leibnizian" natural kinds (kind K is composed of all x's with the "constitution or intrinsic structure" N) are expressed by "real definitions". In Bhaskar's own words:

> Real definitions are definitions of things, substances and concepts; nominal definitions are definitions of words. (Nominal essences are the properties that serve to identify things). Real definitions, in science, are fallible attempts to capture in words the real essences of things which have already been identified (and are known under their nominal essence) at any one stratum of reality. As so conceived, they may be true or false (not just or even more or less useful). [...] [For example] [i]f the real essence of copper consists in its atomic (or electronic) structure, its nominal essence might consist in its being a red sonorous metal, malleable and a good conductor of electricity, etc. But conversely just because the word 'copper' in science has a history, and at any moment of time a use, the nominal essence of copper cannot suddenly be designated by the use of 'reppoc' or 'tin'. Nominal definitions in science cannot therefore be

[10] Although Tyfield never explicitly says so, it seems clear that he is drawing heavily upon Roy Bhaskar's (1975; 1979; and 1989) transcendental realism (now called 'critical realism'). This variant of realism has been most conspicuously championed in economic methodology by Tony Lawson (1997; and 2003). My supposition is based on Tyfield's appeal to a "novel approach [that] is effectively 'transcendental' or 'critical' in nature, involving examination of the necessary conditions of possibility of the premise", and "an alternative approach of a critical and transcendental philosophy" (Tyfield 2008, 63), which deploys "transcendental, i.e., a specifically *philosophical*, argument", a "transcendental approach", "transcendental reasoning" (Tyfield 2008, 80, 81) and a "transcendental analysis" (Tyfield 2009, 60, 61, 68).

> conceived as stipulative, arbitrary or matters of convention (Bhaskar 1975, 211).

Despite the falliblist caveat, we can say that once the real essences of things have been discovered and correctly expressed in real definitions, those definitions must universally and invariably *determine* the correct classification of things and regulate their "nominal definitions"—that is, they would regulate the correct use of words by reference to the "constitution of things". This is an intensional approach to meaning *par excellence*, and for transcendental realism goes to the heart of scientific activity:

> Scientists attempt to discover what kinds of things there are, as well as how the things there are behave; to capture the real essences of things in real definitions and to describe the ways they act in statements of causal laws. [...] Thus there is no conflict between explanatory and taxonomic knowledge. Rather, at the limit, they meet in the notion of the real essences of the natural kinds, whose tendencies are described in statements of causal laws (Bhaskar 1975, 173-174).

With this additional information, Tyfield's crucial distinction between determin*ate* meaning and uniquely determin*ing* meaning progressively disappears as science achieves its goal of discovering real essences expressed by (correct) real definitions. "At the limit", on this view, there would seem to be no means by which meanings could change (other than by, say, disturbing psychological or social forces); that is, *contra* Tyfield's claim, determin*ate* meanings would indeed uniquely determin*e* scientifically established rational future uses.

(C) Tyfield (2008, 80, 81) attributes his identification of the SP's fatal flaw of extensionalism *and* the necessity of an intensional theory to the use of "transcendental, i.e., a specifically *philosophical*, argument" or "transcendental reasoning" in accordance with transcendental realism. A transcendental realist argument, as originally formulated by Bhaskar (1975), begins with the question: 'What is necessary in order for P to be possible?' In other formulations, it begins with the question: "What is necessary in order for P to be intelligible?" The argument seeks to demonstratively infer and establish a priori what must be true *from* the possibility (or intelligibility) of P, where P has already been fallibly established a posteriori. In short, it seeks to establish synthetic a priori truths. A transcendental argument can be expressed in a number of ways. One form is the following:

(1) $\sim\Box Q \rightarrow \sim\Diamond P$
(2) $\Diamond P$
(3) $\therefore\ \Box Q$

We can summarise Tyfield's 'negative' and 'positive' transcendental arguments as follows:

(1) If SP is true, then extensionalism is true.
(2) If extensionalism is true, then determinate meanings are impossible.
(3) If determinate meanings are impossible, then SP is unintelligible.
(4) ∴ If SP is true, then SP is unintelligible. [From (1), (2), (3)]
(5) Determinate meanings are possible.
(6) ∴ Extensionalism is false. [From (2), (5)]
(7) If extensionalism is false, then intensionalism is true.
(8) ∴ Intensionalism is true. [From (6), (7)]

CRITICAL COMMENTS ON THE REALIST CRITIQUE OF FINITISM

I would make three related critical comments on Tyfield's critique of finitism. The first comment is really just a rectification of a conceptual confusion. The remaining two however, are more substantial in that they suggest that Tyfield's approach has difficulties that are not dissimilar to the ones he himself raised with finitism.

Regarding (A), Tyfield asserts, slightly strangely, that the SP's "positive claims, if true, would be unintelligible" (2008, 77). I say this is strange because if one takes "unintelligible" here to refer to a contradictory statement,[11] and accept the axiom of classical logic that a contradictory statement must be false, then it is impossible for such a statement to be unintelligible *and* true. Thus, ironically, the claim: 'If the SP is true, then the SP is unintelligible' is itself unintelligible. Further, this being so, Tyfield's contrapositive, "[i]t follows that if we understand

[11] We might consider three possible types of unintelligibility. Logical unintelligibility: where a statement contains a logical contradiction such as 'The sky is blue and the sky is not blue'. Linguistic unintelligibility: where a statement does not follow (or fails to approximately follow) any grammatical rules, such as 'Is was pile out', or its terms are highly ambiguous, such as in the metaphorical lines, 'The night shifts her gaze, spawning a thousand doubting tears'. And translation unintelligibility: where a statement is expressed in a language or a code that one does not have sufficient knowledge to decipher. (Thanks are due to Dr. William E. Worner for useful discussions on this matter.) It is assumed here that Tyfield is using the term in the first sense, because the others do not serve Tyfield's argument.

the claims [of the SP], they must be wrong", does not logically follow. The premise that is missing from Tyfield's argument that would solve this problem is: 'If alleged rules do not have determinate implications, then they are not really rules at all'. The implicit presupposition that 'there are rules that are not rules' must be false. The correct formulation would then be, 'If the SP contains contradictory propositions, then taken as a whole, the SP is both unintelligible and false'.

Regarding (B), as noted, the distinction between determin*ate* meaning and uniquely determin*ing* meaning progressively disappears as science achieves its goal of discovering real essences expressed by (correct) real definitions. But this would seem to imply that until that "limit" is reached—until the Holy Grail of real essences has been discovered—we do not really have meanings that are so ontologically 'tied down'. What then determines meanings, for it cannot be as yet unknown real essences? Perhaps we could appeal to "Lockean" nominal definitions—that is, classifications defined by knowledge of the 'surface' properties of things. But is it not the case that things possess very many, maybe even an infinite number of properties? How are we to select the limited 'defining' properties without the guidance of the real essences? Bhaskar tells us that:

> To classify a thing in a particular way in science is to commit oneself to a certain line of inquiry. Ex ante there will be as many possible lines of inquiry as manifest properties of a thing, but not all will be equally promising (Bhaskar 1975, 210).

True enough, but does that not mean that the classification of things—and the meanings of terms—will be governed not by real essences or even by properties of things *per se*, but by the somewhat nebulous state of "commitment" (surely something influenced by social conventions) as well as methodological conventions that give meaning to terms such as "promising"? Alas, this would seem to return us to an extensionalist notion of meaning formation, to say nothing of the SP—the rejection of which is the *raison d'être* for the realist-intensionalist theory. One way out of this might be to say that we *do* have knowledge of at least some real essences and thus do have some real definitions.[12] But how do we know this to be the case? The real essence of a thing is said to be "the most important explanatory property", but how do

[12] Bhaskar seems confident about hydrogen, nickel, and copper at least (Bhaskar 1975, 173, 210, 211).

we determine the meaning of the somewhat vague phrase "most important"? Most important for *what* explanatory purpose? *Whose* purpose? And all this is to say nothing of what we shall decide "explanatory" means. At some point, it would seem, we must simply assert that this or that is the ultimate real essence and real definition of x and leave it at that. This involves making an *assumption* at some point that a final truth has been achieved, and about the impossibility of the future discovery of error or improvement. That would return us to a basic analytical arbitrariness—and even to the relativism that the entire effort was designed to avoid in the first place.

Regarding (C), the central challenge for transcendental deductions, as with all deductions, is for the premises to be formulated and specified quite precisely (and in Bhaskar's case, they must also be empirically well-secured). Any ambiguity in or doubts about the truth of the premises allows for the logical possibility of an infinite number of alternative conclusions to be deduced (or none at all, depending on one's interpretation of what is an allowable deductive inference under such conditions). The problem here is that Bloor's account of meaning finitism allows for the possibility of ambiguity in, or doubts about, propositions. Yes, the normativity of social conventions does foreclose chaotic word-use (at least according to Bloor, but not to Tyfield), but it does not eliminate indeterminism in word-use because "negotiation" over and innovation in conventions are always possibilities in living communities, including scientific and philosophical communities. As such, there are always grounds for ambiguity and doubt which undermines the prerequisites of an epistemically secure transcendental deduction. So, if we are to seriously claim that SP can be definitely refuted by means of a transcendental deduction, we must *presuppose* the falsity of Bloor's meaning-finitism and the truth of an undiluted intensionalism. In other words, in order to even mount Tyfield's transcendental argument, we must presuppose what is intended to be proved (*petitio principii*).

THINKING ABOUT THE STRONG PROGRAMME ONCE MORE

Bloor is concerned to assuage worries about arbitrariness often attributed to conventionalism. He explicitly argues that:

> [C]onventions are not arbitrary. Not anything can be made a convention, and arbitrary decisions play little role in social life. The constraints on what may become a convention, or a norm, or an

> institution, are social credibility and practical utility. Theories must work to the degree of accuracy and within the scope conventionally expected of them. These conventions are neither self-evident, universal or static. Further, scientific theories and procedures must be consonant with other conventions and purposes prevalent in a social group. They face a 'political' problem of acceptance like any other policy recommendation (Bloor 1976, 37-38).

In another place, he re-affirms the point:

> Conventionality [...] implies that the behaviour of any one follower of the convention is conditional on the continued conformity of a sufficient number of others. The collective 'decision' to use a concept in a certain way is not arbitrary; it must be one that is perceived to have utility for the group of users and it must be consistent with, and sustainable by, their innate cognitive proclivities—such as the natural operation of their pattern-matching machinery (Bloor 2007, 215).

These passages certainly serve to rule out 'actually existing arbitrariness' (as opposed to 'in principle logical arbitrariness'). What could be objected to, however, is what it ignores. In particular, I suggest that Bloor's account of the constraints on the formation of knowledge-claims, and meanings more generally, does not take sufficient account of objective constraints *other than* those of negotiable social conventions. That is to say, given a set of discursive conventions, there are still some objective *intra-theoretical*, *inter-theoretical*, *and 'worldly'* constraints on extensional possibilities.

This claim can be initially illustrated by means of maximally conventional cases of both objects and activities. For example, take Bloor's (1991, 174) "valid banknote" example as the utmost case of the conventionality of an object. Even here, there are constraints that are not really due to social conventions, such as practical considerations about size, durability, reproducibility, and so on, of the things which are to function as notes. Further, these practical considerations can change depending on yet other objective (non-conventional) factors—for example, technological change ('electronic' money) rendering constraints on physical durability largely irrelevant. An example of an activity that is maximally conventional is the game of chess. The rules of chess are largely conventional (the initial position of the kings on the chess board is not somehow inherent 'in nature' or somehow otherwise 'in' the intensional sense of 'king'). A change in initial position on the board may be negotiated between players—or forced by one player upon the other. There are limits however, as to what could be negotiated/forced in order for there still to be a contest (a game) between players. For example, it would not be possible to retain all the existing rules, but negotiate the kings' initial positions to be adjacent to the opposing queens, or that the kings be initially positioned in a cupboard. And of course, there must be *some* real object that can be practically used as (conventional) representations of chess pieces (be it bits of wood, or pixels on a screen, or whatever). *Mutatis mutandis* less-than-maximal cases of social conventionality.

Let us then look very briefly at objective *intra-theoretical*, *inter-theoretical*, *and 'worldly'* constraints in turn. With respect to non-conventional *intra-theoretic* constraints, one can find them in logic, mathematics and empirics. For example, in the case of logic, there is an intra-theoretical non-conventional constraint of general coherence: non-contradiction within some set of inferential rules is an objective constraint on particular inferences, such that it is not possible to incorporate, say, 'P is $\sim P$' or '$P \rightarrow \sim P$' or '$\sim\Diamond P$ & $\Box P$' as axioms.[13]

[13] It may be noted that Bloor does not deny that there are constraints *per se* on what is negotiable in logic or mathematics. For example, with respect to *modus ponens*, although characterising it as a "logical convention", Bloor offers two types of constraints that prevent the convention from being abandoned. The first constraint upon dropping *modus ponens* is a biological one: it is "innate", a "feature of our natural rationality". The second, and apparently only other constraint, however, is a *social* one: *modus ponens* is prescribed a "cognitive institution" which is given "special protection" from any possible doubts that might otherwise arise due to some ingenious counter-example or other.

We may grant that *modus ponens* is 'hard-wired', and even grant that it is given institutionalised "special protection" from nocturnal doubts. But we may still wonder why such protection is necessary. Might it not be because without *modus ponens*, most of our other reasoning just could not 'go through'? That is, is not the "special protection" really just the making explicit of what is an implicit intra-theoretic constraint?

As an example of the kind of constraints on mathematics Bloor has in mind, one may look to his discussion of Lakatos's historical analysis of the polyhedron. Bloor writes:

> The concept of a polyhedron could not govern men's behaviour in deciding what was to be included in, and what was to be excluded from, its scope. This does not mean that nothing acts as a constraint in these circumstances. The extension and elaboration of concepts can plausibly be seen as both structured and determined. They are determined by the forces at work in the situation of choice—forces which may be systematically different for different men (Bloor 1976, 139).

However, he does not nominate theoretically objective constraints as the "forces at work in the situation of choice". Again, he appeals to *social* constraints explained by factors such as "the professional commitments and backgrounds of actors" (Bloor, 1976, 140). He seeks to illustrate his claim by an analogy with the power-relations between a parent and a child. When the child extends the word 'hat' to a tea-pot lid:

This is precisely why counter-examples to inferential rules are objects of such consternation to logicians (see Smith 1984; and Mortensen 1989). The same goes for mathematics, as it also does for empirical investigations generally. For example, given the conventional stipulation of a concept in terms of discrete variables, it is not then possible to subject it to differentiation (except by fitting some continuous curve and differentiating that). Or, given that the conventional meanings of 'height' and 'weight' have been stipulated, the measurement of height cannot be used as a measure of weight. Or, once it has been stipulated that demand for a commodity is strictly 'price elastic' only where the coefficient $\eta \leq -1$, it is not then possible to say that demand is 'fairly price elastic' where $\eta = -0.2$.

There are also non-conventional *inter-theoretical* constraints. For example, it is not possible to apply the rules of propositional logic to a poem by Dylan Thomas because the latter's terms are not sufficiently clear to be made subject to analysis by the former. Or, given our current state of knowledge, we are constrained in subjecting the amorphous concept of 'consciousness' to psychometric analysis of the kind used by 'differential' theories in psychology. Or, there is an objective constraint on the translation of an exploitative relation given in terms of Robinsonian monopsony theory into an exploitative relation given in terms of a Marxian labour theory of value.

There can also be mundane *practical* constraints: conducting a molecular analysis of every beam of steel used in the construction of a building is not feasible due to time and financial constraints. Similarly, a psychological analysis of each entrepreneur in a national industry in order to represent the thought processes going into the determination of market prices faces objective time and financial constraints. Conducting an experimental analysis of the behaviour of the particles at the centre of the Sun is not physically feasible due to the extreme temperatures and the fragility of available equipment. Observational investigation of the existence of life on the other 'side' of the universe is

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Parental authority will soon cut across the child's natural extension of the concept and insist that really the object is not a hat but a lid. A socially sustained boundary is drawn across the flow of the psychological tendency. [...] It should be possible to transfer this perspective to the data in Lakatos's example (Bloor 1976, 139).

not feasible due to the extreme distance and the limited lifetimes of human beings.[14]

Finally, there is the seemingly most philosophically contentious case of *worldly* constraints. Here we need not refer to 'the world in-itself' and thereby fall into basic problems associated with foundationalist epistemology.[15] We may instead refer to 'the world-under-description', where for us, what is observed and posited to exist must come under a human-made description of some kind (either explicitly theoretical or composed of 'everyday' concepts). The descriptions are conventional, historically contingent, and can change in part because of social forces, but also, given a set of stipulated descriptions, there are objective limits on what can be said about the 'real objects' which are distinct from those descriptions. For example, given a definition of unemployment and a means of measuring unemployment so described, it is not possible for measured unemployment to increase unless there is a change in the world (the real object) under that description.

Further, how might descriptive inadequacy or failure be construed? Take a posited unobserved theoretical entity such as, say, natural unemployment. Again, this expresses something about the world-under-*this*-description, but given the (conventional) 'theoretical' description and given what it implies under some set of conditions, if under the relevant descriptions it fails to predict what it claims to be able to, we

[14] It is noted that Bloor alludes to the requirement that a convention must have "practical utility" and be "perceived to have utility for the group of users" (Bloor 1976, 37-38; 2007, 215). Might this not cover what is being pointed out in the above examples? The problem here is that it is by no means clear that this requirement of Bloor's is supposed to be understood as independent of other social conventions—after all, such utility is characterised as "perceived" by "the group". Such 'perception' seems best glossed as 'interpreted' or 'regarded' rather than, say, 'directly observed', and by the lights of the SP, interpretations are themselves the function of pre-existing social conventions (rather than being individually subjective). In short, what is to count as 'being useful' and even what 'useful' means is itself subject to negotiable "decisions" by a community. This being so, it is suggested that Bloor's references to "practical utility" are not of the objective sort discussed above.

[15] The most basic problem was first articulated by the Sceptics in antiquity and has haunted (and motivated) epistemology ever since. Following Suchting (1986), the problem can be presented as follows. Take the necessary and sufficient conditions of 'knowledge' to be justified true belief. In order to establish a guaranteed relation of correspondence between a knowing subject and a known object, we require a criterion of justification. In order to ensure that this criterion is indeed correct—that it does the job of providing warrant—it too must be justified. Now, this leads us either to an infinite regress (that is, an unending list of different justificatory criteria), to circularity (that is, the original criterion is said to be justified by itself), or to dogmatism (that is, a criterion is ultimately merely stipulated as an intuitively self-evident foundation). None of these options satisfy the conditions of the original conception of knowledge. Thus, there is no knowledge so defined.

may be inclined to regard this as an inadequate description for our purposes and may be thereby motivated to develop or choose a new one. In these cases, the constraints are 'worldly' ones, despite being inextricably bound up with a conventional theoretical description. Indeed, it is because we seek to operate with a '*world*-under-description' that the *worldly* constraint *is* an objective constraint.[16]

THE NOMINALIST-ESSENTIALIST COUPLE

By focusing on the SP's finitism and by offering an alternative approach to meaning, Tyfield implicitly invokes an ancient debate, namely that between essentialism and nominalism.

Briefly, the nominalist approach holds that universals amount to nothing more than words used to group things. Particular entities can be grouped together into sets/classes ultimately by the sheer application of a word to particular individuals. The cause of the grouping-together is a secondary, but not unimportant, matter—it could be perceived or imagined resemblances, habits of the mind, social conventions of a community, religious decree, or whatever. For the nominalism

[16] It is noted that Bloor seeks to tone down concerns that the SP entails some kind of idealism—that is, SP does not deny the existence of the material world and so acknowledges objective worldly constraints. He states:

> No consistent sociology could ever present knowledge as a fantasy unconnected with men's experiences of the material world around him [...]. The whole edifice of sociology presumes that men can systematically respond to the world through their experience, that is, through their causal interaction with it. Materialism and the reliability of sense experience are thus presupposed by the sociology of knowledge and no retreat from these assumptions is permissible (Bloor 1976, 29).

Yet when it comes to saying what materialism amounts to, how it is 'cashed out' by the SP, we find it reduces to an assumption, a schema, an abstract idea, a presupposition:

> All our thinking instinctively *assumes* that we exist within a common external environment that has a determinate structure. [...] Opinions vary about its responsiveness to our thoughts and actions, but in practice the existence of an external world-order is never doubted. It is *assumed* to be the cause of our experience, and the common reference of our discourse. [...] [It is the] ultimate *schema* with which we think. [...] [W]hat is needed to make sense of affirmation [of some truth-claim] is the instinctive but purely *abstract idea* that the world stands somehow or other, that there are states of affairs which can be talked about. This is what is provided by the *schema of ideas* that I have called the materialist *presupposition* of our thinking (Bloor 1976, 36, emphasis added).

Historically, such an account of what 'the material world' amounts to is identified with idealism—either subjectively (e.g., Berkeley) or in some sense objectively (e.g., Kant or Hegel). Indeed, Michael Devitt (1997, chapter 13) argues (negatively) that social constructivism draws upon an idealist Kantian heritage, while Steven Vogel (1996) argues (positively) for a Hegelian-inspired social constructivism.

associated with the SP, a socially caused rule R over term T determines the (conventional) use of T for some x's, which in turn determines the (conventional) meaning of T over those x's. By extension, x_i is T (meaning) due to R over T (use) for a set that includes x_i. The emergence of new R's, especially by a change in social forces, generates new meanings.

I suggest that the underlying concern about the nominalist approach is that because definition and classification are, logically speaking, ultimately arbitrary, this would seem to render critical interrogation of rival concepts overly contingent and fragile, opening up the possibility of a Thrasymachean 'might is right' world of meaning-formation and change. If the nominalist were to argue that there can be conventional principles of critical judgement (see Bloor 1976, 38), the reply back can be that: this is ultimately arbitrary too, and would hardly engender great confidence if it were the powers-that-be who were to impose their definitions of what counts as 'legitimate criticism'. It is this kind of concern, I speculate, that underlies the motivation for an independent and impartial way of forming concepts that leads to the advocacy of essentialism.

The essentialist approach to definition and classification holds that universals are properties of entities and that a class (kind) of entities is intensionally defined as all those entities that possess a certain essential property or set of properties (either *ante rem* or *in re*). The meaning of a term is determinate because the essential properties of things, and so classes/kinds, are determinate. So, the meaning of a particular naming-word is given by the class into which a named thing falls, and the class into which it falls is determined by the thing's essential properties. For the essentialism associated with transcendental realism, the structural properties P of all x's determine the (real) meaning of T, which in turn determines the (correct) use of T over all x's. By intension, x_i is T (use) due to x_i possessing P (meaning). The discovery of new P's generate new meanings.

Ironically, the essentialist approach carries with it a problem similar to the one identified with nominalism. It can be brought out by the following question: if there are two or more competing essentialist definitions, how are we to decide which is the 'true' one? Which one should we use? Suppose we have a thing, x, that has structural properties P_1, P_2, …, P_n. P_1 of x, under the right conditions, manifests event E_1; P_2, under the right conditions, manifests E_2; and P_n manifests event E_n. Say that E_1 is of greatest interest to person A, so, assuming scientific investigation works perfectly, A discovers that the "essence" of x is P_1 and thus announces that the "real definition" of x is a statement about P_1. Meanwhile, say that E_2 is of greatest interest to person B, so assuming scientific investigation works perfectly, B discovers that the "essence" of x is P_2 and thus announces that the "real definition" of x is a statement about P_2. And so on up to n. What then is the "real definition" of x? Is it a statement entailing P_1 which ignores $P_{2, \ldots, n}$? Or a statement entailing P_2 which ignores $P_{1, 3, \ldots, n}$?, and so forth. At the analytical level—that is, taking a 'view from nowhere', beyond the grasp of particular social conventions, psychological states, and the like—the particular "real definition" would be an arbitrarily chosen one. Or in other words, with nothing left but logic to guide us, we cannot but choose arbitrarily. And so we return to the underlying concern identified with nominalism to which essentialism was supposed to offer a solution.[17]

We can think of the versions of essentialist and nominalist approaches to meaning examined in this paper as taking diametrically opposed positions. The proponents of each, by virtue of the radical incompatibility of the two positions, remain locked in an unending struggle: each talking past the other, each claiming the other does not really understand or treat fairly their own position. By taking the meaning of a naming-term as an attempt to capture some aspect of the world-under-description, the realist side emphasises the *world*-under-description, and thus meaning-intensionality, where ontologically grounded meaning determines use. In contrast, the SP side focuses its attention on the world-under-*description*, and thus meaning-extensionality, where socially grounded use determines meaning. Each emphasises something important and necessary to the understanding of knowledge-claims, and each, when overplaying its hand, denies what is correct in the other. Ironically, it is the over-emphasis on either the 'world' or the 'description' aspect of the 'world-under-description', which ensures that each makes distinctively different contributions to the account of the formation, change, and choice of meanings and knowledge-claims, and ironically, is also what prevents either from

[17] A proponent of transcendental realism might object that this misrepresents the situation, because it is not merely properties of x but the "constitutional or intrinsic structure" of x that defines x as a member of a natural kind (an ontological class). This will not do however, since, I submit, the very term "structure" functions as a place-holder for the selection of a limited number of apparently related properties.

progressing to an acknowledgment and incorporation of the other's valuable insights.

