

Naturalism and the elimination of dissent in economics

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For presentation to the biennial conference of the Economic Society of South Africa, Birchwood Hotel,
Johannesburg, 3-5 September 2019
Preliminary draft – v2 – not to be cited

Abstract

In the 1970s, many critics of neoclassical economics believed that the philosophy of science provided an authoritative platform for a wide-ranging critique of orthodoxy. However, these dissenters were to be sorely disappointed. Not only did the critique prove ineffective but in the 1980s the practice of both economics and economic methodology began a sea change that was to prove fundamentally supportive of economic orthodoxy. While there are many factors underlying this change, I argue that the philosophical ideas of 'naturalism' played a role in at least two ways by not only supporting orthodoxy but also undermining the (1970s-type philosophy of science) grounds for its critique. One way was the promotion of the idea of economics as an objective science on a par with the natural sciences; the other was the derogation of (abstract) theorising not closely linked to empirical measurement.

Key words: empiricism, positivism, interpretivism, Quinean and post-Quinean naturalism

JEL codes: B20, B40, B41

Introduction

Following the inflationary 1970s, support among academic economists for the view of Keynesians -- that the market economy was not self-regulating (pointing to the need for government intervention) -- steadily began to decline. Even the world financial crisis of 2008 failed to shake this consensus view about the desirability of the market economy.

Apart from inflation, one of the other factors for promoting this consensus amongst economists was political. Mirowski (2009, pp. 107-112) has argued that the members of the Mont Pelerin society did not believe that free market institutions would develop 'naturally' in the post-second world war era. He points out that, according to Hayek, this was because false ideas about the market had been spread by intellectuals who were dealers in second hand ideas. They therefore determined it necessary to promote actively the efficacy of market organisation. For Mirowski, this constituted the origins of neoliberalism.

I argue in this paper that another factor promoting this consensus was the demise of, what Hausman (2009, p. 53) has termed, '1970s philosophy of science' (e.g. particularly Popper and Lakatos). This had been largely critical of the aspirations of economics to be an empirical science. It therefore provided a

platform for the critique of the orthodox consensus (both from within (Blaug 1980) and without (Cole, Cameron and Edwards 1983)). However, in the philosophy of science in the 1980s a sea change took place. This completely undermined any basis whatsoever for this platform. Its