CONCLUSION

Critique of the SP is not without its merits insofar as the SP does not give sufficient weight to non-conventional discursive and non-discursive constraints on denotation and epistemic practices. Nonetheless, the realist criticism of SP, focusing on the intensionality of meaning, has its own basic problems. Furthermore, each 'side' of this debate offers important insights into meaning-formation and change. However, since each is wedded to different and incompatible theories of meaning, each is unable to acknowledge the importance of the opposing critique. What is required, I suggest, is an approach to meaning that cuts a path between these two positions, retaining their respective strengths and abandoning their weaknesses. This task remains to be completed. While it remains incomplete, the advocates of each will remain at loggerheads, trapped in an incommensurable *faux* debate, where "one bare assurance is worth just as much as another" (Hegel 1977, §76, 49).

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Science and social control: the institutionalist movement in American economics, 1918-1947

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Abstract: This paper deals with the concepts of science and social control to be found within interwar institutional economics. It is argued that these were central parts of the institutionalist approach to economics as the key participants in the movement defined it. For institutionalists, science was defined as empirical, investigational, experimental, and instrumental. Social control was defined in terms of the development of new instruments for the control of business to supplement the market mechanism. The concepts of science and social control were joined via John Dewey's pragmatic and instrumental philosophy. These ideas provided important links to the ideals of foundations, such as Rockefeller, and thus to access to research funding. Institutionalist concepts of science and social control were, however, displaced after World War II by Keynesian policy and positivist ideas of scientific methodology.

Keywords: institutional economics, science, social control, NBER, Brookings, SSRC, Dewey, instrumental philosophy

JEL Classification: B15, B25, B40, B52

I have just completed a book manuscript with a similar title as this paper. The book attempts to pull together the research I have done over the last 12 years on the history of the institutionalist movement, focusing on the period between the two World Wars. A great deal of this research was archival in nature and has appeared, for the most part, as a series of case studies of interwar institutionalism as found in the careers of particular individuals, or as expressed in particular university departments, programs, or research institutes. What the book tries to do is to knit this material together into a narrative account of the institutionalist movement, and, in the process, provide a picture, both in

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general and in particular, of the nature of the movement and its trajectory over time. What I want to do in this article is to focus in on one of the key points I make about the nature of interwar institutionalism, and to make a number of arguments regarding its significance and ramifications.

DEFINITIONS OF INSTITUTIONALISM

The issue I want to raise relates to how the institutionalist movement is defined. There are a large number of definitions of institutionalism in the literature, but almost all of these are based on identifying mark(s) suggested by later commentators. These include evolutionism, Darwinism, recognition of power relationships, methodological holism, support of planning, the ceremonial/instrumental dichotomy, and, of course, dissent from orthodoxy. In my own quest for a definition what I did was to begin from an examination of how the group who initially identified with institutionalism defined it themselves. This raises the question of who exactly constituted this group.

Institutionalism is often identified primarily with Thorstein Veblen, or with a founding triumvirate of Thorstein Veblen, Wesley Mitchell, and John R. Commons. Neither of these views stands up to closer examination. When one looks at the group of people most closely involved with the development and promotion of the idea of an identifiable "institutional approach" to economics, the names that come up are Walton Hamilton, J. M. Clark, Walter W. Stewart, Wesley Mitchell, and Harold Moulton, with Hamilton, Clark, and Mitchell the most important of these. Veblen is in the picture but mainly as a source of ideas and not as a prime mover. Commons is not in the picture at all until a few years later (Rutherford 2000).

The term "institutional economics" seems to have been invented around 1916 by those influenced by Thorstein Veblen (either by Robert Hoxie or Max Handman, depending on which story one chooses to believe). The first major use of the term in the literature was by Walton Hamilton at an American Economic Association (AEA) conference session in 1918 (Hamilton 1919). This session also included J. M. Clark, Walter Stewart, and William Ogburn, and its planning had involved Harold Moulton, who had discussed the session with Veblen and Mitchell. In my book I take this AEA session as the founding moment of the institutionalist movement and the group outlined above as the founding group. So, the question, now, is how they conceived of what they were attempting to create.

To answer this question one can look at Hamilton's paper "The institutional approach to economic theory", which was intended as a manifesto, the other papers in the same session, particularly Clark's, Stewart's remarks as session Chairman, and a series of papers written by Hamilton, Clark, and Mitchell in the couple of years leading up to the session.[1] These pieces of work do give a pretty clear idea of what the founders of institutionalism thought they were about.

Institutional economics was presented as an approach that would (i) focus on institutions, (ii) be concerned with "process", (iii) connect with recent work in related disciplines, (iv) utilize more "scientific" methods, and (v) relate to "the problem of control". Each of these requires some elaboration. Institutions are taken as central as it is institutions that both constrain and mold human behavior. Economic behavior is in large part determined by institutions. The reference to "process" implies an understanding that institutions are not static but in a process of change, both internal change and changes brought about by external developments. In Hamilton's words, institutions "refuse to retain a definite content", and this is true of particular institutions and of the "complex of institutions which make up the economic order" (Hamilton 1919, 315). This reference to process does not imply acceptance of any particular theory of institutional change, although the Veblenian idea that institutions lag behind material and technological developments was quite widely adopted. The interest in related disciplines was expressed primarily in terms of an interest in connecting institutional economics to a foundation in a "modern" psychology, but also involved an interest in connecting to recent work in sociology and law. The concern with proper scientific methods was a concern to make economics more empirical and investigational, and to avoid the speculative and untestable nature of much orthodox theorizing. For institutionalists, science meant being concerned with observation and measurement, avoiding unrealistic assumptions, and paying attention to the results of current research in other disciplines.

Finally, that economics should be relevant to the "modern problem of control" became a very central part of the institutionalist creed. There are a number of meanings attached to this. In Hamilton's own paper it is linked to an economics that does not deal in hypothetical worlds, but

[1] These pieces of work are discussed in Rutherford 2000.

which concerns itself with "gathering facts and formulating principles necessary to an intelligent handling" of contemporary economic problems (Hamilton 1919, 313). Problems such as labor unrest, business cycles, unemployment, poverty, externalities of various kinds, monopoly, manipulation of consumer wants, sharp practice, resource depletion, and waste and inefficiency, were all attributed to a failure of markets, or "pecuniary institutions" more generally, to control or direct economic activity in a manner consistent with the public interest.

The notion of an economics "relevant to the problem of control" recurs over and over. This idea also relates to the focus on institutions, as if economics is to be relevant to the problem of control it "must relate to the changeable elements in life and the agencies through which they are to be directed" (Hamilton 1919, 313). This involves an understanding of economic institutions as social constructions that are capable of change and of being changed, rather than as natural and immutable. Control is to be exercised though the modification of institutional arrangements. This requires detailed knowledge of institutional arrangements, their interrelations, and of the ramifications of any proposed changes. The aim of "social control", therefore, also relates to the need for a properly scientific approach.

In my view, the particular combination of the ideals of science and social control lay at the heart of institutionalism's early appeal and success. In Hamilton's words, what institutionalism offered was "an invitation to detailed study" and participation in "the intelligent direction of social change" (Hamilton n.d.a; Hamilton 1926). As stated by Dorothy Ross, "what fuelled the institutionalist ambition was an overflow of realism and new liberal idealism that could not be contained by neoclassical practice" (Ross 1991, 411). What the rest of this paper will do is examine in detail the institutionalist conceptions of science and social control, and relate them to some of the reasons for institutionalism's successes in the 1920s and 1930s, and its relative decline in the post World War II period.

INSTITUTIONALISM AND "SCIENCE"

The idea of science contained within the literature of interwar institutionalism can be illuminated in more detail by considering the writing on this subject by J. M. Clark, Lionel Edie, Wesley Mitchell, and by many of the contributors to Rexford Tugwell's 1924 volume *The* *trend of economics* (including, George Soule, Tugwell, A. B. Wolfe, and F. C. Mills).[2]

J. M. Clark's notion of what constitutes science and a scientific economics is of particular interest as his own work was far from purely descriptive and he made a number of important theoretical and conceptual contributions. Clark's views of the required procedures in economics were expressed as follows:

> Economics must come into closer touch with facts and embrace broader ranges of data than "orthodox" economics has hitherto done. It must establish touch with these data, either by becoming more inductive, or by much verification of results, or by taking over the accredited results of specialists in other fields, notably psychology, anthropology, jurisprudence and history. Thus the whole modern movement may be interpreted as a demand for a procedure which appears more adequately scientific (Clark 1927, 221).

Clark argued for an economics "based on a foundation of terms, conceptions, standards of measurement, and assumptions which is sufficiently realistic, comprehensive, and unbiased" to provide a basis for the analysis and discussion of practical issues (Clark 1919, 280). Relevance to practical issues, accuracy of data, and comprehensiveness, in the sense of not excluding any evidence relevant to the problem at hand, were the characteristics of a scientific approach to economics that Clark frequently stressed (Clark 1971 [1924], 74). He certainly thought of theory as playing a key role, but he saw the aim of theorizing as that of forming hypotheses "grounded in experience" for further study and empirical test, rather than the production of a highly abstract system of laws. Hypotheses must therefore be formulated in terms that allow for empirical verification or refutation (Clark 1971 [1924], 76).

As for the relationship between institutionalism and science, Clark argued that the term institutional economics is a term "used by a group of the younger American economists to define a point of view—one might almost make it coextensive with the scientific point of view—in economic study". This point of view "sets up the ideal of studying the interrelations of business and other social institutions as they are and not through the medium of any simplified abstractions such as are

[2] This section is based on Rutherford 1999. See also Yonay 1998.

employed by classical, static, and marginal economics" (Clark 1927, 271).

Lionel Edie can be found saying not dissimilar things. He describes institutional economics as "an extension of scientific method in economics", with a special emphasis on the use of recent work in sociology and social psychology to replace the assumption of "independent individual rationality", on the role of empirical investigations of various kinds to verify or disprove theories, modify theories, or suggest new theories "pertinent to the problems confronting us" (Edie 1927, 407-410). In his slightly earlier survey of institutionalist research entitled *Economics, principles and problems* (1926), Edie outlines the main characteristics of the "new approach" as including the influence of newer historical and anthropological research and the use of psychological presuppositions in line with modern psychology, the rejection of the notion of immutable natural laws and a substitution of a view of economic conduct as governed by institutions, the use of quantitative methods to supplement qualitative, a view of economic generalizations as tentative, and of the nature of hypotheses "to be tested by experimental and statistical science" (Edie 1926, viii).

Many of the essays in the Tugwell volume are replete with the language of "science" (Yonay 1998; Rutherford 1999). George Soule contrasts the confidence that is given to scientific knowledge in the realms of physics and chemistry with the lack of authoritative "tested knowledge" in the area of economics. Classical economics moved too quickly from induction to general conclusion, they "improvised their psychology" and subsequently lost touch with the growth of "scientific psychology" based on "experimental method and quantitative testing". All of this led to the "building up of bodies of economic doctrine which more resembled closed systems of metaphysics than an account of the real world" (Soule 1971 [1924], 359-361). Soule detects, however, a "rapid growth toward maturity" to be found in the desire to "make the science practically useful", accompanied by greater availability of data and the use of quantitative research and statistical methods (1971 [1924], 364).

At Columbia, Tugwell attended lectures given by John Dewey, out of which he developed his idea of "experimental" economics (Tugwell 1971 [1924]; 1982, 157). Tugwell argues that the "assurance of rightness in science" is to be found in the replication of experimental results. In this vein he discusses Newton and Galileo. In Tugwell's words "it is sometimes more, sometimes less, difficult to isolate and to demonstrate by experiment the bits of truth that scientists discover; but nothing is accepted as truth unless it can be so demonstrated" (Tugwell 1971 [1924], 386). Social scientists, according to Tugwell, see themselves in "direct line of descent" from natural scientists, the only difference being that the conclusions of social science having to "meet the test of application in a complex going system immediately". Social science must try to "isolate its problems and to devise and use special tools for dispassionate verification" (Tugwell 1971 [1924], 387). These tools include the efforts of specialized research organizations and the use of quantitative and statistical methods.

Tugwell also argues "the truth must be useful; and if science does not help to solve a problem it cannot reach out toward truth" (Tugwell 1971 [1924], 387). This idea allows Tugwell to argue that natural laws in the physical sciences have a different status from the so-called laws of classical economics. Tugwell seems to regard natural laws, even the natural laws of physics, as simply useful generalizations or hypotheses and not as ultimate. However, in the physical sciences natural laws such as the law of gravitation have proven themselves in "innumerable experiments" and in problem solving, while the laws of classical and neoclassical economics have not. Supposed economic laws are often little more than an embodiment of ideology or an expression of dialectical dilettantism (Tugwell 1971 [1924], 393). In Tugwell's words:

> Natural law has lost its force in analogous application because so many times events have disproved its premises. There has been a drift toward the substitution of consequences for premises in the search for truth in all fields [...]. Nothing can be taken as ultimate any more. And these facts are the consequences. Theory must have reference to them if it is to be useful (Tugwell 1971 [1924], 394-395).

A. B. Wolfe remarks on the differences of viewpoint between several of the "younger men" but goes on to state that they all "hold that economics ought to be scientific". Wolfe ascribes this growing "demand for a realistic, inductively analytical, non-metaphysical, scientific economics" to the matter-of-fact spirit of the times and a "growing conviction that the older economic theory, whether classical, neo-classical, or marginalistic, is deficient in scientific quality" (Wolfe 1971 [1924], 447).

According to Wolfe, the main features of the scientific method are (1) unbiased selection of factual data, without "undue limitation of range", and freedom from personal or class interest; (2) hypotheses seen as devices in a trial and error method; (3) all generalizations regarded as tentative; and (4) deductive inferences to be tested by "repeated appeal to experience", and long chains of deductive reasoning to be avoided (Wolfe 1971 [1924], 451). Clearly, Wolfe regarded orthodox economics as seriously deficient in all of these respects. Wolfe, however, did not see science as solely a quest for knowledge for its own sake, but also motivated by ethical ideals and normative standards. He held out the prospect of a "scientific ethics" based on behaviorist psychology and social psychology. A scientific understanding of human nature should "point the way to a fundamental, objectively scientific, ethical norm or ultimate end of life" (Wolfe 1971 [1924], 478).

A high proportion of the institutionalist discussions concerning a scientific economics in the inter-war period contained at least some mention of the importance of quantitative work. In this, the work of Wesley Mitchell, and of those who followed his lead, such as Walter Stewart and F. C. Mills, was central. Mitchell reacted particularly strongly against the speculative, normatively biased, untested, and often untestable nature of existing economic theory. He argued that the social sciences were held in low repute and, given the lack of exactness and certainty of their conclusions, deservedly so. The solution was to imitate the natural sciences in their careful and painstaking work of observation and experiment, systematic analysis, and desire to eliminate normative biases and achieve objective results (Ginzberg 1997; Biddle 1998). Mitchell put the matter as follows:

> There seemed to be one way of making real progress, slow, very slow, but tolerably sure. That was the way of natural science [...]. Not the Darwinian type of speculation which was then so much in the ascendant—that was another piece of theology. But chemistry and physics. They had been built up not in grand systems like soap bubbles; but by the patient processes of observation and testing—always critical testing—of the relations between the working hypotheses and the processes observed. There was plenty of need for rigorous thinking, indeed of thinking more precise than Ricardo achieved; but the place for it was inside the investigation, so to speak—the place that mathematics occupied in physics as an indispensable tool. The problems one could really do something with in economics were problems in which speculation could be controlled (Mitchell 1936 [1928], 413).

Mitchell saw quantitative and statistical work combined with careful policy experiments as the closest approach to the methods of the natural sciences possible in economics. In his attitudes towards science Mitchell was clearly influenced by John Dewey's instrumental approach. Quantitative work in economics required all the trappings of the natural sciences—a "statistical laboratory", research assistants and fieldworkers (Mitchell 1925). This, of course, was the ideal of scientific research that Mitchell embodied in the National Bureau of Economic Research (NBER). Mitchell also talked of experimentation, at least in the form of experiments on group behavior. He recognized some of the difficulties of attempting to apply experimental methods to economics, but argued that they could be mitigated by more reliance on "statistical considerations and precautions" (Mitchell 1925, 31).[3]

A similar emphasis on quantitative and statistical approaches can be found in F. C. Mills's essay "On measurement in economics", again in the Tugwell volume (Mills 1971 [1924]). Mills quotes Lord Kelvin to the effect that without measurement and numerical expression there cannot be a *science*, and quotes both James Clerk Maxwell and Karl Pearson on the statistical view of nature. For Mills all social relationships do not hold universally or with absolute certainty. In the statistical approach "we forego the searching for sole causes and, instead, seek to measure the degree of association found in experience" (Mills 1971 [1924], 43-44). Furthermore, such relationships are not seen as "final formulations of truth" but in a process of development towards higher degrees of probability. Interestingly, Mills finds the statistical conception "in complete agreement with the views of philosophers of the pragmatic school" such as Dewey. Mills quotes with approval Dewey's remarks that generalizations are "not fixed rules [...] but instrumentalities for [...] investigation" and are "hypotheses to be tested and revised by their further working" (Mills 1971 [1924], 45-46). Mills also links a quantitative and statistical economics to the more effective solution of economic problems. Practical problems will be more readily solved "by quantitative study of specific conditions than by the attempt to apply vague generalizations of doubtful validity" (Mills 1971 [1924], 70).

[3] For Mitchell, quantitative work was linked to the institutional approach. He rejected the idea that quantitative work would simply complement orthodox theory, as that theory was not stated in terms amenable to statistical attack. Quantitative work would lead to a focus on the patterns of mass behavior that, for Mitchell, were clearly of institutional origin. See Rutherford 1987.

Similarly, Stewart argued that "an adequate analysis of many of our problems can be made only by a union of the statistical method and the institutional approach" (Stewart 1919, 319).

All of this rhetoric of science stands in marked contrast to the neoclassical literature of the time that tended to stress the limited nature of the applicability of natural science methods to economics.[4] The notion of science apparent in this body of institutionalist writing clearly borrows heavily from John Dewey. Mitchell absorbed Dewey's teaching at Chicago, Hamilton learnt Dewey through the teaching of Charles H. Cooley at Michigan, Stewart went to Columbia for a term to study with Dewey, Mills and Tugwell took Dewey's courses at Columbia. This pragmatic and instrumentalist view of science was broadly empirical with a strong emphasis both on realism of assumptions and on testing of empirical implications.

Testing was not done using econometric techniques, but the implications of theories were compared with the results of empirical and statistical investigations in a variety of less formal ways. Mitchell, in his statistical examinations of the course of business cycles, frequently remarked on the consistency or inconsistency of his findings with various business cycle theories (Mitchell 1913). Morris Copeland tested different views of the quantity theory (pro and con) by drawing out the implications for the leads and lags one would expect to find and then examining the data (Copeland 1929). Hamilton's work on the bituminous coal industry found many buyers and sellers and a relatively homogeneous product, but an industry characterized by "chaos" and not by a stable competitive equilibrium (Hamilton and Wright 1925). Clark examined the effect of overhead costs on the pricing policy of firms, concluding that they resulted in departures from the standard models, and drove such observed phenomena as price discrimination and cutthroat pricing (Clark 1923). Examples such as these could be multiplied.

Moreover, institutionalists saw theories as instruments for both investigation and control, that is, for the solution of both scientific and practical problems. Theories are tested, ultimately, by the results of

[4] See Knight 1935 [1924], and Viner 1928, as examples. Knight compared neoclassical economics to "theoretical physics". Henry Moore and his student Henry Schultz attempted to provide neoclassical theory with an empirical component, but with limited success. Within the institutionalist literature, neoclassical theory was seen as overly abstract and "speculative", and frequently untestable or untested. It is also worth noting that Veblen was frequently criticized for similar failings. See Rutherford 1999.

their usefulness as instruments not just for investigation but also as instruments for social control.

INSTITUTIONALISM AND "SOCIAL CONTROL"

The term "social control" originated in sociology in the 1901 book by Edward A. Ross of that name (Ross 1901). Ross discusses a large number of ways in which societies control the behavior of their members. Social control is the way in which social order is established and maintained. Ross distinguishes between an unplanned spontaneous "natural order" based on a set of social sentiments including sympathy, sociability, sense of justice, and resentment, and a planned and conscious "social order". As societies become more complex the natural order is replaced by a social order, maintained by social controls including public opinion, belief, social institutions, and laws. Social control includes both external incentives and sanctions and the internalization of social norms and values. Thus, the instruments of social control can be either ethical (moral) or political.

Social change, and, in particular, the rapid changes occurring in America with rapid industrialization, required new forms of social control. As has been argued, Ross's *Social control* is a book that is "manifestly interventionist and anti laissez-faire in its tenor, seeking to contribute to the solution of social problems in order to sustain the progressive direction of social change" (Weinberg, Hinkle, and Hinkle 1969, viii). Franklin Giddings in a review of Ross's book states social control is intended; it "springs from a self-conscious knowledge of factors and tendencies in economic life, and proceeds according to plan" (Giddings 1902). This idea of social control profoundly influenced not only institutional economists but also other social scientists, especially those in sociology and political science.

There is a vast institutionalist literature that utilizes the concept and rhetoric of social control. What I will do here is to examine a small part of this literature: some of Hamilton's writings, J. M. Clark's *Social control of business* (1926), Dexter Keezer and Stacy May's *The public control of business* (1930), and the entry "Social control" in the *Encyclopaedia of the social sciences* written by Helen Everett (1931).[5] Keezer, May, and

[5] Other notable examples are to be found in Hamilton and May's *The control of wages* (1968 [1923]), Sumner Slichter's "The organization and control of economic activity" (1971 [1924]), and Leo Wolman's "The frontiers of control" (1927). A number of Mitchell's essays have a similar theme of "intelligent guidance" (Mitchell 1950 [1936]).

Everett were all students of Walton Hamilton. Although written last we will begin with Everett's entry.

Everett begins by discussing Ross's *Social control* and notes the existence of both a wider and a narrower sense of the term. The wide sense of social control involves "exploring the forces by which the group molds and shapes the individual"; the narrow sense of it is that of the "active intelligent guidance of social processes" or "the consciously planned guidance of economic processes". She continues:

> In America the institutionalist school of economics, whose outstanding figures are Thorstein Veblen, Wesley C. Mitchell and Walton H. Hamilton, has made important use of the concept of social control. Indeed it is perhaps their central organizing principle. The emphasis of the institutionalists is that economic arrangements are man made and susceptible to almost limitless variation. While for most economists the idea of control is like a mechanical bit of apparatus, for the institutionalists it is more of the nature of the guiding formula itself (Everett 1931, 345).

Everett also makes an explicit connection to the work of John Dewey, quoting him saying that "we have attained [...] a certain feeling of confidence; a feeling that control of the main conditions of fortune is, to an appreciable degree, passing into our own hands" (Everett 1931, 348; Dewey 1929, 9).

Hamilton's central argument was that the existing system of social control was inadequate to cope with new economic conditions and problems, and that additional methods of social control had to be devised. Hamilton discusses the development of "modern industrialism" from the pre-capitalist manorial system, a process that included both the development of large scale methods of production, and a complex of institutional developments relating to property and contract, markets, the adoption of pecuniary goals and incentives, the corporate form of organization. This gave rise to what he calls the system of "business control", a system based on the instrumentalities of "the corporation, the pecuniary calculus, and profit making" (Hamilton n.d.b).

The key issue, of course, is the adequacy of this system of control. In the economics textbooks it is competition that is supposed to operate to reconcile the individual pursuit of pecuniary gain with community welfare, but Hamilton consistently argues that competitive theory and the policies that it suggests apply only to an economy of "petty trade". For Hamilton "the fundamental issue stands out in clear cut relief":

there is a disconnect between the technology of industry and the form of its control. “An economic order in which the productive processes belong to big business and the arrangements for its control to petty trade cannot abide”. The task is to “devise a scheme adequate to the direction of great industry. In a world of change a society cannot live on a wisdom borrowed from our fathers” (Hamilton 1932, 593). Hamilton’s work on health and the coal industry indicate his willingness to consider new institutional arrangements designed to overcome the particular problems he identified in each case (Rutherford 2005a). He developed a keen interest in law and economics and in what he called “the judicial control of industry”, a theme carried on by some of his students.

Keezer and May provide a critical analysis of the attempt to provide for the control of business through the instrumentalities of the anti-trust laws, the regulation of enterprises “affected with a public interest”, and government ownership and operation. They argue that the anti-trust laws have been ineffective in maintaining competition primarily due to judicial decisions finding that the mere size of an enterprise does not constitute a violation. Similarly, the courts have narrowed the definition of enterprises affected with a public interest to the point where public interest laws have been rendered ineffective in all but a very restricted number of circumstances.

Concern with this doctrine of “affectation with a public interest” and its history before the courts was something also discussed at length by Hamilton (1930), Tugwell (1968 [1922]), and Clark (1926). In the case of public utility regulation Keezer and May find that the court’s definition of the valuation of the enterprise upon which a “fair return” is to be calculated had obstructed the possibility of rate regulation in the public interest.[6] In contrast the barriers to public enterprises, even when established in direct competition to private concerns, are not judicial but political. Public enterprise then, is a “potentially effective form of government control” of business.

Keezer and May argue that an effective system of public control should be able to use all three of the instruments outlined above in much less restrictive ways, that regulators should have access to relevant information, and that the available instruments of control be used flexibly and experimentally. All of this would, however, require

[6] The issue of how the courts decided on a fair rate of return was much discussed by those involved in public utility regulation, notably by Bonbright and Hale at Columbia. See Rutherford 2004.

substantial changes to the then existing judicial interpretations of private property rights (Keezer and May 1930, 230-254).

Clark's *Social control of business* is one of the paradigmatic works of American institutionalism, and the most broad-ranging treatment of the social control issue to be found in the institutionalist literature. Clark's fundamental argument is that it is clear that "industry is essentially a matter of public concern, and that the stake which the public has in its processes is not adequately protected by the safeguards which individualism affords" (Clark 1926, 50). Society, therefore, has ample grounds for interfering with business. Business will, of course, resist controls, and such resistance is not likely to diminish.

The available instruments of control discussed by Clark are the legal framework, particularly laws of private property, contract, and bankruptcy; competition; control by the state; the establishment of standards, for example standards of health and standards of living; and informal controls such as ethical norms or professional codes. The possible grounds for state intervention in the economy are wide. They include public defense, protection of person and property, regulation of common property resources, controlling inheritance and bequest, raising public revenues, prevention or control of monopoly, maintaining the level of competition, protecting the individual where he is not competent to judge or lacks information, problems of agency, providing for victims of change or catastrophe, provision of a social minimum, economic guidance and consumer information, equality of opportunity, "unpaid costs of industry" or externalities, "inappropriable services" or public goods, arms race types of competition and cases where the actions of individuals or firms neutralize each other wasting resources, unused capacity, interests of posterity, and other discrepancies between private and social costs. Clark then discusses various ways of "protecting consumers against exploitation" mostly focused on forms of regulation of prices, but including also public ownership and operation, and other ways of affecting market outcomes either directly or indirectly. "Social control must reckon with the forces of supply and demand, but does not stand helpless before them" (Clark 1926, 459).

It might be added here that the institutionalist interest in social control also extended to macroeconomic policy in the form of anti-cyclical public works programs and other methods to mitigate cycles and cyclical unemployment. There was an extensive pre-Keynesian institutionalist literature on depressions and unemployment, some based heavily on Mitchell's research program on business cycles, and some based on underconsumptionist ideas drawn mainly from J. A. Hobson (Rutherford and DesRoches 2008).

The theme of all of this is clear: existing methods of regulation and control of business in the public interest are inadequate and new forms are required. These new controls may take the form of regulation or of more direct government involvement in the economy. Examples of the types of social control promoted or pioneered by institutionalists include public utility regulation; the creation of regulatory commissions or an administrative approach to the approval or disapproval of business practices; labor legislation of various types, including the promotion of collective bargaining, labor mediation and arbitration; workmen's compensation programs, unemployment insurance, and social security; agricultural price support programs; improved representation of consumer interests; medical insurance programs; and the countercyclical "planning" of public works programs (Clark 1935).

These institutionalist ideals of scientific investigation and social control, and the emphasis they gave to the law as an instrument of social control, provided the basis for the close linkage between institutionalist economists, other progressive social scientists, and legal realists.[7] The connections between institutionalists and legal realists are particularly noteworthy, but are not surprising given that legal realism was also founded on the ideals of applying empirical scientific methods to the study of law, and the promotion of "pragmatic social reform through legislative change" (Fried 1998, 14).

SCIENCE, SOCIAL CONTROL, AND THE FOUNDATIONS

The institutionalist ideals of science and social control were shared not only by many other social scientists but also by many of those in charge at the major foundations. This congruence of ideas lay behind the willingness of foundations such as the Laura Spelman Rockefeller Memorial (LSRM) and, later, the Rockefeller Foundation to fund organizations such as the Social Science Research Council (SSRC), the Brookings Institution, and the National Bureau of Economic Research

[7] For a discussion of some interdisciplinary linkages between institutionalists and other social scientists and legal realists, see Rutherford 2004. For a discussion of legal realism and institutionalism, see Fried's (1998, 10-15) discussion of Robert Hale as an institutionalist and realist. Hale began in economics at Columbia but moved into the Law School. Columbia had a large contingent of both institutionalists and legal realists. Hamilton moved to Yale Law School in 1928, then the major center for legal realism.

(NBER). The major figures at the SSRC were Charles Merriam,[8] Mitchell, and Ogburn. The NBER was headed by Wesley Mitchell, and Brookings by Harold Moulton. All of these organizations were heavily institutionalist in orientation.

The most notable development in funding for the social sciences in the interwar period came initially from the program developed by Beardsley Ruml and his staff at the Laura Spelman Rockefeller Memorial Foundation (Bulmer and Bulmer 1981). In 1922, Ruml set out his views in a "general memorandum" that emphasized the importance of the development of social sciences, and, in particular, the "production of a body of substantiated and widely accepted generalizations as to human capacities and motives and as to the behavior of human beings as individuals and groups". The underlying purpose was the generation of social scientific knowledge that could be used for social improvement. Ruml argued that "all who work toward the general end of social welfare are embarrassed by the lack of that knowledge which the social sciences must provide", the situation being as if "physicians were practicing in the absence of the medical sciences" (Ruml 1922, 9-10).

In 1923 Ruml commissioned Lawrence K. Frank to carry out a review of social science research in universities and independent research organizations. Frank was an economist of institutionalist persuasion who had trained at Columbia with Wesley Mitchell. Frank's report deplored the lack of funding for properly scientific social science research, by which he meant work that was "investigational" or "experimental", terminology that reflected the influence of John Dewey. These scientific methods he contrasted with those of speculative theorizing and library based research, the dominance of which had resulted in both the "inertia" of the social sciences and its failure to separate itself from political partisanship (Frank 1923, 20-21). After completing his report Frank joined the staff of the Memorial.

Ruml and Frank came from social science backgrounds and both were concerned with advancing "basic" research that might contribute to the solution of social problems over the longer term. The LSRM supported a conception of social science requiring "not the reading of books and abstract thought" but "realistic" and methodical empirical and quantitative research (Bulmer and Bulmer 1981, 347-348). There was, then, a clear and quite explicit consensus between Ruml and Frank

[8] Merriam was a Chicago political scientist. He fully shared Mitchell's emphasis on quantitative social science. For a discussion of the SSRC see Fisher 1993.

at the LSRM and economists such as Wesley Mitchell, and others of similar mind, which worked through the NBER and other organizations to promote a particular concept of "scientific" economics; one that was associated with empirical and quantitative work directed to improved social control. In this manner the major foundations and institutional economists could, and did, form an alliance based on the shared values of an investigative science directed towards improved social control. As argued by Donald Fisher:

> What brought the social scientists and the foundations together was the concept of "social control". For sociologists, this concept had become the central theoretical thrust behind their attempts to investigate social problems [...]. Institutional economists like Mitchell used the concept as a means of linking together their efforts to improve upon the mechanisms of competition and the marketplace [...]. The theoretical and the practical merged as these social scientists and foundation officials [...] sought to use social scientific research to solve social problems and thereby increase the degree to which society was socially controlled (Fisher 1993, 58).

This congruence between the major foundations and institutional economics persisted even after the LSRM was merged into the Rockefeller Foundation and Ruml was replaced by Edmund Day and then by Joseph Willits. Both Day and Willits shared the institutionalist view of science and the goal of improved social control (Rutherford 2005b). This was not incidental to the success and position of institutional economics in the 1920s and 1930s.

This relationship was also expressed clearly by Wesley Mitchell. Mitchell was very concerned to keep scientific research separated from political partisanship, because he wanted to protect the NBER from any possible charges of political bias, but he fully accepted that the goal of research was improved social control. In 1936 Mitchell suggested the formation of a National Planning Board, a kind of federally funded NBER, to plan and organize the study of social problems. A planning organization could, "by throwing light upon the consequences that different lines of action would produce", contribute to the "attainment of a more rational scale of social values than now prevails among us" (Mitchell 1950 [1936], 135). The practical application of this point of view can be seen in the NBER involvement in studies such as *Recent economic changes in the United States* (1929), while Mitchell's linking of scientific investigation into consequences to the appraisal of "social values" is a viewpoint that clearly owes much to Dewey's philosophy.

SCIENCE, SOCIAL CONTROL, AND THE DECLINE OF INSTITUTIONALISM

As we move forward into the late 1930s and 1940s the position of institutional economics changes quite drastically. There are many elements to this changing situation but in the context of the theme developed here those of most importance involve the development of Keynesian economics and its related policy positions, and the widespread adoption within economics of some version of the positivist view of science.

It is sometimes claimed that institutionalism was simply swept away by the arrival of Keynesian economics. Such a picture is far from accurate (Rutherford and DesRoches 2008), but Keynesian economics did pose major challenges, particularly as it apparently provided an effective cure for unemployment through the use of the instrument of fiscal policy. For example, Clark willingly conceded that "certain central problems cannot be successfully handled without the use (which does not imply exclusive reliance) of the income-flow method of analysis of which Keynes's studies are the most prominent form" (Clark 1942, 9).

After World War II, the apparent success of Keynesian macro policy and its connection, both in the UK and in the US, to broader programs of economic reform of a progressive nature, allowed Keynesians to take over the claim to represent effective social control.[9] The volume *The new economics*, edited by Seymour Harris (Harris 1948) is dedicated "to those economists who, following the leadership of Lord Keynes, are endeavoring to make of economics a useful tool for the diagnosis and treatment of economic disease" (Harris 1948, v). Harris goes on:

> Keynes indeed had the Revelation [...] laissez-faire is outmoded; the excrescences of capitalism must be removed; government control of money, interest, savings, and investment is recommended; but individual liberties to choose occupations, to select goods for consumption, to make profits, should not be impaired (Harris 1948, 5).

Keynesianism also quickly became connected to an empirical component in the form of macro-econometric models. This was critical, as institutionalists could no longer claim to be the primary

[9] The objects of Keynesian social control were still narrower than in the institutionalist case.

representatives of empirical economics. Keynesian economics generated a very similar kind of appeal to that generated by institutionalism some decades earlier. Keynesianism seemed to offer exactly that promise of science and social control that institutionalism had held out in the 1920s and generated a very similar degree of excitement among the younger economists of the time.

As argued above, in the 1920s and 1930s it was institutionalists who made the stronger claims to "science". This situation radically reversed itself in the post-1945 period mainly due to the importation of various positivist ideas of science. Positivism in various forms was brought to the United States by W. V. Quine and Rudolph Carnap, and by the many *émigré* academics who arrived to escape fascism in Europe.[10] It is sometimes claimed that institutionalists (and particularly people such as Mitchell) adopted "positivist" ideas, but such claims require care. As Wade Hands has argued:

> Yes, pragmatism, like logical positivism, was a "scientific philosophy"; and, yes, both approaches promote the extension of scientific reasoning [...] and, yes, both are broadly "empirical" and concerned with "experience"; but the similarities essentially stop with these basic points. Dewey in particular had a very "latitudinarian" view of the experimental method of science [...] and never exhibited the positivist tendency to view "science" as a narrowly circumscribed endeavor. Dewey was both anti-epistemology and anti-foundationalist and certainly never shared the positivist goal of dictating the proper empirical foundation of all scientific knowledge. Perhaps most importantly, he considered the scientific form of life to be social, linked to democracy, and not a subject for armchair philosophizing about the ultimate character of knowledge (Hands 2004, 959).

In the post World War II period institutionalist claims to scientific status were strongly challenged both by the Cowles Commission (Koopmans 1947), and later by Chicago economists such as Milton Friedman (1953). Orthodox economists came very largely to adopt either the logical empiricism of Rudolf Carnap, Carl Hempel, and Ernest Nagel, or Friedman's positivist version of instrumentalism. Logical empiricism emphasizes the "hypothetico-deductive" nature of theories. Theories contain axioms and statements derived from them. The axioms "may refer to either observables or theoretical entities", and the "system is

[10] For discussion of the impact of *émigré* economists in America see Craver and Leijonhufvud 1987; Scherer 2000; Hagemann 2005; and Mongiovi 2005.

given empirical meaningfulness only when the system is given some empirical interpretation" via the translation of some of the theoretical statements into observational language (Caldwell 1982, 25). It is usually the "lower level" deduced consequences of a theory that will describe observables and that are subject to empirical verification. Friedman's version of instrumentalism is much less formal and simply focuses attention on the testing of a theory's predictions with no attention being given to the realism of assumptions. Both positions, however, provided a view of science that could counter institutionalist demands for realism. Both gave wide range to deductive theorizing with the emphasis only on the empirical testing (by verification or falsification) of some specific implications of the theoretical model. This gradually displaced the broader institutionalist concern with realism.[11]

Logical empiricism, in addition, claimed to be a general description of scientific procedure, applicable to both the natural and physical sciences, and it largely displaced pragmatism as the ruling philosophy of science in the United States. Once neoclassical and Keynesian theory had an empirical component, they could claim the mantel of science while at the same time accusing institutionalists of naïve empiricism. All of the criticism of institutionalism as descriptive, lacking theory, or anti-theoretical either explicitly or implicitly adopts one of these views of what constitutes a "scientific economics".

CONCLUSION

This paper argues that the ideas of "science" and "social control" were central ideas in the formation of institutional economics in the interwar period. This is not the usual presentation of institutionalism, but it is one that is in line with the way in which those most closely involved in the formation of the institutionalist movement defined it themselves. These ideas, perhaps more than any others, defined the movement and its central aims of critical investigation of the functioning of the economic order and its "intelligent guidance" in order to better meet the social or public interest. The rhetoric of science and social control is to be found everywhere in the institutionalist literature of the 1920s and 1930s. In the definition of science, in the idea of social control, and in the links between them, institutionalists drew heavily on the philosophy of John Dewey.

[11] This did not happen in all areas of the discipline at the same pace. Labor economics, for example, remained quite empirical and institutional for some time.

These ideas also provided the points of connection between the institutionalist movement, other social scientists and legal realists, and the foundations that provided financial support. These connections led directly to the ability of institutionalists to further their research agendas through organizations such as the NBER, Brookings, and the SSRC.

The decline of the position of institutionalism in the post 1945 period can also be connected to these central ideas, but in a different way. Keynes and Keynesian economics and the related programs of reform associated with the welfare state took away the particular institutionalist association with the rejection of laissez-faire and the promotion of programs of "social control". The arrival of positivistic ideas of science also displaced the pragmatic ideas embedded in institutionalism and provided new "scientific" justifications of the deductive and abstract methods rejected by institutionalists. The accepted idea of what constituted scientific economics changed dramatically and in directions damaging to the more empirical approaches associated with institutionalism. Institutionalists found themselves no longer able to claim to be more scientific or better at developing effective instruments for social control than their Keynesian or more orthodox competitors.

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Making economics more relevant: an interview with Geoffrey Hodgson

GEOFFREY M. HODGSON (Watford, England, 1946) is research professor at the University of Hertfordshire Business School, UK. He is editor-in-chief of the *Journal of Institutional Economics* and a member of the Academy of Social Sciences in the UK. He has published over 120 articles in academic journals and his books include *Economics and institutions* (1988), *Economics and evolution* (1993), *Economics and utopia* (1999), *How economics forgot history* (2001), *The evolution of institutional economics* (2004), and (with Thorbjørn Knudsen) *Darwin's conjecture: the search for general principles of social and economic evolution* (2010).

Professor Hodgson is widely known for his extensive work on institutional economics, and his numerous contributions to a broad variety of topics in heterodox economics and social theory. His current research focuses on the theoretical and methodological foundations of institutional and evolutionary economics. In particular he is interested in the application of Darwinian principles to socio-economic evolution, the conditions underlying increasing socio-economic complexity, and the impact of increasing complexity in capitalist development.

During a visit to Erasmus University Rotterdam in November 2009 Professor Hodgson granted EJPE the opportunity to discuss many of these issues extensively. The interview ranged widely over such topics as the relationship between institutional and neoclassical economics; the methodological challenges in institutional economics; the potential role of biological and evolutionary ideas in the social sciences; and the role of economics and economists in the recent economic crisis and how the profession should change.

EJPE'S NOTE: This interview was conducted by Clemens Hirsch, PhD candidate at the Erasmus Institute for Philosophy and Economics (EIPE), Erasmus University Rotterdam, and currently visiting researcher at the Department of Social and Moral Philosophy at the University of Helsinki.

EJPE: ***Professor Hodgson, you are one of the most well known contributors to contemporary institutional economics. However, your earlier career was quite different. Can you tell us how did you arrive at economics in general and how did you get interested in institutional economics in particular?***

GEOFFREY HODGSON: My first degree was in mathematics and philosophy at the University of Manchester. As an undergraduate in the 1960s, I became interested in left-wing ideas—like many others at that time. I got interested in Marxism in general and in Marxist economics in particular. Eventually, I found myself publishing a few articles in that area but they were actually critical of the technical aspects of Marxist economics. That was unusual: I was sympathetic to Marxism, but I was also a critic. My criticisms were based on the then popular framework of Piero Sraffa (1960) and I did some post-graduate work in that area.

A double shift occurred in my thinking in the late 1970s and early 1980s. I became politically disenchanted with Marxism, particularly because of the way that Marxists responded to the free-market arguments of Friedrich Hayek and others. In the Thatcher era their response was very weak, but just as importantly, I thought that the foundational principles of Marxist theory were at fault. I still think there is a lot of validity in Marxism, but I was searching for an alternative perspective. One problem that particularly alerted me was the lack of any developed theory of the human agent in Marxism. Instead, all the explanatory work is done by the social structure: Marxists examine the social structure and then place agents in their positions in that structure. At least in classical versions of Marxism, that is deemed sufficient to explain agent behaviour. I thought that was a major shortfall.

But my dissent was not simply with Marxism. It was also with other versions of heterodox economics at the time. In the 1960s and 1970s at Cambridge, and some other places in the UK and USA, the dominant heterodoxies were either Marxist economics or post-Keynesian economics. Institutionalism was less influential, and was then completely absent in the UK. I turned to the old institutionalism of Veblen in the early 1980s because it had more persuasive psychological underpinnings. I became interested in psychology and critiques of the standard rationality assumptions in economics. I was also influenced by Herbert Simon and by a number of other people, including Hayek and

G. L. S. Shackle. I started a long march that took me away from Marxism and—through Veblen—got me interested in evolutionary theory as well.

For my work on Marxist theory in the 1970s I had a brief international fame. But I walked away from that way of approaching economic analysis. For about ten years I was not invited anywhere. Things began to change after I published my book *Economics and institutions* in 1988.

So you were already interested in institutionalist issues before new institutionalism began to emerge in the mid 1970s?

Part of my critique of Marxism was also that it failed to take institutions sufficiently into account. In 1977 I published a book called *Socialism and parliamentary democracy*, which critiqued Marxism for failing to take the institutional importance of parliamentary democracy into account. Parliamentary democracy is normatively important. But in practical terms it is also an institution with which people have to engage in one way or another. And it is a source of political legitimacy. At the time many Marxists had a crude insurrectionary perspective, where every such institution had to be overturned. I argued against that. So I think there is an institutionalist thread going right back to my Marxist period.

You mentioned that institutions had been ignored—or not properly taken into account—in economics for a long period of time. Why do institutions matter in economics?

Institutions matter because there is no society or economy without institutions. If you define institutions—as many people do—as systems of rules for guiding human conduct, then everything we do is bounded by institutions. We are conversing in a language that has rules. If we do not follow those rules we reduce the probability of being understood. We have just been for lunch, there we follow rules: about appointments; about paying; about table manners. Often we do not think about it, we just follow the rules all the time. So, any social activity is permeated with rules and thus rule systems—institutions—are unavoidable.

New institutional economics today is a flourishing but also quite heterogeneous field of research. How would you draw the current boundaries of the field and how would you position your own work within those boundaries?

When the new institutional economics emerged in the current era with the publication of Oliver Williamson's 1975 book *Markets and hierarchies*, and several others around that time, it adopted quite a narrow project. This was to take individuals as given and then to try explain how institutions emerge. This struck me immediately as an incomplete story because, again, you have the problem of explaining individual preferences and dispositions. A shortcut is made in the familiar way, as in much of mainstream economics, to simply assume a preference function and not to explain where it comes from. That had already been my concern about other systems of thought, including Marxism. There was no explanation of individual psychology, individual agency, and so on. I reacted against the new institutionalists for that reason, although I was very sympathetic to their concern with real institutions. That was a big change in economics, because previously institutions had often been ignored. The concern with institutions, like the firm and the state, was an extremely important move. I also think that core concepts like transaction costs are important and real, and the logic of transaction cost arguments is powerful. I do not think it is the whole story, but I think it is an important part of the story about why firms exist.

It further became clear, particularly by the 1990s, that there were developments within the new institutionalism that offered a broadening agenda. New institutionalists such as Douglass North (1990; 1994) and Masahito Aoki (1990; 2001) were saying things that were much more consistent with my position. They talked of the need for explanations of institutional evolution and of individual preferences. North developed a theme, which is now very prominent in his work, about ideology and the role of ideas. He writes of the need to learn from cognitive science and psychology. That was exactly my agenda. So I perceived a convergence with North and this wing of the new institutionalism.

Today I would sum up the new institutionalism as very heterogeneous. I find myself comfortable with many aspects of it and I am very critical of other aspects, but I am happy to swim in that pond. The old institutionalism is also very heterogeneous. A project I started in the 1990s and finished a few years ago was my two-volume history of the old institutionalism: *How economics forgot history* (2001), and *The evolution of institutional economics* (2004). My research for these books, which took many years, reinforced my view that the old institutionalism was actually very heterogeneous.

Today I think there is enormous opportunity for the interchange of ideas and conversation between different currents. I describe my own position as an eclectic with a strong Veblenian preference, because I think Veblen had a theoretical system that—although underdeveloped—in many ways remains powerful today.

Do you think there are promising elements in old institutional economics, which have not been taken up by new institutional economics and should be given more attention?

Yes, several things. Psychology is already having an impact in the new institutionalism. But I would like to see that go much further, particularly with respect to theories of the firm and how organizations work more generally. That agenda is an exciting one and if it is pursued, it will give new insights. There is already some movement in that direction.

My research agenda after writing the two books just mentioned was to take up early work on evolution and show how evolutionary principles can and have to be brought in. Perhaps surprisingly for a social scientist, my argument is that the Darwinian core principles—of variation, selection, and retention—offer a general framework that helps us understand all complex evolving systems. This applies to social systems as well. It does not give us all the answers but it is a way of organizing our inquiry in those areas. Old and new institutionalists alike are concerned to explain change, and sometimes radical structural change, in systems. Darwinism offers a framework for further theoretical development in this area.

What about the relationship between new institutional economics and the neoclassical mainstream? It seems that new institutionalists vary in their assessment of the mainstream. Some are very critical about it whereas others essentially side with Oliver Williamson in seeing the new institutionalism as a strand of research compatible with, and largely complementary to, the mainstream. Your own work often is very critical of neoclassical economics. What is your motivation behind this assessment?

I define neoclassical economics in terms of the assumption of rational utility-maximizing agents with relatively well-defined choice sets. A principal aim is to try to explain how particular equilibria are formed through agents making choices and interacting with others in particular settings. The limitations of neoclassical economics have partly to do with its psychological assumptions and the thinness of the rationality assumption. When expressed in a broad and inclusive manner, the rationality assumption is not so much wrong as rather empty and really not that useful. Gary Becker, for instance, always has to bring in auxiliary assumptions to get anything out of it. Mark Blaug and others have made similar critical points concerning rationality.

One of my criticisms of Williamson is that he does not take context sufficiently into account. When people operate within institutional settings they take into consideration the norms and rules that prevail. We are social animals. We are strongly attuned to verbal and non-verbal signals, including body language and expressions of sympathy or anger. Such signals and emotions are all around us in all kinds of institutional and organizational settings. Hence when people go to work in the morning at nine o'clock and go into the firm they become moulded by those institutional and cultural settings. This 'downward causation', from institutions to individuals, is lacking in Williamson's story. He just adopts a comparative statics argument, like Coase did in 1937: Coase considers the relative transaction costs of two governance modes, the market versus the firm. But there is no discussion of how individuals are changed. The same individuals are maximizing the same preference function in both contexts. Williamson also adds an unnecessary but symptomatic twist concerning opportunism. Although some people are opportunistic it is not the main reason for the existence of the firm. Williamson's stress on opportunism also goes against the minority of mainstream economists who assume rationality but stress the possibility of altruism.

Last year Oliver Williamson and Elinor Ostrom received the Nobel Prize in Economics for their work on institutions. That adds to the two previous institutional economists, Ronald Coase (1991) and Douglass North (1993), who have been awarded Nobel Prizes as well. What are your thoughts on this choice and to what extent do you expect these prizes to raise the standing of new institutional economics within the economics profession?

Let me just make a quick amendment. There were two earlier institutional economists who got the Nobel Prize: Simon Kuznets in 1971, and Gunnar Myrdal in 1974. They were old institutionalists. At that time, before the full triumph of neoclassical formalism throughout the discipline, old institutionalists retained some mainstream respect and influence. That being said, I want to stress that despite my disagreements with Williamson's work, I am delighted that he and Elinor Ostrom got the Nobel Prize for their very important work on institutions and governance systems. It certainly puts institutional economics further up the agenda. The Nobel awards to Coase, North, and now to Williamson and Ostrom, have helped enormously to raise the prestige and profile of institutional inquiry. To get the Nobel Prize for work that is rigorous without necessarily being expressed in mathematical language provides a lesson for us all concerning the possibilities for making advances in economics that are not confined to mathematics. Williamson and Ostrom are not well-known for their mathematics. They are better known for their deep insights into the nature of institutions. Mathematics is an important tool; but without a rigorous conceptual framework and significant empirical inquiry it is of limited use.

There seems to be an important shared theme in your work and the work of Elinor Ostrom. Like Ostrom, you often emphasize the importance of informal institutions in explaining economic behaviour, and that what is really shaping human behaviour are the habits, norms, and routines that people have. How, then, do you see the explanatory role of both formal and informal institutions in economics? I have the impression that not very much work has been done yet to integrate both of these aspects in a systematic manner.

This is a very important and interesting question. A barrier to progress in this area is that the terms 'formal' and 'informal' institutions are used in different ways. Coming from a philosophical background like you, I am concerned to be precise about meanings. I have noticed at least three prominent definitions and formulations of the dichotomy between the formal and the informal (Hodgson 2006). For many, formal means legal institutions, and informal means rules that are not codified in law. But that is not the only usage of these terms and it is always important to be clear.

In my work, I emphasize the concept of 'habit'. Habits drive individual agents. Cognitive or behavioural dispositions—ways of thinking; ways of doing; ways of interpreting—are all expressions of habit. The habit concept gives you not only a means of understanding individual preferences or dispositions, but also a means of understanding how institutional settings and constraints can affect or change individuals. In a 2004 paper in the *Journal of Economic Behavior and Organization*, Thorbjørn Knudsen and I have done some agent-based modelling on this, where agents change their preferences or habits as a result of interacting with others and establishing particular kinds of behaviour. There is both upward and downward causation in the model, which provides a rigorous means to consider causal mechanisms operating in different directions.

In a lot of my writing up to last year, I emphasized that habits underpin institutions. That is a very Veblenian view. I think that is still true, but it misses an important part of the story concerning how laws and organizations work. Arguably, unless laws are rooted in habits, they are unenforceable. But the problem with that argument is that law is so vast and complex that no single person can embody all the habits corresponding to the legal system of The Netherlands, or Britain, or anywhere else. So how does it operate? Law involves an authority mechanism. The special habit in this case involves recognition of authority. Here I cite the Milgram experiments on authority. I reread Stanley Milgram's book *Obedience to authority* (1974) recently. He actually sets up an evolutionary argument for a disposition to recognize authority: it is an evolved mechanism that helps cohesion in groups. We recognize the legitimate authority of the legal system, the police's authority, and so on, and in many cases that is sufficient to get us to conform to laws.

As I explain in a 2009 paper in the *Journal of Economic Issues* (Hodgson 2009a), the relationship between habits and institutions is more complicated. The authority mechanism is a neglected key element in this story that helps us understand how certain types of formal institutions operate.

In much of my research I have focused on the firm as an institution. More recently I have turned to law as another institution. The way they operate is similar in some ways, but quite different in others. Legal authority has to do with the role of the state and the recognition by citizens of its legitimate authority. The legitimating mechanisms involve democracy and consent, at least in many modern societies. Within the firm it is a slightly different story. There are hierarchies within firms and authority rests on a different kind of legitimacy. Authority claims there are established through contract. We need to do much more research on the ways in which social structures and social positions have effects on individual acquiescence or rebellion.

In your more recent writing one concept figures very prominently: 'generalized Darwinism'. Could you briefly explain the concept and its main benefits for theorizing about institutions, and more generally in the social sciences?

By generalized Darwinism I mean the abstraction and generalization of core Darwinian principles to other evolving complex systems. I learnt from my philosophy training and start with the ontology. I specify the phenomena that we are considering in this context as 'complex population systems'. These involve populations of entities which are heterogeneous to some degree and interact with each other, giving rise to complex patterns and outcomes. These entities need resources to survive and such resources are in some sense immediately scarce. To use Darwin's phrase: they face a 'struggle for existence'. Individual entities furthermore have the capacity to acquire solutions to certain problems concerning their survival and pass those solutions on to others. So there is a notion of information retention and replication. Note here that I am defining information in a very broad (Shannon-Weaver) sense, involving an input signal and a reaction (see Shannon and Weaver 1949). Information in a narrowly defined human or interpretative sense is taken into account at a later and less general stage of the argument.

After specifying that ontology, I argue that an explanation of the evolution of the social system must involve the Darwinian principles of variation, selection, and replication or inheritance. Why? Because we have to explain that variation exists and how it persists in the system. Entities are degradable and can expire and we have to explain why some survive and others do not. And we have to explain how information solutions are stored and replicated or passed on from entity to entity. Without such explanations we have an incomplete story. Any complete scientific analysis of such a system must involve those elements.

Having specified the ontology, the case for generalized Darwinism becomes quite straightforward. Other evolutionary economists, such as John Foster (2005) and Ulrich Witt (1997), emphasize self-organization. That may be very important, but it does not give you a complete explanation. You still require the Darwinian principles. Self-organization can and does occur in both human society and in nature, but it is not the complete explanation, because it cannot explain why one self-organizing system survives rather than another.

However, generalized Darwinism is not biological reductionism. It is not saying we have to explain social phenomena in biological or genetic terms. Whether you can or you cannot is partly an empirical issue. It is not something which is assumed at the outset by generalized Darwinism. Neither is it assumed that every outcome is efficient or that evolution is an optimizing process. In biological evolution the outcomes are not necessarily optimal—so too in social evolution. Survival does not always involve the fittest. Neither does this argument necessarily justify free market economics. It depends on the particular context and mechanisms involved.

Having established the agenda, the next step is to express the core concepts in a generalizable form. But one has to be careful not to bring in any unnecessary biological baggage with those generalized terms. That is particularly important with concepts like selection and replication. Fortunately, in both those areas a great deal of important work has been done, particularly by philosophers of biology since David Hull's important 1988 book *Science as a process*. This work helps us to move forward to specify these concepts in a manner that can apply to all complex population systems. They address the commonalities rather than the specifics.

In my work with Thorbjørn Knudsen (Hodgson and Knudsen 2010) we refine those concepts. We try to build on the work of others to improve their formulations. We also argue that the general replicator-interactor distinction—which in biology is the genotype-phenotype distinction—is also vital to understand the process. There has been some questioning of the relevance of the replicator-interactor distinction, by both biologists and economists such as Richard Nelson. In response we defend the distinction and refine previous definitions of the replicator and interactor concepts.

What is the use of all this? First of all, I would defend the role of free inquiry in the academy, even in the absence of any known payoff. Today grant-awarding authorities and governments are too keen to insist that research must immediately show a business or other payoff. While I strongly believe we have a responsibility to society, demonstrating immediate payoffs is not the way that research works. Generalizing Darwinism is long-term research, concerning conceptual underpinnings. By its nature, we cannot predict how fruitful it will be. It is much based on scientific hunches. We simply have to pursue it, refine it, and see how far it goes.

But we already have a glimpse of some payoff value. One thing that comes out of our abstract conceptual work is the importance of information and its replication. This might open some doors for understanding how human society and business institutions evolve. We also examine the conditions under which information-retention and replication create possibilities for future complexity, involving greater variation and more complex interactions. So I think we are in sight of middle-ranging theorizing—after Robert Merton's term for theory that is less abstract, but more than inductive empirical work—although we are not quite there yet.

There is one thing about evolutionary frameworks that I find somewhat puzzling. Isn't it always possible to come up with an evolutionary story ex post? So the framework is likely to be consistent with a broad range of social phenomena, but in order to give it explanatory bite isn't it necessary to be much more specific and explicit about how to falsify or verify evolutionary conjectures?

Again a good question. Several decades ago there was a debate involving Popper and others, concerning whether Darwinism is falsifiable. If Darwinism means the survival of the fittest and fitness is the capacity to survive, then you have a circular argument. It is a tautological formulation with no predictive value. Survivors survive because they are fit, and they are fit since they survive.

But this claim involves a misunderstanding of Darwinism. First, 'survival of the fittest' is an inexact formulation of Darwinism. Although Darwin adopted the term, it came originally from Herbert Spencer and Darwin had misgivings about it. Second, fitness itself is a problematic concept (Knudsen and I address that in our writing too). Philosophers of biology—and even biologists themselves—do not define fitness simply in terms of survival. They use proxies such as the propensity to produce offspring. In such cases the formulation ceases to be a tautology—it is potentially false. So the tautological point can be circumvented once one is careful about the concepts.

Your ex post argument is slightly different. It is partly true that evolutionary explanations are often backward-looking: they explain things that have already happened and are quite weak in predicting. And your argument would apply to biology as much as it would apply to any evolutionary process in the social domain. Yet, evolutionary biology is extraordinary powerful and successful as a science. Why so? The answer involves a combination of general frameworks and particular heuristics. Within the overarching Darwinian framework scientists bring in auxiliary hypotheses which have contingent value depending on the circumstances.

Similar arguments apply when we move to social evolution. The overarching framework is just that: it does not provide the detailed answers. You get explanatory value out of it by adding particularities—particular mechanisms, particular contexts, particular processes—within that framework. The important thing is that the framework helps us to understand key processes in a very complex situation. With varied interacting agents we can see though the tangled mess and identify some key processes at work. Among other things, we need to understand how business firms evolve and how human institutions interact with the natural environment. The agenda is potentially huge.

In which sense is your recent work on generalized Darwinism an elaboration or a generalization of your earlier writings on evolution? That is, the move from evolution to generalized Darwinism, is it just a terminological modification or does it imply major conceptual differences as well?

As early as the 1980s, Veblen influenced me greatly in terms of incorporating Darwinian and evolutionary ideas. I was surprised to discover that evolutionary economics had a different conception of what evolution meant and the Darwinism issue was mostly on the fringes. For example, in their 1982 book *An evolutionary theory of economic change*, Richard Nelson and Sidney Winter mention Darwin once in passing, and not for any analytical insight. They list a whole series of intellectual mentors but Darwin is not one of them. As another example, take the USA-based Association for Evolutionary Economics. I learned that the 'evolution' in their title does not mean Darwinism, despite their declared Veblenian origins and affinities. It simply means development. And when the International Schumpeter Association was formed in 1988, and announced the *Journal of Evolutionary Economics*, 'evolution' meant Schumpeter more than Darwin. Schumpeter himself however makes little reference to, or use of, Darwin.

It was rather strange that evolution suddenly reappeared in the social sciences, and in economics in particular, and yet it was unclear what it meant and the prominent Darwinian meaning was sidelined. My 1993 book *Economics and evolution* asked: What does this term mean and on what grounds might it be adopted in social science? As case studies in that book I look at Schumpeter, Veblen, Hayek, and others. I did not argue there for a generalized Darwinism framework—I became persuaded by that shortly afterwards. Various people, including Daniel Dennett (1995), triggered my fascination with that line of inquiry.

I have been interested in evolutionary ideas for a long time, but I always wanted to know what this term meant. I find that Darwinism provides us with the only satisfactory general framework for understanding the kind of processes we are looking at in human society over the long term.

There seems to be a current trend in social science of borrowing ideas and concepts from biology, such as Darwinism. Do you think that biology can be a fruitful source of ideas for the social sciences, and are there further biological ideas and concepts that you find particularly interesting?

I am with Alfred Marshall here. He saw biology as the Mecca of the economist. Biology is important for the social sciences because in both cases we have highly complex, variegated, interacting systems. The success of scientific explanation in biology, in its highly complex domain, is a lesson for economists.

But that does not mean that we should slavishly imitate everything we find out in biology. There are lots of analogies that do not work. There is nothing in society like the gene: the way replicators work in the social domain is very different from genes and other biological replicators. They both pass on information from entity to entity, but the mechanisms and the nature of that information are very different. We should not collapse economics into biology either by slavish imitation or by believing that biology offers the key to understanding everything social. Far from it. We have a lot of interesting work to do concerning biological influences on human behaviour but we still have to explain things partially in terms of culture and institutions.

So there are limits to biology as well. Another limit, which people often mistakenly raise as an objection, is that humans have important capacities which are absent in other species: deliberation, conscious prefiguration, intersubjective understanding, conjecturing what others think and intend, and trying to anticipate their behaviour through such conjectures. All this means that humans are special and the abstract apparatus of generalized Darwinism is inadequate. We have to build into that framework additional assumptions that are specific to human society. Here we learn much more from social theory, philosophy, psychology, and anthropology. Those answers become vitally important. So the extremely important observation that humans are different in terms of their mental capacities has to be taken into account in the evolutionary analysis. But this does not mean that you throw Darwinism out of the window. It means that you have to incorporate additional, more specific theories into its framework.

Could there be other inspirations from biology that are not yet exploited? Surely yes! One of my PhD students is working on the notion of niche construction. He takes the idea from biology, looks at the biological debates, and develops a taxonomy of different uses of the term and sees whether they are applicable to business niche construction. He compares preceding theories of the firm and observes that they often downplay relevant processes of interaction between businesses and their environment. So we can get inspiration of all sorts from biology. But generalizing Darwinism is not dependent on raiding everything from the biological store. We can often get insights from other sciences too. We can get insight from anthropology, complex systems theory, and even from some forms of mathematics. This is a way that science progresses: by combining ideas from different domains, synthesizing them, and obtaining new understandings.

Unlike many other institutionalist economists you have been sensitive to methodological questions throughout your work. For example you have written extensively on the issue of methodological individualism (Hodgson 1986; 2007). Is there a danger that a multidisciplinary account such as you just suggested will lead to a whole variety of serious methodological difficulties?

I will respond on methodological individualism and then answer your question on methodological problems. In 2007 I published an article on methodological individualism in the *Journal of Economic Methodology*. I argue there that everyone in the social sciences, as far as we are aware, ends up explaining social phenomena in terms of both individuals and relations between individuals. Kenneth Arrow says much the same thing in the *American Economic Review* (1994). For Arrow, even general equilibrium explanations involve structured relations between agents. We know of no exception to this rule. We always have to explain in terms of individuals *and* relations among individuals. When social theorists mention structure they mean relations between individuals. So every successful explanation in the social sciences involves some combination of individuals and structures. There are forms of Marxism where individuals are pushed out of the picture. But structure alone cannot explain things, and anyway without individuals there can be no structure.

Methodological individualists are extremely shady and imprecise about what they mean by the term. There are several definitions of methodological individualism and some protagonists shift from one meaning to another. I ask a methodological individualist: Do you believe that explanations can and should be in terms of individuals alone? Or do you believe explanations can and should be in term of individuals *and* relations between individuals? If they are foolish enough to take the first option—involving individuals alone—then I say: Please show me one successful example of such an explanation. So far I have not been shown one.

Concerning the second option, my argument is: Why call this methodological individualism? There are two explanatory elements in this story which are both foundational: individuals *and* relations between individuals. So, if you call it methodological individualism you are stressing half of the story. A structuralist could call this methodological structuralism and be equally in error. It is an equal bias, in the opposite direction. Both would be wrong. They would commit the same error of stressing one explanatory element and not the other.

Should we follow Joseph Agassi (1975) and call it institutionalist individualism? Here I question why one term is a noun and the other an adjective. Why not individualist institutionalism? Again the symmetry of explanatory elements, 'institutional individualism' is biased in its choice of adjective and noun. Overall, methodological individualism suffers from a deep ambiguity. By saying precisely what it means we can get rid of a lot of fog and confusion. We can transcend silly debates which are caught up in ambiguity and may have other agendas behind them.

You ask what problems we face as institutionalists in understanding institutions from a methodological point of view. Following work in that area in the 1980s and 1990s involving Anthony Giddens, Roy Bashkar and others, a key question is the relation between the individual agent and social structure, and in what sense there is mutual determination of one by the other. But social theory has become unpopular because it is perceived to have got down in the wrong kind of issues, methodological individualism being an example. I think that this rejection of social theory is over-hasty and mistaken. Many sociologists and social scientists said: 'A plague on both houses! This is getting us nowhere! Let us escape from this mess and just build models, gather data or whatever'. That is a foolhardy reaction, because neither theory nor empirics are possible without implicit or explicit methodological underpinnings. I think a number of critics have observed that when people try to ditch these issues they end up bringing them in through the backdoor. We cannot escape from these fundamental problems of social science.

I argue that evolutionary theory helps us in this area too. Some social theorists offer a model of the social world where agents just appear with beliefs. They may give us a rich story about interaction between agents, mutually constitutive agents and structures, and agents facing constraints bequeathed by history. All this is important, but they give us an inadequate account of the origins and development of the human agent. They commit the same error as Marxism, omitting a causal account of agency itself. There is here an evolutionary story in terms of the development of the individual—how individuals have developed in particular cultural and institutional settings—and there is an evolutionary story about how these dispositions are transmitted, genetically and otherwise, through time. Marxists, critical realists, and many other social scientists ignore that.

We have been talking already about the fragmentation of institutional economics. On the one hand, there is certainly a lot of epistemic plausibility to the idea of exploring a problem from different points of view, and few people would object to pluralism in some form. On the other hand, I have the concern that this pluralism has a potentially problematic downside with respect to achieving cumulative progress in the field, both theoretically and empirically. Do you see any danger of this sort and, if so, how do you think it should be dealt with?

Some people are against pluralism. Some economists define their subject in a way which excludes whole domains of alternative inquiry and alternative methodologies. But let us move on to your main question. When the pluralism debate was reignited in the mid 1990s, I was a participant. There was a conference in Bergamo in Italy. Uskali Mäki, Sheila Dow, Wade Hands and others participated. A book, *Pluralism in economics*, edited by Andrea Salanati and Ernesto Screpanti

(1997), came out of it. Several contributions in that book made the point that there is an ambiguity in the concept of pluralism. Does it refer to pluralism in the mind of a single individual, or to pluralism in the academy? People seemingly unaware of these earlier contributions—and of that book in particular—have reiterated the same point over and over again that was raised right at the beginning.

My view is that pluralism in a single head is a recipe for nonsense, because if you hold contradictory ideas then you can logically crank out all sorts of absurd propositions. So I am not some kind of new-age philosopher who believes that you can get on with conflicting ideas. We do have conflicting ideas, but we have to try and reconcile them. Science sometimes adopts different assumptions in different domains. But eventually scientists have to worry about that, as economists worried about the discrepancy between general equilibrium and Keynesian theory. They resolved that in a wrong way, but nevertheless they were right to worry about it. So pluralism in a single head is something to be fought against and overcome. I am against that kind of pluralism. Even if I may be inconsistent sometimes myself, I would like to be corrected and to move towards a consistent position.

But I am in favour of pluralism in the academy. Pluralism there is important for making progress in science. Without a variety of views, everyone is locked into one groupthink way of seeing the world, and things do not change. We know from the history of science that things change when someone brings in new ideas and these clash with the old. Some new approach emerges and in some cases new approaches prove to be robust and useful in scientific terms. Without variety, there would be little chance of generating progress and novelty in science.

On the other hand, if we have an extreme amount of variety in the academy, then we would have chaos and no progress at all. We would be constantly attacking every position from a variety of angles, disallowing any possibility of development clustering around an approach or paradigm or set of principles, and preventing it from taking off scientifically. Neither extreme is conducive to the development of science. Such an argument has been made very powerfully by Philip Kitcher in his 1993 book *The advancement of science*, where he considers the optimal degree of pluralism in the academy. Part of such a sophisticated pluralism involves rejecting ideas and screening out things which seem untenable. We know that is risky. We know that if you exclude things the chances are that some fruitful lines of inquiry will be lost. They will be casualties. But we do need the capacity to build up critical masses of enquirers thinking along similar lines, so that a division of labour within that particular paradigm can be established and research can move forward.

It is a delicate balance and a difficult problem. There is no simple formula. But I am in favour of pluralism in the academy and I think that economics has gone too far in repressing dissent and minimizing variance—so far that by the 1980s you had to conform to a whole set of principles simply to be admitted into the discipline. There has been some significant increase in pluralism since then. It is now legitimate to challenge the core rationality assumption using behavioural economics or experimental research. There is some sign of progress, but I still think that economics is insufficiently diverse. I support those who argue for greater pluralism in economics, but we should also reflect on the limitations of pluralism. There is an urgent need to develop new theoretical approaches. That means the clustering together of people with similar ideas, rather than endlessly piling diversity upon diversity.

You say some things should be excluded. On which grounds would you exclude ideas in institutional economics?

There are some relatively simple initial tests. We reject ideas that are ungrounded in the existing literature, for example. We occasionally come across people from business and elsewhere who claim to have valuable scientific ideas and insights. My reaction is often: That is very interesting, but you have got to make it much more rigorous, and you have got to show how it relates to previous thinking. This is sometimes not the answer they want to hear because that means they have got to do a lot more work to get it there. Maybe I am turning away people with brilliant ideas simply by that negative response, but I think that is one condition for entering the academy.

Other criteria are more difficult. I have rejected the notion of an immediate explanatory payoff. With such a criterion Darwin would have been stopped when he came back from the Galapagos: This is interesting, but not worth pursuing; we cannot see where you are going. Darwin would not have gotten a research grant—he did not need one fortunately. We would have stopped a lot of research at birth if we asked for immediate results.

But the reaction of the peer group is vital. Kitcher's insight is that science is not to be understood simply as a set of individuals engaging with the world and trying to understand it. Science is a communal process, with its own vital institutions. This is an epistemic community, where each individual is dependent on the others and the community itself establishes standards. That does not rule out the possibility of something going wrong. Because of the scientific failure of its own institutional mechanisms, economics to some degree has gotten sick, as Mark Blaug and others have observed.

Let us try to turn institutional economics towards politics. Do you see a potential role for institutional economics in policy application and, if so, what would this role look like?

My work has not been very close to policy application. I am interested in political problems: I have political views, and I am critical of free market economics. But I do not have that much experience in moving from middle range theory towards policy application. Other people are very good at that. Elinor Ostrom is a very fine example, I find her work inspiring and immensely valuable in helping us understand the key role of institutions and it also has immediate policy implications.

However, in a recent paper (Hodgson 2009b) you analyze the role of economics in the crash of 2008. What do you regard as the key factors behind the failure of economics to predict this financial crisis?

Failure of prediction is an interesting issue. We may consider those who claim to have predicted the crash and to give credit where credit is due. But after a point it becomes a very difficult question to answer because all sorts of people have written: This cannot go on! There is too much debt! And so on. But is that a prediction?

It is just as interesting to consider what reception the prophets of doom received in the academic community. Here we have evidence of a failure to acknowledge the possibility and also a prominent mechanism of dismissal, in the form of the observation: You haven't got a model! This was the response to Nouriel Roubini when he spoke at the International Monetary Fund in 2006: 'Where is your model? Is it simply rhetoric? Is it simply descriptive stuff? Unless you have got a model I am not going to take you seriously!' This is a highly biased epistemic screening device that economists have been regrettably trained to take seriously. It meant that economics was not alerted to a potential problem. Also at play was the ideology of free markets, where free markets can do little wrong and there is no extra-market remedy for a downturn. At root a combination of free market ideology and inappropriate epistemic screening led to a limited number and weighting of relevant warnings of impending disaster.

To their credit, Paul Krugman and others have come out and criticized the profession for its failure. But I think it is amazing that we have had the greatest economic crash since the Great Depression, but so little in-depth discussion or self-reflection by economists on possible internal flaws in economics itself, which in turn might help to account for its failings and help us to deal with them practically. There is some discussion along those lines, but it is muted, inadequate, and surrounded by indifference. So this has become a crisis for economics as well as a crisis for the economy. We have to act. That means raising serious questions and trying to get good answers to them.

Do you actually see, for instance in Great Britain, that economics as a science is really coming under pressure from the public for its failures?

Among sophisticated journalists with some economic training, there is a great deal of criticism, both in Britain and in America, of the profession and its failings. There is criticism of the failure of financial economics to envisage possibilities along the lines that have emerged and the failure of macroeconomics to deal with the crisis. Financial economics focused far too much on money-making instruments, which are often lucrative for those who develop them. But financial economists are not trained to look at the broader picture. They acquire a vested interest in promoting their own financial instruments so as to get lucrative consultancy contracts, rather than playing an ethical role and taking up their responsibility as scientists to forewarn about dangers.

So there is a moral crisis amongst economists as well. I am very much in favour of an initiative from America to establish a professional code for economists. Like doctors we have duties. Our duty is not simply to ourselves: to make money and to get nice academic positions and big research grants and nice consultancy contracts and to go to conferences in exotic places. We may do that, but it is not the main objective of the profession. That objective is to serve society. As scientists we serve by helping to understand how society works and the potentialities and dangers inherent in any institutional process or development. That is our moral duty. The ethic of self-interest which economists seem to believe in has corrupted economics to the point that we abstain from our scientific duty. Just as doctors have a duty under the Hippocratic Oath to care for people medically, we should have the equivalent of the Hippocratic Oath to care for the health of the economy and to advise accordingly.

Do you think that the profession will actually change in response to the economic crises of the recent past or will economists just return to business as usual?

A big debate that is going on is whether economics will change from within or will change by being challenged by an alternative locus in the academic community under some other label, perhaps 'political economy'. David Colander is an advocate of the first strategy. But I am not convinced that it is possible. Perhaps we should be pluralists here too: some of us should work to change economics from within, and some of us should work to change it from without.

One last question, maybe as a general conclusion. What would you regard as the key achievements of new institutional economics so far, and where do you see its main challenges for the future?

Its key achievement, which is very big, is to put institutions back on the agenda. They were on the agenda in previous schools of economics but they somehow slipped off. When I was studying economics in the 1960s we were often presented with an institution-free world. The firm was a black-box. The state was just a point in space outside the system. So we knew very little about institutions. The new institutional economics has put institutions back on the agenda, not only in terms of minority academic inquiry, but also in all sorts of policy institutes like the World Bank, the International Monetary Fund, and so on. These agencies take institutions very seriously now.

But there are a number of important challenges. The problems I have raised in this interview concerning structure and agency, concerning the role of downward as well as upward causation, and explaining ongoing change are fundamental. I think we also have to make progress in terms of developing more middle-range theory and we have to make further progress in developing applications of such analysis. There are key problems that remain unresolved, such as the causes of firm performance and the determination of their structure and boundaries. The interface between economics and law needs to be rethought and reconstructed. We need a better theory of the individual. We also have to reconstruct welfare economics in the light of institutional and evolutionary insights. I could go on, but there is enough here to keep us occupied.

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Review of the *Oxford handbook of philosophy of economics*, edited by Harold Kincaid and Don Ross. New York: Oxford University Press, 2009, 688 pp.

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1

The *Oxford handbook of philosophy of economics* aims at bringing out what is *new* in the philosophy of economics—an aim that, I believe, has been successfully achieved. The introductory chapter by Don Ross and Harold Kincaid does a superb job of describing the current orientation of philosophy of economics, the result of developments in philosophy of science, in economics, and in the relationship of philosophy of economics to both fields. Throughout the 1980s and 1990s philosophy of economics was in fact mostly concerned with applying abstract philosophical rules to the case of economics, whereas nowadays it is more preoccupied with understanding and evaluating economics as it is actually practiced and with developing, in-house as it were, the philosophical tools required for these tasks.

This, according to the editors, is not only how philosophy of economics is now done, but also how it *should be* done. In order to deliver a philosophy of science that concretely engages with scientific practice, "the key for philosophers is to keep their ears as close as possible to the ground—in this case, the ground being the economics seminar rooms around the world in which the graduate students gather" (pp. 28-29). I find this valuable advice, especially for young philosophers and methodologists of economics—one of the main audiences of this *Journal*. The range of topics discussed in the *Handbook* pretty much covers the whole spectrum of interests of contemporary philosophy of economics. As such it provides a valuable resource for philosophers and methodologists of economics, not only to gain an up-to-date map of the field but also, I believe, to discover new directions of inquiry. Thanks to the practice-oriented character of many of its contributions, the *Handbook* will also interest economists, or so one hopes.

In what follows I will not discuss each contribution in detail or offer a general discussion of the book. Since virtually every author is a renowned expert on his/her respective topic and every chapter is self-contained, readers interested in a particular theme can easily identify the chapters they wish to consult. Instead, I will give a general idea of the book's contents by briefly summarizing each chapter and then talk more extensively about a specific portion of the book.

2

The volume is organized into four parts. Part I "Received views in philosophy of economics" collects partly autobiographical reflections by three of the main influential contributors to the philosophy of economics from its early days, namely Daniel Hausman, Alex Rosenberg, and Uskali Mäki. I will say more about these later. It also includes an essay by critical historian of economics Philip Mirowski, who, in his typical engaging style, aims to persuade us that the celebrated transformation of economics into a science of knowledge is in fact a "nonexistent achievement".

In line with the overall aim of the *Handbook*, the rest of the chapters mostly deal with philosophical issues that emerge from recent developments occurring within economics, namely: (i) the development of massive computing power, (ii) the rise of game theory, (iii) the increasing integration of economics with other sciences, and (iv) the turn to empirical experimentation.

Part II "Microeconomics" deals with the ways in which these developments have affected microeconomics. Cristina Bicchieri examines the potential of the experimental turn in game theory for generating models of rationality that include a social component. James Woodward assesses experimental investigations of social preferences and concludes that the non self-interested aspect of behaviour comes out as a robust result, but contemporary approaches to explaining this have so far failed to do so in a systematic, non ad-hoc way. Considering his previous work, it is not surprising that Francesco Guala's contribution discusses the methodology of experimental economics. Nevertheless the discussion is given a novel and original twist by his use of experimental economics as a case study to articulate the concepts and content of a normative methodology which takes scientific practice seriously, but also offers normative advice relevant to that practice. Anna Alexandrova and Robert Northcott examine the use of idealized economic models to construct the 1994 U.S. Federal Communications Commission (FCC) electromagnetic spectrum auctions and their contribution to the success of those auctions. To explain the role that economic models played in this particular case, they advance their own account of "models as open formulas", and propose that progress in economics is best viewed as a variety of engineering progress. John Davis analyses the conceptions of the individual implicit in new research approaches in economics,[1] and shows the ways in which they depart from the atomistic conception presupposed by neoclassical economics. Following on his previous work, Don Ross takes recent empirical research to task in order to shed light on the relationship between people, subpersonal interests, and brain systems. Finally, Jack Vromen reviews recent developments in evolutionary theorizing: evolutionary game theory, neuroeconomics, and bioeconomics. He argues against conflating proximate and ultimate (evolutionary) causes of behaviour, but argues that knowledge of proximate causes may be helpful for construing more realistic evolutionary scenarios.

Part III "Modeling, macroeconomics, and development" includes a heterogeneous set of chapters. Paul Humphreys analyses the novel philosophical issues raised by the advent of computational modelling vis-à-vis more traditional techniques. Kevin Hoover deals with the venerable discussion about the importance of microfoundations for macroeconomics and shows why it is merely an "ideology". In her brief but insightful piece, Nancy Cartwright casts doubt on the role of both causation (at least as conceived in current accounts) and invariant relations for the purpose of reliable predictions in policy and technology planning. She concludes with an open and somewhat unsettling question: "What can we offer that is better?" (p. 421). Stan du Plessis reviews modern attempts to demonstrate that, rather than being a problem, data mining is a necessary part of a sensible modelling strategy. Harold Kincaid compares neoclassical growth theory and contemporary development economics as approaches to explaining growth and aims to make explicit and assess their unarticulated assumptions about explanation and evidence. He then argues that work in contemporary development economics is more promising because it does not rely on the suspicious assumptions crucial to neoclassical growth theory. Finally, Gary Fields's contribution is about models of labour markets in developing countries: the message (which could have been elaborated further) is that models of labour markets are context

[1] Namely, behavioural economics, agent based computational modelling, behavioural game theory, and neuroeconomics.

specific—where the plural in both 'models' and 'markets' indicates that there are different kinds of (labour) markets as well as multiple ways of modelling them.

Finally, part IV is made up of four chapters that tackle different aspects of the relationship between economics and welfare. Keith Dowding examines approaches to measuring human welfare and the way in which problems of interpersonal comparability can be solved in practice. Based on his previous work, Ken Binmore argues that the conception of utility of modern economics is compatible with making interpersonal comparisons. Erik Angner discusses measures of well-being in economics and psychology, exploring their fundamental commitments and arguing that those commitments contribute to explaining why measures of well-being are so different in the two fields and why fruitful communication is hard to come by. In his lengthy contribution, Partha Dasgupta disentangles facts and values, and argues that contemporary economists principally analyse the former and are right to do so.

3

I now look more closely at the articles by Rosenberg, Hausman, and Mäki. This choice of focus is mostly a matter of taste—I found the narration of the authors' intellectual development in parallel with that of our field fascinating. Although this may not have been fully intended by the editors,[2] it turns out that these essays not only tell the story of where we come from, but also, to some degree, show us where we stand and where we should go from here.

In his contribution "Laws, causation, and economic methodology", Dan Hausman recounts the development of his views from the 1970s onwards.[3] As is well known, his early work centred on laws. He saw his task as demonstrating that economics did have laws, albeit of a particular kind. Hence, his account of the role of inexact laws in explanation and prediction, elaborated in his influential *The inexact and separate science of economics* (1992). Issues within economics as well as difficulties with the notion of inexact laws led Hausman to move

[2] Ross and Kincaid write, "Part I of the Handbook showcases the image of economics against which a majority of philosophers of science have increasingly reacted. It thus describes a platform relative to which the rest of the book's contents amount to a complex response" (p. 28).

[3] Hausman's piece also includes a nice section in which Hausman explores points of contact and divergence between his own views and those of Mäki and Rosenberg.

progressively away from questions about laws and engage with issues of causation and causal explanation. In his *Handbook* chapter, Hausman proposes a variant of the erotetic-contrastive approach to explanation, which takes explanations to be answers to why-questions that often implicitly contrast the explanandum phenomenon to another outcome (or set thereof). Explanation is thus a matter of citing causes that discriminate between the explanandum phenomenon and contrasting outcomes.

Citing discriminating causes however is not enough. Explanations should also provide accounts of how the cause produces the explanandum (i.e., they should provide a mechanism). Finally, explanations are better if they are deep: (i) an explanation is deep if it can account for many contrasts or for contrasts within a larger range; and (ii) an explanation is deep if the mechanism that links the cause and the effect is robust. On this view, it is not particularly illuminating to debate whether the inexact generalizations of economics qualify as laws. Instead, we should ask whether these generalizations identify discriminating causes (and their mediating mechanisms) and possess some degree of invariance, an attribute of generalizations that, Hausman holds, is crucial to achieving our practical ends (see Woodward 2003).

Like Hausman, Alex Rosenberg's early work concentrated on laws. But unlike Hausman, he sought to find the reasons for the predictive limitations of economics and concluded that in economics there are no laws. He identified the source of these shortcomings in the reliance on intentional states that economics shares with psychology (Rosenberg 1992). In this chapter, entitled "If economics is a science, what kind of a science is it?", Rosenberg admits that his early diagnosis was partly incorrect. Rosenberg has now come to believe that the predictive limitations of economics are due to the fact that it is a biological science and hence a historical science. As such, it constructs factual claims about historical trends with varying degrees of generality. Thus, according to Rosenberg, economics has no laws but rather spatiotemporally restricted generalizations that describe local trends that result from non-economic laws (notably natural selection) operating over local initial conditions. Economic interactions are reflexive, and this accounts for the fact that economic models have only transitory applicability even in their intended domains, i.e., why their predictive power is limited. Even though "the account of economics as a biological science leaves its actual character both largely untouched and endorsed as scientifically responsible after all, in spite of its predictive weakness" (p. 63), Rosenberg claims there is room for improvement, and some of it is already under way. Recent developments in economics have in fact made it act more like it should if it really were a biological science (game theory, for example, allows treatment of strategic interactions and of the impact of increasing returns on various kinds of asymmetries).[4]

Unlike Hausman and Rosenberg, Uskali Mäki's early views on the philosophy of economics were never presented in an extended monograph, and hence his piece, "Realistic realism about unrealistic models", also helps us see more clearly how some of the threads in his many published articles fit together in a single systematic account. Mäki's main motivation has consistently been to show that "*[u]nrealisticness in* economic models must not constitute an obstacle to *realism about* those models" (p. 68). The other major element of Mäki's philosophy of economics is the idea of isolation: all theories and models isolate a slice of reality from the rest of it. Idealizing assumptions, though patently false, serve the strategic function of theoretically isolating the causal factors or mechanisms of interest. The message then is that theories or models can make true claims about the isolated factors or mechanism, even if they contain a wealth of falsehoods.

Over the years Mäki has further refined his view, but the basic tenets have remained the same. According to Mäki, economic models typically isolate causal mechanisms, intended as mediating causal chains between input and output phenomena. "[B]y isolating a possible mechanism that could be causally responsible for, or could have significantly contributed to, the pattern" (p. 86), models provide possible and partial explanations of patterns of some generality (the typical economics explananda). Mäki also notes that explanatory activity in economics is often driven and shaped by the ideal of unification: "the insistence on microfoundations", "the avoidance of ad hoc explanations", and the phenomenon of "economics imperialism" are all, according to Mäki, manifestations of the pursuit of this ideal (p. 86). This aspect of economic theorizing has been relatively underanalyzed. In a series of publications, Mäki has sought to rectify this situation by

[4] Other developments include evolutionary game theory, interdisciplinary engagement with theories in cognitive and social psychology and in neuroscience, experimental economics, and models of asymmetric information. These themes are only briefly explored by Rosenberg, so how these developments make economics act more like a biological science is not fully spelled out.

offering a framework for the assessment of unification as an ideal, as well as its manifestation in economics imperialism (e.g., Mäki 2001; and 2009).

Rosenberg addresses the general question of what kind of science economics is. But even though it is illuminating to recognize that economics is more like biology than previously thought, that does not get us far in coming to grips with the peculiarities of economics. As Hausman notes, "the differences between generalizations in economics and certain areas of biology are at least as important as any similarities they may have in virtue of both biology and economics being historical sciences" (p. 47). So, even after having recognized that economics is a biological and historical science, the distinctive characteristics of its generalizations and the way they are and should be used for purposes of prediction, explanation, and intervention require careful study.

Hausman and Mäki claim to be concerned with local rather than global diagnoses of economics as it is actually practiced. As far as their contributions in the *Handbook* are concerned, they have also come to share an interest in the explanatory practices of economics—though whereas Hausman's interest in explanation mainly originates from questions about causation, Mäki's emerges from his work on unrealistic models. Because models are the main tools employed to formulate causal and explanatory claims, questions about causation and causal explanation are tightly connected to the metaphysics, pragmatics and epistemology of models. The preoccupation with how to normatively evaluate causal and explanatory claims, and the tools economists employ to generate them, is more salient in Hausman than in Mäki. Yet, as in the sort of normative methodology Guala advocates, philosophical assessments and prescriptions should be grounded on accurate accounts of how causal and explanatory claims are actually generated and for what purposes.

For example, although economists often attempt a description of mechanisms (one of Hausman's requirements for a good explanation), they endorse a specific conception of what sort of mechanisms are genuinely explanatory, namely *micro-economic* mechanisms. For certain purposes reductionistic explanatory strategies are just fine, but the idea that the *only* legitimate mechanisms for the explanation of economic phenomena are at the micro level is clearly questionable (e.g., Hoover contribution in this volume). Also, the emphasis on unification to which Mäki draws attention implies that economists insist on the application of the *same kind* of micro-economic mechanisms.[5] It is not at all clear however that the repeated application of the same kind of mechanism in different situations across domains is of epistemic value. Whether a mechanism operates in a given situation or domain needs to be determined case by case.

More generally, it remains to be established whether the research strategies and explanatory commitments of economics—which are not yet well understood—do serve well the aim of picking out the discriminating and deep causes of the phenomena to be explained. Likewise what is needed for successful planning and intervention also requires careful study, for if Cartwright is right, causation with or without invariance may not be enough. Answers to these questions are likely to depend on the kind of causal and explanatory claims we are looking at and their context of use. All in all, this suggests that in this area—and in other areas of the philosophy of economics, as the *Handbook* demonstrates—significant progress has been made, but a great deal of exciting work still awaits us. Both are good news.

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[5] The situation may be changing in favour of modelling detailed causal mechanisms according to the context of application, as stated in the "Introduction" by Don Ross and Harold Kincaid (p. 13).

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Review of *The methodology of positive economics: reflections on the Milton Friedman legacy*, ed. Uskali Mäki. Cambridge: Cambridge University Press, 2009, 382 pp.

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Friedman's 1953 essay "The methodology of positive economics" is undoubtedly one of the—or perhaps *the*—most influential and most widely and hotly debated papers on economic methodology. What economic methodologist would not dream of having more than 2,500 citations in Google Scholar for writing "the only essay on methodology that a large number, perhaps majority, of economists have ever read" (Hausman 1992, 162)?

At the same time the essay appears somewhat difficult to interpret and, to the extent that it has been interpreted, controversial. Indeed, many different methodological perspectives have been read into it. According to one interpreter, the essay "provides ingredients for a number of doctrines, such as fictionalism, instrumentalism, positivism, falsificationism, pragmatism, conventionalism, social constructivism, and realism" (Mäki 2003, 504) and "What the reader is served is an *F-mix*, a mixture of ingredients many of which are ambiguous and some of which are hard to reconcile with one another" (Mäki, 90 [all undated page references are to the volume under review]); according to another, "One can find in it echoes, and sometimes much more than echoes, of Popper, Kuhn, Quine, Toulmin, Laudan, and even Feyerabend" (Blaug, 351). But this apparent confusion does not stop commentators taking a firm view on its worth: methodologists and philosophers have generally taken a very critical stance, whereas the majority of practising economists seems to endorse its conclusions, whatever they are taken to be (Hands 2001, 57).

It should be no surprise, then, that more than half a century after its publication, the essay still attracts an audience. The book under review is the outcome of a 2003 conference held at Erasmus University Rotterdam to celebrate the 50th anniversary of the essay's publication. According to its editor, the volume collects papers that "were commissioned from what was close to the best possible team of scholars on the theme" (p. xviii).

It would be interesting to find out what this alleged 'theme' is supposed to be. The only thing that is clear after reading the book is that Friedman's 1953 methodological stance is not it. This is as startling for a book that has the same title as Friedman's essay as it is disappointing for those—like me—who are interested in economic methodology and hope to learn something new about methodological issues. Rather, the book comprises papers on a wide variety of topics that are more or less loosely related to Friedman's essay, such as its genealogy, its historical context, whether it caused the formalist revolution, whether it licensed the formalist revolution, what type of methodology Friedman as a practising economist endorsed, and many more.

The absence of new material on the 1953 essay was particular disappointing to me as a methodologist because: (a) I do not think the essay is quite as obscure as some commentators make it appear—"Actually, it is at once wonderfully ambiguous and incoherent" (Blaug, 351)—; (b) in my view, the position Friedman does defend in the essay has not made itself sufficiently heard in recent times; and (c) the only chapter in the book that explicitly deals with Friedman's 1953 stance (Mäki, 90-116) makes an utterly implausible case that the essay can be read (or 're-read', or 're-written'; see the title of Mäki's paper) as a statement of realism. Let me go through these points in turn.

Ignoring labels for the time being, there can be no doubt about some of Friedman's 1953 methodological ideas. They can easily be summarised in two prescriptions. The first prescription is that the aim of 'positive' economics—along with the philosophical climate of his time and most economists up to this day, Friedman believed in a strict dichotomy between a realm of economic 'facts' and another one of 'values'—is to devise theories or hypotheses that successfully predict economic phenomena within some domain of relevance (i.e., economists ought to conjecture such theories or hypotheses). The second prescription is that economic theories or hypotheses ought to be evaluated on the basis of the significance of their assumptions and not their descriptive accuracy.

The qualifier 'within some domain of relevance' of the first principle is necessary to make the two principles coherent because an assumption implies itself. If, for instance, some theory *assumes* that businessmen maximise expected revenues (Friedman, 21) whereas in fact they price at average cost (p. 22), the theory can be taken to *predict* that businessmen maximise expected returns, which, being incorrect, would invalidate the theory. But once a domain of relevance is identified (for Friedman in this case market prices and quantities), assumptions and predictions can be distinguished.

The second principle has a positive and a negative part. To start with the latter, Friedman thinks that the fact that an economic theory contains false assumptions does not by itself speak against the theory. It is a methodological truism that false theories can be predictively accurate—Tycho's geocentric system saved the phenomena no less than Copernicus's heliocentric system for instance (McMullin 2009). To argue that a theory is inadequate *because* it contains false assumptions means therefore to commit a methodological fallacy. However, that does not mean that the assumptions are irrelevant for evaluating a theory or that theories are to be evaluated *only* with respect to their predictive success. Rather, and this is the positive part of the second principle, assumptions should be 'significant', by which Friedman means they should "explain much by little"—i.e., be simple and fruitful at the same time (Friedman, 10). When he criticises the theory of monopolistic competition for instance (p. 34ff.), Friedman never talks about its predictive success. He rejects it because there is an alternative theory (neoclassical economic theory) that is based on simpler and more fruitful assumptions. Thus, providing both theories are equally 'valid' (p. 8f.)—equally predictively successful—the neoclassical theory is preferable.

This is not the place for a full-fledged defence of Friedman's methodology. But since, as mentioned above, nearly all philosophical commentators have been highly critical, let me suggest at least one reason why his position might not be quite as unattractive as many philosophers and methodologists have made it look.

The slogan "Essentially, all models are wrong, but some are useful" (Box and Draper 1987, 424) has often been quoted, in economics and in many other sciences. Many sciences are heavily model-driven, and economics is no exception. Models are false by their very nature. Rather than sets of statements, models are representations of their targets. All representations must, on pain of utter uselessness, simplify, abstract, approximate, idealise, and what have you.

Those who think that truth is the aim of economics, or that understanding economic phenomena is its aim and only true accounts provide genuine understanding, have difficulty coming to terms with this fact. For not all the ways in which a typical model distorts reality are equally harmless. In rare cases, one can ignore an idealisation because although it is literally speaking false, it is still approximately true because the idealisation makes a negligible difference. But many models, especially in economics, are better described in the following terms:

> A model may give a *totally wrong-headed* picture of nature. Not only are the interactions wrong, but also a significant number of the entities and/or their properties do not exist (Wimsatt 2007, 102, emphasis in the original).

I will say a little more below about the kinds of examples Friedman discusses. What should be clear is that to the extent that such models play an indispensable role in economics, as almost everyone agrees they do, those who think that economics should aim for more than Friedman-style usefulness have a lot of explaining to do.

Back to Friedman. As long as it is understood that the above two principles form the core of Friedman's methodology, it does not matter a great deal what label one attaches to it. Almost every label that has been proposed captures some aspect correctly but is at the same time somewhat confusing because of the connotations it brings with it. 'Instrumentalism' correctly captures the idea that lack of descriptive accuracy in a theory's assumptions is not a reason to reject it, but at the same time suggests that 'anything goes' as long as the theory predicts successfully, which is not Friedman's position. 'Positivism' correctly captures Friedman's emphasis on prediction at the expense of explanation (he puts the latter term in scare quotes whenever he uses it), but suggests an epistemic concern with unobservables that Friedman does not have. 'Pragmatism' correctly captures Friedman's aiming at practical usefulness rather than truth and the central role user interest or purpose plays in his methodology, but it suggests a denial of the fact-value dichotomy, in which Friedman was a firm believer. 'Fictionalism' correctly captures the idea that for a theory to be useful it does not have to be literally true, but it ties Friedman's methodology to a little known and relatively obscure work of philosophy (Vaihinger 1924; but see Fine 1993).

All the just mentioned philosophies are quite closely related, however, and have one thing in common: they are anti-realist. Realism is their common enemy. In this light, it is all the more astonishing that the only chapter in the book that is fully devoted to Friedman's 1953 methodology tries to present it as a statement of realism (Mäki, 90-116). How can its author, an accomplished methodologist, make such a mistake?

Key to the misinterpretation may be Friedman's continued use of a physics example to illustrate methodological points. The law that predicts that the distance travelled by a falling body is $s = \frac{1}{2}gt^2$ (Friedman, 16) is, when applied to a compact ball dropped from the roof of a building, literally speaking false, because it assumes that the body falls in a vacuum. But since air resistance makes a negligible difference *for this application*, the hypothesis is useful nonetheless. Moreover, even when air resistance makes a non-negligible difference, for instance, when the falling body is a feather rather than a ball (p. 17), the hypothesis is useful because it predicts the *contribution gravity makes* to the fall. Gravity, like other forces in mechanics, continues to contribute to outcomes even when its operation is impeded by other causal factors such as air resistance.

But the mechanical example is exceptional and therefore misleading as an illustration of Friedman's methodological points. The test case for his principles is the economic hypothesis that firms behave as if they were rationally seeking to maximise their expected returns (p. 21)—after all, he wrote the paper in response to the marginalist controversy (Backhouse, 235ff.). But 'maximising expected returns' is not analogous to 'being subject to $f = m\mathbf{a}$' for at least three reasons. First, the maximising hypothesis does not have the right form to be a hypothesis about a causal factor that continues to contribute to an outcome in the presence of impeding causal factors. A businessman whose pricing decisions are partly determined (say) by a fairness norm does not maximise returns, not even approximately. One either maximises or one does not, maximising a little is like being a little bit pregnant.

It is easy enough to come up with related hypotheses that have the right form. It is not incoherent, for instance, to say that businessmen seek both wealth and fairness. Economists then might focus on what happens when the wealth motive operates unchecked by other motives. The problem with this suggestion is that, by and large, what economic factors do depends on the whole setting in which they are embedded.

To talk about what gravity were to do if it operated all on its own makes sense because situations can be created in which gravity does operate all on its own—or very nearly so. By contrast, to ascribe a wealth motive to businessmen is nonsensical unless certain kinds of institutional structure are presupposed. Indeed, applying the term 'businessman' presupposes such an institutional setting. In turn, details of the structure in which any motive of action is embedded will influence the behaviour that is caused by the motive. Thus, unlike physical forces, which have a stable contribution to outcomes independently of context, what economic factors do tends to be more context-specific. Therefore, what we learn about how certain motives—such as seeking wealth—operate when other motives for action are absent, even if correct for that situation, tends not to be very useful for predicting what happens in more complex situations.

Third, Friedman thinks that actual businessmen use an average cost pricing rule (p. 22). Suppose he is right. This would mean that the assumption that they maximise revenue is not idealising away other causal factors, but rather portraying a radically different factor, an 'entity or property that does not exist' in Wimsatt's words, as being responsible for outcomes of interest.

On all three counts, therefore, to assume that businessmen maximise returns is to give a totally wrong-headed picture of society. No realist defence of idealisation I can think of can make sense of this part of Friedman's story. And this is the essential part of his story.

Putting aside the fact that there is very little about the methodology of Friedman 1953 in this book, and that what there is is highly implausible to say the least, the remaining essays do contain some interesting and useful material. Dan Hammond recounts the genesis of the essay and how it changed from drafts into the published version in response to comments from other economists such as George Stigler. Thomas Mayer tries to answer the question of whether the essay caused the changing appearance of economics in the second half of the twentieth century (the 'formalist revolution'). Wade Hands asks whether it *licensed* the formalist revolution and in particular who is right between Blaug and Hutchison, who have argued that it did, or Mayer, who has argued that it did not. (Hands's short answer is that Mayer is right.) Melvin Reder assesses to what extent empirical evidence can bear on the neoclassical theory of wage setting. David Teira and Jesús Zamora argue that Friedman proposed his principle that the validity of economic hypotheses is determined by their predictive success as a way to gain the trust of public opinion regarding the claims established by the profession. Roger Backhouse locates the essay in the context of the marginalist controversy of the 1940s. Oliver Williamson writes about the theory of the firm and that it badly needs (but as of lately, also makes) testable empirical predictions. Jack Vromen provides a critical survey of selection arguments in favour of the maximising hypothesis. Chris Starmer contrasts the explicit methodology of the 1953 essay with two 'methodology in action' pieces written by Friedman with Leonard Savage in 1948 and 1952. Kevin Hoover inspects the implicit methodology of Friedman as a practising economist and identifies it as 'causal realist'. Michel De Vroey asks whether there really is a divide between 'Marshallian' and 'Walrasian' economics, as Friedman claimed in a paper written in 1949 (though not in the 1953 paper). Mark Blaug looks at the debate over the essay after 50 years and argues that "Friedman may have won some methodological battles", but "lost the methodological war" (p. 353) because, as he demonstrated in his *Monetary history of the United States* (Friedman and Schwartz 1963) Friedman sought 'thick evidence', that is, a wide variety of different kinds of mutually corroborating evidence, whereas most of the profession contends with narrow or 'thin' econometric evidence. A 'Final word' by Friedman himself concludes the book.

If one understands the book as one on Friedman rather than the 1953 essay, it is quite a pleasure to read.

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Dancing at gunpoint. A review of Herbert Gintis's *The bounds of reason: game theory and the unification of the behavioral sciences*. Princeton: Princeton University Press, 2009, 304 pp.

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The bounds of reason seeks to accomplish many things. It introduces epistemic game theory, discusses other-regarding preferences in games, offers an evolutionary model of property rights, and proposes a plan to unify the behavioural sciences. Most notably, it is a plea for the importance of human nature and sociality for the determination of strategic behaviour on the one hand, and a defence of traditional decision theory on the other.

Being *normatively predisposed* by their nature, human players accept social norms as correlation devices that *choreograph* a correlated equilibrium. While social norms put on the dance, epistemic game theory is driven by the "cannons of rationality" (p. 83), as Gintis puts it in one of the many and sometimes hilarious misprints. Traditional decision theory is "mostly correct" (p. 246), and Gintis relies largely on its support for solving games. Thus the choreographer is restricted to where the cannons cannot reach. Game players are dancing at gunpoint here—with important consequences for the proposed unification of the behavioural sciences.

But I am jumping ahead. The main part of *The bounds of reason* concerns a decision-theoretic approach to game theory. Its purpose is to investigate the (Bayesian) epistemic basis for central solution concepts, both as a justification of what is reasonable, as well as a derivational basis for predicting what is actually observed. This Bayesian rationality forms Gintis's "cannon of rationality", which he aims at various game theoretic solution concepts.

The first victim of this artillery is the assumption of common knowledge of rationality (CKR). Gintis argues that CKR is neither derivable from Bayesian rationality nor can it be epistemically justified on its own. It therefore cannot function as a premise of game theory, but must rather be interpreted as an "event" that may or may not occur

(p. 100). This argument wreaks two kinds of collateral damage. First, rationalizability in normal form games loses its epistemic justification. Without CKR, players do not necessarily eliminate all unrationalizable strategies. This is indeed a relevant possibility, as Gintis illustrates with a number of intuitive and experimentally supported cases. Second, subgame perfection is undermined. With CKR demolished, no alternative epistemic justifications of subgame perfection are available (p. 120), "it is reasonable to assume Bayesian rationality, avoid backward induction, and use decision theory to determine player behavior" (p. 112).

Gintis's next target is the Nash equilibrium (NE) itself. The sufficient epistemic conditions for NE in games with more than two players are common priors and common knowledge of conjectures. Gintis employs two different ordnances to destabilize these foundations. First, using modal logic, he argues against the claim that any event self-evident to all members of a group is common knowledge. This is true only, he shows, if one assumes that the way each individual partitions the universe is known to all (pp. 152-153), but that is a much stronger assumption and more likely not satisfied. Second, he argues that the sufficient conditions for NE are a kind of agreement theorem à la Aumann (1976). Agreement theorems of this sort have implausible implications, for example that rational, risk-neutral agents with common priors and common knowledge of posterior probabilities would not trade assets. Thus, Gintis concludes, common knowledge (or common priors or both) are widely violated, putting the applicability of the Nash equilibrium in doubt.

Having thus established the field of fire, the dancing can begin. The tune is set by Aumann's *correlated equilibrium*, which Gintis considers "a more natural solution concept than the Nash equilibrium" (p. 44). The idea is that an existing game is expanded so that "Nature" first gives a publicly observable signal. Players' strategies assign an action to every possible observation. If no player has an incentive to deviate from the recommended strategy—assuming the others do not deviate—the distribution is called a correlated equilibrium. Nature, then, is the choreographer who guides players' choices in areas where the cannons cannot reach.

Correlated equilibrium only requires rationality and common priors, while Nash equilibrium requires stronger premises. But where do the common priors come from? Drawing on Vanderschraaf's (1998) analysis, Gintis defines a *symmetric* reasoner as an agent who can infer, from his own conclusions about a state of affairs, the conclusions of other players. In a group of Bayesian rational symmetric reasoners, mutual knowledge of an antecedent implies common knowledge of the conclusion (Theorem 7.2). Presumably (although this remains vague) the possibility of a symmetric reasoner depends on social properties like cultural norms. Thus, it is social properties that make common priors possible.

However, if the correlated strategies involve multiple strategies with equal payoffs, then players have no incentive to follow the choreographer's instructions. This is where social norms come in more explicitly: if norm-conforming behaviour is a correlated equilibrium, then players will choose the corresponding correlated strategies (Theorem 7.3). Gintis bases the main message of the book on these two results, namely that the decision-theoretic approach to game theory is incomplete: it requires more than "mere player rationality" to solve (at least some) games. Where the cannons cannot reach, ballet is supposed to lead the way.

Gintis stresses the various methodological implications of this result. First, he argues, this implies the rejection of methodological individualism: agent behaviour depends on social emergent properties—common priors and common knowledge—that cannot be analytically derived from a model of interacting "merely rational" agents. Second, reason is "socially bound" by the existence of social norms that cannot be explained by individual rationality itself.

With game theory thus circumscribed, Gintis proposes a unification plan for the behavioural sciences. He identifies four incompatible models: the psychological, the sociological, the biological and the economic, "all four […] flawed" (p. 221). Out of this flawed mess, Gintis sets out to forge a correct whole. Maybe not surprisingly, decision theory forms the core of this unified approach. Gene-culture evolution and socio-psychology are to detail the shape of the utility function, sociology is also supposed to explain the existence and form of the normative choreographer, and complexity theory deals with emergent properties.

I find this proposal for unification not very convincing. Gintis shoots wide, leaving out so many details that it is difficult to see what a unified behavioural science would look like. How, for example, is complexity theory to deal with emergent properties? The author does not say, but apparently could not resist throwing in buzzwords like these, either.

Further, if the four models are incompatible, how does Gintis seek to make them compatible? He only says that in the unified discipline, sociological and economic "forces" will complement each other (p. 242). But such a divided-domain perspective is not so new. Mill (1844 [1836]) long ago characterised economics as investigating certain causal tendencies; the result of these partial investigations had to be synthesized with investigations from other disciplines in order to explain or predict real world phenomena. The question remains how these forces are to be properly delineated.

But maybe one should instead interpret Gintis as proposing a division of labour. For example, as Gintis suggests, gene-culture evolution and socio-psychology are to detail the shape of the utility function, which decision theory then bases its work on. But that again is not news—economists have lived with this so-called Robbins-Parsons division of labour for most of the latter half of the twentieth century (Hodgson 2008). My impression is that this division has become increasingly obsolete, both because economists themselves have become more interested in the form of the utility function, and because some psychologists and sociologists have moved away from seeing utility functions as central to behavioural explanations. Indeed, most of the empirical research into the form of the utility function over the last 20 years or so has happened *within* economics, not in sociology or psychology. Furthermore, there is considerable controversy about this research. Some economists insist on expanding the utility function. Gintis, for example, believes that "internalised norms are arguments in the preference function that the individual maximizes" (p. 233), and he thinks that this research will yield "a pattern of human attributes that can likely be subjected to axiomatic formulation much as we have done with the Savage axioms" (p. 144). Yet even some of Gintis's behavioural colleagues are cautious about attributing such "individual propensities" (Loewenstein 1999, F31), and instead suggest associating behavioural traits with certain contexts. Others have argued that the attribution of fairness preferences and similar is not borne out by the empirical data, and that sensitivity to norms cannot be explained by including new terms in the utility function (Bicchieri 2006). Thus, Gintis's vision of unification is likely to meet resistance even in his home science, economics.

This holds *a fortiori* for psychology and sociology. Many evolutionary psychologists, for example, prefer explaining behaviour as the result of context-dependent, adapted heuristics, rather than as the outcome of the optimization of a utility function under constraints (Gigerenzer and Brighton 2009). Gintis brushes these differences aside as mere preferences for procedural over as-if models (p. 236), but the difference cuts deeper than a mere question of realisticness. First, focusing exclusively on the model that best fits the data increases the danger of 'overfitting'. The more flexible a model, the more likely it is not only to capture the underlying pattern in the data, but also unsystematic patterns such as noise. Thus, it may be methodologically prudent to restrict oneself to parsimonious procedural models rather than to as-if models with a large number of free parameters. Second, when deriving normative conclusions from decision models, the way deliberation procedures are represented often matters. Gintis himself stresses this for the case of epistemic game theory, where a definition is deficient because it "does not tell us how to find the set that satisfies it" (p. 91, see also p. 195); but he apparently applies different standards for the underlying decision theory. Thus, many psychologists and sociologists may not be willing to accommodate themselves to Gintis's unification proposal: they may think that Gintis overstates the reach of his cannons, and refuse to limit their dance to those few areas where Gintis does not claim firing rights.

This brings me to Gintis's view of the status of decision theory itself. When asserting the correctness of decision theory, it is not clear whether Gintis means this in a normative or a descriptive sense. When discussing decision and game theory in the book, he refers both to "plausibility" (p. 90) and "common sense" (p. 109), as well as evidence from behavioural experiments. At least in a descriptive sense, the correctness claim is controversial. Over the last few years, mainstream economists have redoubled their efforts to rationalize choice that violates the weak axiom of revealed preference (WARP) (e.g., Bernheim and Rangel 2009; and references therein). I cannot see how genuine WARP violations can be compatible with classical decision theory, unless one gives up on the idea of revealed preferences altogether.

Gintis may be inclined to do so, since he suggests that preference inconsistencies can be resolved by using a "more complicated choice space" (p. 9)—i.e., including more parameters in the utility function. This sits well with Gintis's professed as-if perspective of mental models: optimization models are only employed to describe behaviour, not to make claims about the actual psychological set-up of agents (p. 236).

But it chafes uncomfortably against the claim that economic models are supposed to be "testable" (p. 129), and that game theory follows the "hypothetico-deductive method" (p. 223). Without tight constraints on how the choice space can be re-described, how can one test these models? The danger is that re-description continues until all existing data is fitted, and then the form of the utility function has become so weighed down with parameters that no meaningful tests are possible anymore.

Gintis further offers an evolutionary defence of decision theory, yet the few references he gives model the evolutionary context under highly specific conditions. The danger here is always that such models present just-so stories without sufficient robustness. But just take Gintis's own case for transitivity: an organism with an optimized brain, he argues, chooses transitively. That is, if that organism, choosing between pairs of alternatives, chooses A over B, and B over C, then it will also choose A over C when both are available (p. 235). But at the same time, Gintis stresses the dependence of preferences on contexts and current states. This should then also hold for evolution: when choosing between A and B, or B and C, the selective pressures may be different than when choosing between A and C. Hence natural selection does not necessarily entail preference transitivity.

With the fire power of decision theory seemingly somewhat less than Gintis claims, the rationale of epistemic game theory may shift. Gintis presents the reader with a strong contrast between well-founded Bayesian rationality and the baseless assumptions of classical game theory. Recall his conclusion that CKR is "an event, not a premise". Presumably, this means that CKR is sometimes false of real-world situations, and because premises must be true in general, CKR cannot function as a premise, and should be discarded from the game-theoretic toolbox. But if decision theory itself is not as well-founded as claimed, then presumably assumptions like preference transitivity cannot function as premises, either. But that would be an absurd conclusion. Rather, it seems that Gintis is operating with a very narrow view of modelling methodology, in which model assumptions must be true to be acceptable. Given that the contrast between decision theory and game theory in this regard is less than claimed, a more nuanced methodological perspective would be preferable.

Finally, reading the book left me somewhat confused about how important a role is assigned to social norms and hence to the choreographer. Social norms are commonly understood to often go against individual benefits. Yet in Gintis' view, social norms function as correlation devices only if they signal strategies that are *best replies* for all players involved (Theorem 7.3). This is certainly not the only way to deal with the interaction of social norms and game theory. Cristina Bicchieri (2006, 3), for example, suggests that social norms transform mixed-motive games into coordination games. But it may be the most conservative one, leaving a large range for decision theory and letting the dancing happen only where its ordnance does not reach.

To conclude, this is an ambitious project, and an exciting one. Gintis draws on many different strands of research, and presents interesting findings that will be new to many social scientists. Yet in order to support his main thesis, he sometimes oversells the confidence we can have in these theories and their results, and he gives short shrift to alternative perspectives that would seem relevant. In a book that essentially argues for the unification of the behavioural sciences, such one-sidedness appears to be a major weakness, as one could suspect that the proposed division of labour is mainly determined by the author's personal predilections. Nevertheless, it is an important and courageous attempt, and a starting call for more research in this direction. May the dance continue!

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Review of Jesper Jespersen's *Macroeconomic methodology: a post Keynesian perspective.* Cheltenham (MA): Edward Elgar, 2009, 272 pp., and of Luigi Pasinetti's *Keynes and the Cambridge Keynesians: a revolution to be accomplished.* Cambridge and New York: Cambridge University Press, 2009 [2007], 412 pp.

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Since its rise to prominence in the late 1970s and early 1980s, many economists have been discontented with 'representative-agent' macroeconomics. Neither its policy implications nor the assumptions on which it is based seem credible in the light of the stagflation of the 1970s; persistent high unemployment—especially in Europe—in the 1980s; the repeated financial crises in many parts of the world during the 1990s; or, of course, the financial crisis of 2007-2008. By constructing models in which co-ordination failures were impossible, a necessary consequence of the combination of far-sighted representative agents (quite apart from the absence of any real role for something as basic as money) the creators of such models seem to rule out the possibility that they will ever explain what drives capitalist economies from one crisis to another. The door would seem to be wide open to new methodologies for doing macroeconomics, such as these two books claim to provide.

Reading either of these books, the macroeconomist is confronted with a clear choice: either to reject virtually all the macroeconomics taught in leading graduate schools today, or to be branded as a 'neoclassical' economist, guilty of all sorts of sins. The 'new Keynesian economics', whether emphasising institutionally based labour market rigidities or imperfections of competition, does not offer a way out, for it is seen by both authors as being essentially neoclassical. This is an uncomfortable position for anyone who is unhappy with recent developments in the discipline to be in. Fortunately, as I will suggest, it is a choice that does not have to be made and should not be made.

Following a number of recent post Keynesian writers, Jespersen finds the unity of post Keynesian economics to lie at the methodological level, in the well-known methodology of critical realism advocated by Tony Lawson. This postulates a stratified reality, in which the deepest layer—of causal mechanisms, power structures, and institutional relations—is covered by two other layers, of events and data respectively. It is interesting to note the contrast with the methodological pluralism that used to be taken as characterising post Keynesian economics (such as Geoffrey Harcourt's 'horses for courses'—see, for example, Harcourt and Hamouda 1988). Jespersen's account is also of interest for the way this methodology is linked to the three worlds of Karl Popper, correctly seen as an opponent rather than a supporter of 'positivism'. I will not question the claim that Lawson's critical realism can fit post Keynesianism well. Indeed, my view remains that it is so elastic that a good case can be made for it fitting virtually any approach to economics, even neoclassical economics.

This orientation leads Jespersen to start his account of macroeconomic methodology by laying out the ontology of post Keynesian economics, for the first stage in a critical realist methodology is 'initial ontological reflection', mapping the 'macroeconomic landscape' (Jespersen 2009, 95). This leads into a discussion of uncertainty and the need to model the economy as a whole, and to an emphasis on path-dependence rather than equilibrium. At the risk of oversimplifying the arguments, I suggest that there are important parallels (that I will discuss later) with Lionel Robbins's well known essay (1932): whereas Robbins claimed to deduce all the facts of economics from the assumption of scarcity, Jespersen manages to deduce the Keynesian notion of effective demand from the 'ontological fact' of uncertainty. Effective demand depicts a causal relationship and hence is part of the deep reality of capitalist economies. The world is inherently Keynesian at a deep level. This is, of course, reminiscent of Marx's claim to be laying bare the realities of capitalist society.

In the interests of brevity, I will not debate the details of Jespersen's argument, which covers much that I omit here. Instead, I propose to discuss the premises on which it rests. The first of these is the claim that one should begin with ontology—with 'the fundamental nature of being and reality' (Jespersen 2009, 130, 2n.). Jespersen offers many arguments relating to uncertainty, but they seem to rest either on a belief that this is the way the world is, or on the claim that the world must be characterised by uncertainty (which is of course sharply distinguished from risk). This is reminiscent of the grounds on which Robbins claimed that the economic world was characterised by choice under conditions of scarcity (which is why I drew the analogy with Robbins earlier on). Whether one considers such insights to be 'intuitions' or deductions from what we observe, how do we distinguish between the Robbinsian and post Keynesian views of reality? Intuitions need to be tested, for there are senses in which both Robbins and the post Keynesians are right even though their intuitions seem to lead in very different directions. Perhaps the problem is starting with ontology: maybe these 'deep' objects that apparently populate the economic world should be seen as constructions arising from our theorising and ontology is the worst place to start.

This leads to the second assumption underlying Jespersen's critical realist methodology. He argues that there is a divide between two traditions:

> 1. Methodological individualism and closed system reasoning [...], theoretically rooted in deductivism and logical positivism.
>
> 2. Socially embedded macroeconomic theory based on open system reasoning with a deliberate affinity to reality (the economy as a whole) (p. 96).

Neoclassical economics falls squarely in the first tradition and post Keynesian economics in the second. This raises two questions. The first is whether the divide is achieved through ignoring work that might challenge it. The obvious recent example is George Akerlof's theorising about individuals as social agents. Akerlof's individuals are socially embedded as are the agents that he and Robert Shiller explore through behavioural methods. Akerlof is absent from Jespersen's index, but I feel safe in conjecturing that he would place him on the neoclassical side of the divide, for many of his theories of social interaction rest on assumptions about individual behaviour. That is the result of the methodology Jespersen employs, according to which ontology is fundamental rather than a construction placed upon an economic theory.

In many ways, Pasinetti's (2009 [2007]) book is very different, both in structure and in aim—Pasinetti is more comfortable than Jespersen in claiming the mantle of Keynesianism rather than qualifying it as 'post'

Keynesianism. Yet Pasinetti shares Jespersen's belief that there is an insuperable divide between neoclassical and Keynesian economics. Even more clearly than Jespersen, his book is about going 'Beyond neoclassical economics', the title of chapter eight, the first chapter in the section where he lays out his own production-oriented approach. The argument is buttressed by accounts of past ideas that argue for a methodological break, first between 'mercantilism' and 'classical economics', represented above all by David Ricardo, and second between 'classical' and 'neoclassical economics'. The classical approach focuses on production, in contrast to the focus on exchange found in both mercantilism and neoclassical economics. What is needed, Pasinetti claims, is to return to the classical approach with its stress on production.

A further parallel is that, like Jespersen, Pasinetti sees a layering of theory, if not of reality, that could easily be expressed in critical realist terminology. I would contend that the new classical macroeconomics and real business cycle theory, the clearest case of representative-agent modelling, can also be defended using critical realist methodology. Like Jespersen, both groups consider technology to be part of what Jespersen calls the deep reality, though of course they part company in that Robert Lucas would add tastes as the source of invariant parameters. Pasinetti, influenced as much by Piero Sraffa and the literature on linear production theory, talks about this layering in terms of a 'separation theorem', stating that we must disengage

> those investigations that concern the foundational bases of economic relations—to be detected at a strictly essential level of basic economic analysis—from those investigations that must be carried out at the level of the actual economic institutions, which at any time any economic system is landed with, or has chosen to adopt, or is trying to achieve (p. 275).

The foundational bases are to be found in Sraffa's *Production of commodities by means of commodities* (1960), albeit modified to allow for technical progress increasing the productivity of labour. It is represented in the classical concern for 'natural' prices, which interestingly are seen by Pasinetti to have a normative dimension.

The classical approach might seem poles apart from the Keynesian: in the early years of post Keynesian economics this was the view of many of those who were adopting the label. However the methodological parallel between Pasinetti's separation theorem and critical realism is clear. Paradoxically, given the absence of both uncertainty and dynamic analysis from Sraffa's *Production of commodities*, in Pasinetti's hands the production approach, which might at first sight seem at odds with Jespersen's focus on uncertainty, leads to similar requirements for good economics: theories must be dynamic and recognise the fact of uncertainty. Keynesian economics can be married to the classical approach.

What concerns Pasinetti for most of his book, however, is not developing this paradigm, but explaining why it was not taken up more widely within the economics profession as a whole. The answer, Pasinetti argues, lies in Cambridge (UK) where there was to be found an array of talented individuals who should have been able to create and propagate the new paradigm. His starting point is, naturally, Keynes. His first two chapters argue that Keynes wanted to break decisively with orthodoxy, but that after Keynes an accommodation with orthodoxy took place. The explanation of why the Keynesian revolution was aborted lies, for Pasinetti, in the Cambridge school itself. The generation comprising Keynes's pupils failed to achieve its potential, either in developing the new paradigm or in training a generation that would take over from them. He develops this theme, with some repetition for the pieces were written for different occasions, in highly readable chapters on Joan Robinson, Richard Kahn, Nicholas Kaldor, Richard Goodwin, and Piero Sraffa (on whom there are essentially three essays, albeit numbered as sections of a single chapter).

After the *General theory*, Pasinetti argues, there was a divide between those followers of Keynes who wanted to break with orthodoxy (Kahn, Kaldor, Robinson, and Sraffa) and those who compromised with it to different degrees (such as Roy Harrod and John Hicks). (Harrod and Hicks, though not Cambridge economists, could have formed a powerful force, Pasinetti argues, had they combined with the Cambridge Keynesians.) Not only were they divided, failing to work together to develop the production paradigm as a basis for Keynesian economics (as for example Goodwin and his Cambridge contemporary and close friend, Richard Stone, failed to work together) but they failed to produce a further generation.

This account is fascinating as an insider's view of Cambridge. Pasinetti is surely right to argue that the sociology of the economics profession is important to an understanding of which ideas prospered and which did not. However, there are puzzling features of his account. It makes Cambridge the centre of the world—indeed, at times Cambridge seems to comprise most of the known world—and ignores the profound transformations that had taken place in economics during and since the Keynesian heyday. Surely, after 1945 it was developments in the United States, in places like Harvard, Princeton, MIT, and Chicago, that determined the path the profession would follow. Cambridge was not without influence (many Americans visited regularly and Cambridge had strong connections with MIT), but it cannot be considered in isolation (or even along with Oxford).

Is it right to argue that Keynes's pupils failed to train a third generation to take over, and that Cambridge was simply given up to the neoclassicals? Before concluding that this is the right perspective, I would want to know more about the generation comprising Robert Rowthorn, Robert Neild, John Eatwell, and those who set up the *Cambridge Journal of Economics*. There is also the paradox that one of the key 'neoclassical' economists at Cambridge was Frank Hahn, who was supervised by Nicholas Kaldor, with a thesis on income distribution in the Kaldor-Robinson 'Keynesian' mould. Hahn was, moreover, a harsh critic of the uses of general equilibrium theory that are rightly criticised here; indeed, he was a staunch defender of Keynesian ideas. The example of Stone, whom Pasinetti discusses, and who had clear connections with Keynes, would seem to call for greater questioning of the divide between neoclassicals and Keynesians, as does Goodwin's ability to engage with neoclassical economists. James Meade, a very significant figure at Cambridge in the 1960s, also needs closer examination, for though a self-confessed neoclassical economist, he was also a long-standing associate of Keynes, having been involved in the development of the multiplier and also having worked closely with Keynes during the Second World War.

Post Keynesian economics is, as many post Keynesians acknowledge, a programme that is in need of considerable development. That makes studies of post Keynesian methodology potentially important. But, despite my doubts about representative-agent macroeconomics, neither of these books persuades me that post Keynesians have yet developed a workable methodology. If what are believed to be insights into economic reality are to be of any use, they need to be operationalised, and this seems not to have happened with post Keynesian economics. Hence, I find myself wanting to know more about what post Keynesians do in practice, rather than their fundamental beliefs about what they believe should be done. There is some of this in both books, though primarily as a statement about how post Keynesians should construct macroeconomic theory. Thus, even though methodological stances have moved on, these two books suggest to me that there is still as great an emphasis as ever on creating an identity through defining post Keynesian economics in opposition to a stylized neoclassical economics as there was in the 1980s when the survey by Harcourt and Hamouda (1988) was published.

To me, the fault seems to lie in the belief that ontology is fundamental and that theorising should begin with analysis 'at the essential level', to use Pasinetti's phrase. It is this that leads naturally to the postulation of a fundamental divide between neoclassical and other approaches. One thing that is interesting about Keynes is that, though he clearly did believe he was fomenting a revolution in economic theory (a belief that seems amply justified by events, even if there is room for disputing the details) he was able to work with both those who became seen as post Keynesians and those who are seen by Pasinetti as having compromised: Meade, Harrod, Stone.

If we do need a new paradigm (and I leave open the question of whether this is the right way to think about the changes that are needed in macroeconomics) perhaps it is something that will be recognised only after the event. That would suggest that it would be more fruitful to start, like the new Keynesians who are dismissed in these books, with looking for new ways to solve problems, postponing discussion of ontology to a much later stage (if it is needed at all). I have no doubt, for example, that Shiller's behavioural approach to Keynesian problems has limitations, yet it is surely worth exploring and does not merit dismissing as simply 'neoclassical'. Similarly, Stiglitz (2010) criticises the post Keynesian focus on uncertainty as opposed to risk, not because he fails to understand the distinction, but because he does not see that it plays any role in explaining the events that led up to the recent financial crisis. (It is interesting to note that, as Harcourt recently pointed out, Stiglitz has claimed that he learned much from Kaldor as a student at Cambridge in the 1960s.) It may be that uncertainty is a feature of the economic world and that any system economists are likely to consider is open—sensible 'neoclassical' economists would not dispute either of these points—but the question is how one analyses such an economy.

The analogy with Robbins made earlier, is relevant because his *Essay* perhaps offers a cautionary tale for post Keynesians. Robbins's approach to economics started with ontology—the belief that the world was characterised by individual agents making choices under conditions of scarcity. This led him to a belief in the primacy of economic theory that was inconsistent with his own belief that empirical work was important. In drawing a sharp distinction, of which I was reminded by Jespersen's discussions of layered reality and by Pasinetti's 'separation theorem', between propositions of permanent significance and ephemeral relationships, he was led to neglect the 'middle ground' of relationships that may not be permanent but last long enough to be important in practice: the territory explored by modern econometrics. Critical realists see as much with their talk of 'demi-regularities', but fail—I suggest—to see its full significance.

If my argument is right, the attempt to find methodological unity in post Keynesian economics may, paradoxically, be a step backwards from Harcourt's potentially more pragmatic 'horses for courses' approach, in which the diversity of methods was celebrated, or the advocacy of pluralism of which Sheila Dow is a representative. These, at least, have the potential to challenge the insuperable methodological divide, postulated by both Jespersen and Pasinetti, between neoclassical and post Keynesian economics. Pluralism might even allow an accommodation with 'neoclassical' economists such as Hahn and Stiglitz who share the post Keynesians' admiration for Keynes.

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Review of Nicholas Bardsley, Robin Cubitt, Graham Loomes, Peter Moffatt, Chris Starmer, and Robert Sugden's *Experimental economics: rethinking the rules*. Princeton: Princeton University Press, 2009, 384 pp.

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Experimental economics has brought about the most extraordinary changes to economics. Not so long ago the economics profession simply could not see the purpose or relevance of laboratory experiments, but over the past thirty years their number has grown continuously and experimental economics has become one of the most exciting fields of economics.

As is often the case with new areas of research, methodological reflection has lagged behind the rapid growth in, and the various applications of, experimental tools and results. Methodological debate may have been further restrained by the strong scepticism toward laboratory experimentation in economics: experimental economists may have felt they had to wait for more favourable timing to openly address legitimate critiques and acknowledge the limitations of the experimental method.

Now that experimental economics is firmly established, the time is ripe for experimental economists to finally address fundamental methodological issues, or else risk prematurely consolidating their methodological conventions around insufficiently debated and scrutinised rules. This concern is the driving force behind *Experimental economics: rethinking the rules* (*EE*). As the subtitle of the collective enterprise suggests, *EE* sets out to offer a critical assessment of the rules of experimental economics, built on the work and reflections of six highly regarded and experienced experimental economists.

This is not to say that readers will come away thinking that experimental economics is an uncontroversial field of research. While a set of common practices can be identified around well-defined and well-established principles and procedures, methodological disputes between practising experimental economists do exist which at times imply more fundamental divergences about the attributes of economics experiments and what can be learned from them. After reviewing the main methodological tenets of experimental economics, the authors indeed conclude that "none of them should be accepted uncritically as part of 'the' methodology of experimental economics" (Bardsley, et al. 2009, 333). That experimental economics does not have a unified and uncontroversial set of methodological rules is not taken as problematic. The authors convincingly argue that experimental economics benefits from a flexible set of rules, which allows experimental designs to be tailored to the objectives of investigation. This is the key message of *EE* and, in my view, the major contribution of this collective endeavour.

EE brings together the various methodological reflections its authors have produced in recent years, resulting in a comprehensive and up-to-date account of experimental investigation in economics. The distinction between the use of experiments as tests of theories and the pervasive, but unacknowledged and unaddressed, use of experiments as tools for investigating empirical regularities organizes the book. Both issues raise specific methodological issues which are addressed in detail. While the use of experiments as tests of theory calls for closer examination of the relation between experiment and theory (chapters 2 and 3), the use of experiments as tools for investigating empirical regularities requires more careful analysis of the relation between the laboratory and the real world environment to which empirical observations potentially apply: the 'external validity' of economics experiments (chapters 4 and 5). Two additional topics are discussed in separate chapters: the use of task-related incentives to induce economic motives in experimental subjects, probably the most rigid convention of experimental economics (chapter 6); and the statistical analysis of experimental data, perhaps the most neglected issue in methodological discussions (chapter 7). *EE* presents the major methodological questions pertaining to experimental practice in a clear and accessible way, illustrating the issues at stake with various case-studies from experimental economics while offering the authors' position on ongoing debates, except when the authors fail to obtain a consensus position among themselves, providing further evidence of the contentious nature of experimental economics.

Economics experiments have been prolific in generating so-called 'anomalies', i.e., patterns of judgment and choice that are inconsistent with the traditional model of utility maximisation and the neoclassical assumptions of unbounded rationality, unbounded self-interest, and unbounded willpower. Economists have since introduced amendments to standard rational choice theory to account for such observed behaviour, for example by introducing revisions to the axioms of expected utility theory to make the demands of rationality less stringent, or by introducing other-regarding motives into individual utility functions. A different strategy downplayed the relevance of these results to economic theory, arguing that the experiments that produce the challenging results do not belong to the domain of economic theory: contexts where decision-makers have incentives to deliberate and have opportunities to learn by experience (e.g., Binmore 1999).

Bardsley and his co-authors present a framework for addressing such contentious issues around the implications of experimental tests for theory (pp. 64-71). The goals are twofold: to promote laboratory tests by extending the testing conditions for theory; and to promote adequate interaction between experiment and theory by imposing restrictive conditions on admissible responses to disconfirming tests. The authors argue that any laboratory environment that fits within the "base domain" of an economic theory (defined by the possible phenomena to which an application of the theory seems reasonably unambiguous) should be presumed to provide legitimate testing conditions for that theory (e.g., a theory that refers without qualification to choice under uncertainty is held to apply to any choices experimental subjects make in the laboratory in conditions of uncertainty). Laboratory environments are particularly convenient because they can be purposefully designed to fit within the base domain of relevant theories, establishing a direct correspondence between laboratory constructs (e.g., experimental lotteries) and the formal concepts of the theory (e.g., prospects in expected utility theory).

The laboratory can no longer be expected to offer adequate test conditions if it differs from the "intended domain" of a theory (defined by the phenomena the theory is deemed to predict or explain). For example, tests of equilibrium predictions that specify equilibrating mechanisms, say arbitrage, must implement them, otherwise they fail to belong to the theory's intended domain. But, the authors stress, disconfirming evidence cannot be dismissed by simply pointing out that the laboratory conditions do not fit the intended domain of the theory. Reasons must be given as to why differences between the laboratory and the intended domain of the theory should be relevant, which must be suggestive of new testable hypotheses (p. 77). If empirically supported, defenders of a particular theory must accept the subsequent contraction of its domain of application. Experimental tests beyond a theory's intended domains are nonetheless encouraged because they allow us to better map and understand the contexts where a theory succeeds and fails. The authors then apply the framework to cases of responses that have downplayed the relevance of disconfirming evidence, pressing economists to carry out these tests and acknowledge the implications of their defences for the domain of application of standard economic theory.

The discussion of theory testing is placed within the framework of the Lakatosian methodology of scientific research programmes (MSRP). Following the descriptive and prescriptive functions of the MSRP, the authors organise experimental work in the larger frame of scientific research programmes, which allows for defining experimental research programmes according to underlying commitments and conventions, and propose the Lakatosian prescriptions for experimental economics. While the authors acknowledge that the performance of research programmes is not and cannot be fully captured by the Lakatosian criteria of theoretical and empirical progress demanding the successful prediction of novel phenomena (pp. 106-114), these standards are taken as generally valid prescriptions to deal with scientists' inevitable a-critical attachment to a set of fundamental presuppositions (a programme's 'hard core'), and thus the risk of scientific communities "slipping slowly from science to prejudice" (p. 139). The authors then analyse research on individual decision-making under risk along these lines, and conclude that "the experimental method has played an effective and positive role in challenging existing theory, and enriching the evidential base against which theories can be judged" (p. 139). And they consider that the effective interplay between theory and experiment is "a common and very positive characteristic of all the major programmes of experimental research in economics" (p. 139).

No doubt the assessment of the role of experiments as tests of theory must focus on the fruitfulness of the dialogue between theory and evidence, and on the role of underlying commitments therein given the well-known difficulties entailed by the Duhem-Quine thesis that undermine the confirming (or disproving) force of empirical tests. However, the appropriateness of the MSRP as a prescriptive framework is problematic for reasons already identified in non-experimental research programmes, such as the arbitrariness involved in the definition of scientific research programmes and the exclusive focus on 'novel facts' to measure scientific progress.[1] Indeed, as the statements quoted above suggest, the authors' appraisal of economics research programmes is based on a somewhat flexible examination of the relation between experiments and theory rather than on a careful and exhaustive identification of the actual novel facts discovered in economics labs.

The insufficiency of the Lakatosian criteria of progress is also patent in the concluding chapter of *EE*, where the overall positive appraisal of experimental research is based on its contribution to the revision of economists' most ingrained beliefs—namely the status of rationality assumptions—which has increased economists' interest in building more realistic models of economic behaviour; and the overall contribution of the experimental method to initiating the transformation of economics into an empirical science (pp. 343-344).

A tension thus informs *EE*. While concerning themselves with economists' long-term attachment to background assumptions, Bardsley and his co-authors do not spell out the detrimental impact that economists' pre-commitments have had on experimental economics. Regarding the conventions of experimental economics, in particular, although on the one hand experimental economics is taken to use an insufficiently debated and scrutinised methodology, on the other hand its practice, both in theory testing and in the investigation of empirical regularities, is deemed to have been fruitful.

It might be argued that the conventions of experimental economics may have been adequate to carry out particular research programmes, but that designs that deviate from these standards have nonetheless been implemented and that it has been the latter which have contributed most to the revision of economists' most ingrained beliefs and to transforming economics into an empirical science. But this claim is not made. One can then but wonder about the urgency of revising experimental economics rules and of the plea for a more methodologically pluralist experimental economics.

Even though early experiments had theory testing as their stated goal, their results inspired the design of novel experiments to explore the new phenomena produced by experimental means. Gradually the discipline started "to treat experimental observations as part of the material that it is to explain", marking a "momentous methodological step" in a discipline that has long been considered as a hypothetico-

[1] See Hands 2001, 286-296, and references therein.

deductive science (p. 167). Economics experiments have in this way acquired a life of their own, generating a list of 'stylized facts' which are now being used as an empirical basis for the (re)construction of economic theory. In sum, experiments have become what Bardsley and co-authors call "exhibits", i.e., replicable experimental designs that reliably produce interesting results (p. 156).

Experimental economics has by now a substantial list of exhibits and associated regularities. As a result of experimental research, economists' practice is thus shifting from highly abstract and formal theorizing towards empirical investigations, which need not be understood in relation to some pre-existing theory and whose results can be organized as experimentally observed robust regularities (p. 195). But while exhibits are more autonomous from economic theory than experimental tests, they must establish a closer relation with the world outside the laboratory. The use of experiments as tools for investigating empirical regularities requires that experimental economists be able to justify the relevance of the regularities observed in the simple and artificial circumstances of the laboratory for improving our understanding of real world phenomena, i.e., the external validity of economics experiments.

This topic has been neglected by the pioneers of experimental economics, who have evaded the issues at stake by focusing on the testing role of experiments. They have claimed, in what *EE* labels the "blame-the theory" argument (p. 155), that the unrealistic features of the laboratory (i.e., the lack of external validity) are ultimately attributable to the theory under test because an experiment must be at least as 'realistic' as any theory is.

Even though the orders of abstraction of economic theory are much higher than those of economics experiments, where experimental participants engage in particularly interesting economic problems, the laboratory is necessarily a simple and artificial social context. The simple and artificial conditions of the laboratory offer particularly convenient circumstances for scientific inquiry because they allow experimenters to manipulate and shield their objects of study from the interference of factors that may have an effect on, but are not part of, the study. In fact, it is the high control that economic experimenters can exert over laboratory conditions that allows them to create situations in the base and intended domains of economic theories and thus to test them. But this control may be problematic in inductive inquiry, for it may render the laboratory worlds substantially different from real world environments.

Bardsley and co-authors recognize that an economics experiment is a fairly simple and artificial situation and discuss in great detail various types of artificiality (e.g., isolation, omission, contamination, and alteration) and suggest how to circumvent some of them. The artificialities of omission and contamination, for example, are not taken to be particularly problematic because they can be dealt with in experimental design, by adding or eliminating the omitted or the extraneous factor. The artificiality of alteration poses a more difficult challenge, however. While the critiques of isolation, omission, or contamination question the influences of the laboratory on the object of study, the criticism of alteration questions whether the object of study can actually be observed in the laboratory (p. 226).

The authors recognise that the laboratory may be inadequate to study some classes of phenomena. They give the example of relational phenomena, which depend on relations with other phenomena and on people's perceptions that those relationships are satisfied. This is the case for tax compliance and evasion, which evokes a relation between citizens and government permeated by citizenship duties which cannot be recreated in the lab. Even though experiments can never bear on the nature of the relation in question (e.g. citizenship duties), they may still provide some useful insights into these kinds of phenomena. Experiments that replicate the analytical structure of the decision-problem (e.g., requiring subjects to report their endowments on the basis of which they pay experimenters a 'tax') may improve our understanding of the problem-situation (e.g., perceptions about the probability of being caught under-reporting). Thus, while the characteristics of laboratory experimentation constrain the kind of social phenomena that can be investigated by experimental means, the relevance of economics experiments is ultimately an empirical issue and, relying here on the work of Francesco Guala (2005), one that may require establishing the quality of the experimental analogy and checking the similarity between the lab and the real world situations to which experimental results are supposed to apply (pp. 234-235).

But a careful justification for the use of experiments in inductive science is still missing. Bardsley and co-authors do not put forward an argument that justifies the ability of economics experiments to provide meaningful knowledge of real-world situations, and thus the use of economics experiments in inductive inquiry, as they do for the use of experiments in theory testing. They do not spell out what in their view are the epistemic attributes of experiments that allow economists to learn about real world economic behaviour. This is somewhat unexpected given the overall optimistic tone regarding the desirability of an inductive turn in economics and the role of economics experiments in bringing about such a change. The detailed analysis of the various sources of artificiality nonetheless provides rich material for those who might be interested in further exploring the still most challenging issue of experimental economics: the possibility of learning about real world economic behaviour from laboratory experiments.

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This volume derives from Chao's 2002 University of Amsterdam doctoral thesis. Chao contrasts the received and the semantic views of theory in economics and comes down clearly in favour of the latter. He illustrates his arguments with discussion of demand theory, the consumption function, and the so-called LSE approach to econometrics. The account is commendably (perhaps overly) concise and is generally clear. However, it betrays both its vintage and its thesis origin. In this review, I discuss Chao's views on structure primarily in relation to demand theory.

The received view sees theory as consisting of a set of abstract axioms plus a set of correspondence principles which link the axiomatic relationships to the world. So-called modern (i.e., pre-behavioural) demand theory appears to conform to this paradigm—the theory consists of a set of axioms defining the relation $\succcurlyeq$ and a correspondence rule which interprets $\succcurlyeq$ in terms of choice. Unfortunately, the remainder of the economics corpus fits the received view less well. The semantic view is not susceptible to such a precise characterization. It relates to a broad collection of less abstract approaches to the role of theory in which theory and structure become closely related concepts. Theory posits a structure, or a range of structures, to which the world corresponds, either isomorphically or by analogy. Models have structure and their structures purport to represent the structure of the world—hence the title of the book.

Models, and hence implicitly also theories, are partial accounts of a complicated reality and therefore necessarily simplify. Model structures can therefore only aspire to being partial representations, in the same way that a road map is, by design, a partial representation of the terrain. Alternative partial representations are possible—road maps and topographical maps offer different representations, each of which has its own validity. Even if it were desirable, an isomorphic correspondence between the world and theory would be unattainable and Chao is therefore right in preferring an analogy-based account of the semantic view. However, this leaves open how we establish whether a simple theoretical structure does indeed represent an unknowable and probably complicated structure.

The experimental sciences (not explicitly discussed by Chao) finesse this problem by aligning the world with theory through the creation of controlled environments. By simplifying the world, analogy approaches isomorphism. Economics remains largely non-experimental and the literature Chao discusses is entirely non-experimental. In non-experimental disciplines, whether astrophysics, economics or meteorology, we are obliged to analyze the data generated by 'nature's experiments'.

Chao sees Richard Stone's (1954) paper on the linear expenditure system (LES) as defining the birth of modern demand theory. He quotes Louis Phlips (1983) as stating that it is only once the restrictions imposed by theory have been imposed that an equation relating quantity purchased to income and prices can be recognized as a demand equation: "Economists have realized that *a function that does not satisfy the Slutsky conditions is not a demand equation*" (Phlips 1983, 56, emphasis in the original).[1] On Phlips's view, apparently endorsed by Chao, Stone was indeed the first economist to qualify as doing demand theory. The claim is absurd.

Stone made contributions of the first order of importance to demand analysis, but he was continuing the programme initiated by Harold Schultz and set out in the final chapter of his monumental *Theory and measurement of demand* (1938). Schultz saw himself as building on the work of his teacher Henry Ludwell Moore. The largest part of the empirical analysis in that book takes the form of regressions between appropriately transformed variables (detrended or differenced). The Slutsky condition arrives only later in the book where Schultz remarks: "The attack on this problem need not be wholly empirical" (1938, 599). He discusses "difficulties encountered in statistical testing of the theory" (1938, 628-633) in the context of inter-related demand and performs a number of informal tests, comparing estimates of the left and right hand sides of the Slutsky equation. However, he is more concerned with empirically distinguishing between complement and substitute commodities.

[1] The assertion is less emphatic in the 1974 first edition of the book.

When Stone (1954) imposed the Slutsky symmetry condition in the LES, his objective was reduction in the number of cross-price elasticities to be estimated, not theory testing. In a previous discussion (Gilbert 1991, 300), I quoted a 1985 letter from Stone in which he wrote: "I introduced the [Slutsky] condition, which could not be expected to hold rigorously for a community of consumers, as a plausible means of greatly reducing the number of constraints to be estimated". Stone was a user and not a tester of theory. It was only later that testing moved centre-stage, once demand theory was taken over by econometricians, perhaps starting with Byron (1970). Subsequently, this was seized upon by the methodologists as the way economic science should proceed.

Both Schultz and Stone saw the role of theory as that of organizing data and structuring research. Schultz states: "[Theory] is, therefore, ideally suited not only for organizing the masses of accumulated data but also for giving coherence to future investigations" (1938, 663). He goes on to remark that quantitative research will make theory more "realistic" (1938, 665). Demand studies employing aggregate data exploit theory by treating aggregate outcomes as if generated by a representative consumer. As both Schultz (1938, 630) and Stone realized, households are heterogeneous (to use a current term) and aggregate data would therefore be inappropriate if the objective were to test the preference-based theory. But, as both authors stressed, the same theory may nevertheless be useful in structuring aggregative data.

These considerations reinforce Chao's arguments in favour of the semantic approach. He could perhaps have made these arguments more coherently if he had recognized that the three decades following the publication of Stone (1954) gave too much priority both to the role of the axiomatic preference-based theory of demand and to the informativeness of this theory in relation to aggregative data.

Elsewhere, Chao appears sympathetic towards an entirely empiricist approach to structure. He quotes Gustav Cassel (1932, 81) with approval on the law of demand and classifies David Hendry as a closet semantic structuralist. Hendry discusses representation in terms of the congruence of the estimated model with the data generating process (DGP) summarizable in the form $D\left(X_T^1 \middle| X_0, \theta\right)$ (Hendry and Richard, 1982). Hendry advises a battery of tests, including tests for temporal invariance, in which rejections imply lack of congruence. It is possible that there are alternative congruent relationships in which case nonuniqueness may be attained by encompassing tests. Conclusions, of course, remain provisional.

If this were all, it would have to be judged as unsatisfactory. The DGP is itself a construct of the modeller, not least through choice of the sample {1, ..., *T*}, its frequency, the variables of interest *X,* and the implied level of aggregation at which the problem is studied. Reification of the DGP, on the misleading analogy of Monte Carlo experimentation (where the DGP is well-defined and discoverable), makes representation too simple. Hendry's actual econometric practice (as exemplified in Hendry 1993), results in structures which owe much more to standard economic theory than would be likely to arise through the adoption of a purely black box approach, as for implemented in the 'autometrics' module of the OxMetrics™ software with which he is associated.

There is an additional consideration. Economics is a profession as well as a science and much of what many economists write is motivated, directly or indirectly, by professional concerns. Hendry's work on the consumption function and the demand for money, discussed by Chao, was related to the forecasting interests of the U.K. Treasury and the Bank of England. Theory is relevant to forecasting, in particular because it may provide guidance as to when forecasts have or may become systematically misleading, but the testing of theory is an incidental concern in that context. The characterization of the DGP is a useful way to describe the forecaster's intermediate objective even if the DGP is itself a construct of the same forecasting exercise.

Chao finishes his account with the conclusion: "Models are representations; and, more importantly, models aim to represent structures" (p. 134). The first clause of this statement is unexceptionable, but the second seems either tautological or incorrect. Models embody structures with the objective of being informative about the world. That does not imply that the model structure is the same as the structure of the world, whatever that might mean. Reference to the DGP confuses this issue since the DGP is a construct of the modeller. It is important, as Hendry emphasizes, that an empirical model provides a satisfactory statistical characterization of the dataset on which it is based. Nevertheless, an affirmative answer to this congruence question leaves open the more difficult epistemological question of whether, and in what way, the model, and hence also the DGP, represents the world. If we are to answer that question, we need to match the empirical model with a theoretical conception of the economy.

Wade Hands (2001) argues that it can be misleading to suppose that economic science can be discussed in the same methodological terms as the natural sciences. Of course, the natural sciences are themselves diverse and the same arguments show that the methodologies employed in meteorology and biology differ from those in physics. The principal role of theory in economics is that of organizing experience, including data experience. This is not too different from what happens in meteorology. As in meteorology, forecasting is important and, in that context, theory is important insofar as it is useful in improving forecast accuracy. Truth is another matter. Unlike meteorologists, economists are also involved in policy. In this context, we have to understand why things happen as well as to predict what will happen. This involves stronger invariance requirements. Furthermore, whereas forecasts will generally be generated from a single model, policy discussions may rely on a number of competing (complementary and competitive) structures which focus on different mechanisms and rely on different analogies based on different theoretical perspectives. All of this underlines Chao's rejection of the received view of economic methodology in favour of an analogy-based account of the semantic theory. It also forces acknowledgement of the impossibility of a purely empiricist resolution of the representation problem.

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Review of Samuel Gregg's *Wilhelm Röpke's political economy*. Cheltenham (UK): Edward Elgar, 2010, 224 pp.

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The article "German economic miracle" in the *Concise encyclopedia of economics* on the Library of Economics and Liberty website states that Wilhelm Röpke was a leading advocate of currency reform as a way of bringing to an end post-war stagnation and suppressed inflation. His name is coupled there with that of Ludwig Erhard, carrying therefore the strong implication that Röpke was an important contributor to post-war German economic policy. But at the least this is an exaggeration. As regards currency reform, there were hundreds of such proposals in Germany after the war—Hans Möller lists 217 such plans together with a further 24 drafts or references to plans in Appendix II of his *Zur Vorgeschicthe der Deutschen Mark* (1961). As regards the idea that Röpke advanced specific or novel arguments about a currency reform as a means of dealing with suppressed inflation, this "purchasing power overhang" had been discussed in German academic journals during the war, ceasing in 1944 only because a shortage of paper brought an end to the publication of virtually all academic periodicals.

Moreover, the German currency reform was unusual solely by virtue of being the last of many European currency reforms, and also because it was imposed by the occupying allied powers rather than a sovereign government, as with the Belgian, Dutch, Danish, and so forth, currency reforms. While the reform in June 1948 did mark a point after which West German economic recovery gathered pace, in many respects it "succeeded" economically in spite of its terms, rather than because of them. Its prime significance is rather as the formal initiation of the Cold War: its imposition in the three Western zones triggered a parallel reform in the Soviet zone announced the Wednesday following the Friday on which the reform had been promulgated, the Berlin Blockade started the same day, and the formal division of Germany between West and East by the creation of separate civil governments followed shortly thereafter.

The question is not therefore whether Röpke had particular views on free markets and economic policy, but whether there was anything distinctive or significant about such views. Gregg thoroughly summarises Röpke's work and writings, and very usefully provides a comprehensive bibliography. Whether there was anything especially original about Röpke's writings is another matter, but not a question to which Gregg gives much attention. Time and again conventional elementary ideas, not to say platitudes, about free markets, welfare and government activity are presented as if through simple repetition these might become more meaningful. Opening the book at random (on p. 68), we find a discussion of Röpke's view that "mathematics" cannot take proper account of real human behaviour. Gregg's analysis overrates the hold of mathematical reasoning on the discipline of economics in the 1950s, attributes this mathematical approach to "Keynesian economists", and fails to note that, with respect to social and economic statistics, the arguments attributed to Röpke here were very dated by the last third of the nineteenth century. That is three mistaken ideas on one page; ideas which have indeed been repeated down the decades, but which by sheer repetition have gained nothing in veracity.

Another random opening (at p. 79) illustrates a different problem. Here it is suggested that Röpke was not the kind of liberal who thought a market economy to be all that was needed to optimise the human condition. On the contrary, he emphasised that "free economies depend upon an extra-economic framework of moral, legal, political and institutional conditions". The problem is that while this is a perfectly reasonable stance, Röpke never advanced beyond very general statements of this idea. Compare his writing to another contemporary "economic liberal", Ronald Coase, whose arguments were advanced in, for example, detailed studies of broadcasting technologies and legal cases regarding property rights and compensation.

This absence of critical appraisal on the part of Gregg starts right at the beginning of the acknowledgements, with an epigraph from Ludwig Erhard (admittedly not the most reliable of sources when it comes to post-war history) taken from his contribution to a memorial volume to Röpke published in 1967. Here Erhard suggests that "during the most tragic phase of German history" he had "illegally obtained" Röpke's books "which I absorbed as the desert drinks life-giving water" (p. vi). Leaving aside the purple prose, we might first ask: which books were these then? Why might they be "illegal"? Röpke's books were chiefly synthesised from previous publications, flattening any sense of novelty. From the later 1930s he chiefly published in newspapers. His principal academic work *Crises and cycles* appeared in 1936 and was reviewed the same year in the *Economic Journal* by James Meade, who described it as an introductory synthesis that lacked conceptual precision. More telling is the publisher of *Crises and cycles*: during this period William Hodge published a number of economic texts by European liberal economists, many of them suggested to the publisher by Friedrich Hayek. Röpke's 1942 text *International economic disintegration* was also published by Hodge, as was his later 1944 *Civitas humana*, which was mainly a compendium of his journalistic writings. While Erhard was probably referring to Swiss publications of Röpke in German, the provenance of these "books" was more politically suspect than the contents. By contrast, James Meade could be classed as an "economic liberal", but if we compare Röpke's writings with Meade's 1948 *Planning and the price mechanism* we see at once that Meade's liberalism was underpinned by substantial and distinctive economic argument, unlike anything we find in Röpke, or, perhaps also noteworthy, among the overwhelming majority of contemporary German liberal economists.

Röpke was certainly an opponent of National Socialism, and his outspokenness in this led to his inclusion on the list of academics dismissed in 1933 primarily because of their Jewish descent and/or allegiance to the Social Democrats. Röpke was neither Jewish nor a social democrat, but he contrived to get himself dismissed all the same by doing little more than adopting an intransigent stance. Indeed, after the war he took the same kind of stance in stating that he would only return to his former post at Marburg if he were invited to do so. He was not so invited. To be fair, this is as much a reflection on the condition of German economics since the 1920s as anything else: when the new, predominantly Jewish and/or socialist, generation of economists emigrated in 1933 they left behind a rump of an economics profession that could be most kindly described as intellectually undistinguished. This has all been documented by the work of Harald Hagemann and Claus-Dieter Krohn, particularly in their *Biographisches Handbuch der deutschsprachigen wirtschaftswissenschaftlichen Emigration nach 1933* (1999); Adam Tooze's account of interwar economics, *Statistics and the German state 1900-1945* (2001) also demonstrates the degree to which the incumbent German economics professoriate lost the dynamism and originality for which they had been internationally renowned in the mid-nineteenth century.

Röpke became a refugee, but went East, not West. In the autumn of 1933 he was in Turkey, one of several German émigrés recruited to assist in the modernisation of the university system. Few of the émigrés remained longer than a few years, and in 1937 Röpke moved to the Institute of International Studies in Geneva, where he worked for the remainder of his life. He found both the Institute and Switzerland congenial, spending much of his time developing broad critiques of creeping collectivism and modern economics. In 1953 Erich Schneider published a devastating critique of Röpke's treatment of "Keynesianism" which argued that he was out of touch with the relevant literature and had an inadequate first-hand knowledge of Keynes's writings. Gerhard Mauch's balanced assessment of Röpke in Hagemann and Krohn's *Handbuch Bd. 2* suggests that his position was best understood as "liberal-conservative", although more suggestive is the fact that he was president of the Mont Pèlerin Society in 1960-1962.

Samuel Gregg plainly finds Röpke an interesting figure, but despite the care with which he has trawled Röpke's writings he fails to convey to anyone not already convinced of Röpke's significance quite what might be of any especial interest here. Furthermore, although the bibliography to the book is extensive, it refers to no recent critical reassessments of Ordoliberalism. My own contributions to the genre apart, neither Dieter Haselbach's ground-breaking *Autoritärer Liberalismus und Soziale Marktwirtschaft. Gesellschaft und Politik im Ordoliberalismus*, (Nomos Verlag, Baden Baden, 1991), nor Ralf Ptak's definitive demolition of the myths of the social market economy, *Vom Ordoliberalismus zur Sozialen Marktwirtschaft. Stationen des Neoliberalismus in Deutschland* (Leske + Budrich, Opladen, 2004) are included. Nor indeed are any of the other works to which I have referred in this review.

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PHD THESIS SUMMARY: Models in science: essays on scientific virtues, scientific pluralism and the distribution of labour in science.

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PhD in philosophy, April 2010
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In my dissertation I have been concerned with the existence of multiple models of the same phenomenon. A common explanation for this multiplicity is that different models serve different virtues, so the multiplicity disappears once the virtues that are required for a given purpose are made explicit (the consensual view) and the existence of multiple models does not undermine the possibility of a single standard for scientific assessment. I indicate two complications for this view, respectively demonstrating that this view is neither necessary nor sufficient for analysing all scientific controversies.

The first controversy is drawn from economics: neoclassical economics' a priori preference for generality, regardless of the purpose at hand. The second concerns a debate in history, the *Historikerstreit*, from which I drew the conclusion that if the political views and personal interests of scientists coincide with the different sides in a debate, then the question of what virtues should be served by a model is no longer given, but becomes an integral part of the debate.

The shortcomings of the consensual view (Rawls, Giere) in providing adequate guidance in dealing with multiple models have led me to the literature on pluralism in philosophy of science and political science. From that literature, I distilled two additional views on the interplay between different models, an agonist (Mouffe, Rescher) and an antagonist (Kuhn, Lawson) view. In contrast to consensualism, both views hold that multiplicity will not eventually disappear: multiple standards for scientific assessment remain possible at all times. As a consequence, the dynamics of such a scientific community has a complexity not captured in traditional (consensualist) models of the distribution of labour in science.

On the consensual view a scientific community will tend toward consensus, which is a single, optimal equilibrium. In order to find out what the dynamics of a community under multiple standards for scientific assessments would look like, I teamed up with Matthias Greiff, a German economist specialising in network economics. We developed a model describing the dynamics of standards competing for adoption, in analogy to the models used to describe the dynamics of technological standards competing for adoption which were used during the Microsoft antitrust trial. In our model, the consensual model is retained as a special case. Our main finding was that the insights derived from consensual models (single standard models) are not robust against an increase in the number of standards. Most importantly, such systems boast multiple equilibria which are not necessarily optimal.

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PHD THESIS SUMMARY:
The psychological foundations of Alfred Marshall's economics: an interpretation of the relationship between his early research in psychology and his economics.

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PhD in economics, March 2010
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Alfred Marshall (1842-1924) was one of the earliest professional economists at the University of Cambridge in the nineteenth century, and a founder of the Cambridge School of economics which nurtured leading economists such as A. C. Pigou and J. M. Keynes. However, in his early research, before becoming a professional economist, Marshall explored what psychology had to say about the human faculties (1867-1868). The purpose of my PhD dissertation is to argue that Marshall's economics was greatly influenced by this early research in psychology.

In brief, Marshall's economics is known for the partial equilibrium theory developed in *Principles of economics* (1920 [1890]). To Marshall himself, the fragmentary statical hypotheses which are included in partial equilibrium theory were temporary auxiliaries to provide preparation and practice for understanding dynamic economic phenomena. The main objective of Marshall's economics is demonstrating the organic growth theory of economy, which accounts for the mutual progress of human nature and economic society.

There has been little attention given to the significant influence of Marshall's early psychological research on his economics. In this dissertation, I argue that Marshall's analysis of the mutual progress of human nature and economic society emerged from his understanding of the complementarity between the findings of psychological research and his economic concerns. To the best of my knowledge, this is the first study that proposes this argument.

In chapters 2 and 3, I demonstrate the comprehensive relationship between Marshall's psychological research and his economics. In chapter 2, I focus on the existence of the relationship by analyzing human character in Marshall's early study of psychology and human nature in his economics. In chapter 3, I broaden the discussion by taking up Marshall's analysis of the concept of sympathy in his economic approach. There are two main points related to the concept. First, Marshall was indirectly influenced by Adam Smith in terms of the concept of sympathy. Marshall's first psychology paper, entitled "The law of parcimony" (1867), evaluated Herbert Spencer's concept of sympathy highly. Spencer himself had drawn on Smith's *The theory of moral sentiments* (1790) to construct the core notion of social evolution involved with individuals' sympathy in *Social statics* (1868). Second, Marshall's idea of economic progress is based on sympathy, which plays an important role in the improvement of human nature in both the working class and business man.

Chapter 4 is devoted to Marshall's core idea about the mutual progress of human nature and economic society, which has its origin in what he experienced during his American trip in 1875. Marshall was inspired to analyze the organic growth of the economy by the relationship he saw between industrial development and human ethical growth in America. This argument can be clearly supported by Marshall's two lectures entitled "Some features of American industry" (1875) and "The economic condition of America" (1878). It is important to understand the lectures by considering three main elements: the analysis of human character in Marshall's early psychological research; de Tocqueville's discussion of the relationship between citizens and commune; and Hegel's conception of subjective and objective freedom.

Chapter 5 investigates the significance of the continuity in Marshall's study of human nature from his early psychological research to his economics. Looking back on his whole career, Marshall reminisced that psychology had been his ideal field of study his whole life. Furthermore, Marshall often emphasized that 'The Mecca of the economist' lay in economic biology. That economic biology came from the organic growth theory discussed in *Principles of economics* (1920 [1890]), which was itself greatly influenced by Marshall's psychological research. For Marshall, to study psychology was to take up the challenge of seeking to understand the significant potential for the further development of human faculties (Keynes 1972 [1933], 171), and therefore his psychological research should be taken into account in properly interpreting his organic growth theory.

In conclusion the contribution of my PhD dissertation is to ascertain the significance and the role of Marshall's early psychological research for his later economic analysis, and particularly for his analysis of the mutual progress of human nature and economic society. Through this new approach to Marshall's organic growth theory of economics, I hope that this dissertation will contribute to the better understanding of modern economics.

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PHD THESIS SUMMARY: A theistic analysis of the Austrian theories of capital and interest.

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PhD in economics, March 2010
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This thesis examines the philosophical background that culminates in the Austrian School of economics' theories of capital and interest.

In chapter two, I describe the character of the school, its history, and the educational, environmental and social background of the main authorities. The school is characterised by an adherence to Carl Menger's doctrines, with Eugen von Böhm-Bawerk, Friedrich von Hayek, and Ludwig von Mises as disciples who elaborated and developed subjectivism.

In chapter three, I introduce and explain the philosophical position used in this study, which is the ontology and epistemology developed by Cornelius Van Til. In chapter four, I argue that the main Austrian authorities hold different epistemologies and ontologies, which conflict even with their apparently shared commitment to methodological individualism and subjectivism. I use Cornelius Van Til's philosophy to elucidate the commitments of the Austrians and claim that differences in epistemology emerge from distinctive ontologies.

In chapter five, Carl Menger's work on value, goods, and price is assessed. He developed a subjectivist theory of capital, in which time demarcates the value of present and future goods, with value determined by the want-satisfying individual. In chapter six, I examine the development of capital theory by Böhm-Bawerk, Hayek, and Mises: productivity and value differentials over time are the elements that Austrian capital theory attempts to explain. In chapter seven, I examine how Böhm-Bawerk, Hayek, and Mises produced distinctive theories of interest. Mises included elements of Menger's, Böhm-Bawerk's, and Frank Fetter's work within the framework of his epistemology of praxeology and affirmed a pure time-preference theory of interest. I maintain that Mises's capital and interest theories are the distinctive representative theories of the Austrian School.

As a (Dutch) Christian philosopher, Van Til wrote in the theological tradition of Augustine as well as Reformation theology. He developed a theistic world-view (i.e., ontology, epistemology, and ethics) that was grounded in Scripture, in which the character of (a personal) God is one who possesses exhaustive (i.e., internally consistent) knowledge, and who is self-existent, self-sufficient, and eternal. Moreover, the created order (including humanity) is temporal (or historical); therefore, human knowledge is derivative (though not exhaustive) and true as far as it concurs with God's revealed knowledge and plan.

The starting point for investigation, the object of knowledge, is anything referred to as a physical, mental, abstract, or spiritual fact. The question of 'objective' depends on one's perspective and is ontological. For any non-theistic position, a fact refers to the existence of any fact apart from God; therefore, facts exist by themselves and are assumed to have come into being by chance. Thus human experience of facts is immediate. The laws of logic are also operative by chance in the universe and dictate what is acceptable as possible or probable. The non-theist therefore reasons univocally, assuming that any fact may exist; the theist reasons analogically, and assumes that no fact can exist unless God's existence is taken as the ultimate presupposed fact.

The non-theistic position of the Austrian School, in which I include Menger, Böhm-Bawerk, Mises, and Hayek, affirms ontological monism. These authorities presuppose the (self-) existence of the spatio-temporal realm and the (self-) existence of universals, such as the universal law of cause and effect, as well as the logical structure of the human mind. These ontological propositions are their theoretical preconditions for epistemological claims to knowledge.

Theism's argument is that true claims to knowledge can only be developed from a world-view which presupposes that eternal universals exist in the being of God—the ontological Trinity. An individual cannot relate the concrete particulars of human experience to one another and therefore produce eternal universals. However, the Austrian solution presupposes the effective self-existence of the physical universe, as well as universals, such as causality and logic, and provides a way for the individual to make human experience intelligible.

A central question that I address is whether the Austrian School possesses a sound ontological and epistemological foundation and therefore whether its theories of capital and interest are incontrovertible. I have argued that the preconditions for a world-view that the Austrians have chosen are irrational: they are simply assumed. This is tantamount to stating that all temporal reality is a product of chance and ultimately mysterious. A subsidiary question is whether Mises's argument for apodictic certainty is unquestionable; I argue that without a valid ontology, this epistemological proposition cannot be justified.

The Austrians' claims to knowledge in their theories of capital and interest are propositions derived from the presupposed universals of individualism and subjectivism. In Mises's case, his epistemology requires only one a priori—that humans act—in order to develop economic theories. However, I argue that the Austrians hold to an ontology that precludes the justification of universals. This prevents them from making claims to knowledge, much less claims for propositions concerning capital and interest.

My argument is that the Austrians cannot justify their position and therefore cannot justify their theory of reality, which serves as the foundation for their economic method. The important result is that without a sound theory of reality, they cannot possess a sound theory of knowledge; therefore their claims to knowledge cannot be justified.

The Austrian School of economics has made a significant contribution to economic science, but its theories of reality and knowledge, as well as its method and theories of capital and interest, and the application of these to contemporary policy, would find greater legitimacy if reconstructed in the context of a world-view in which the authority for theories of reality and knowledge are grounded not in the authority of the autonomous individual, but in the authority of the God of Christianity.

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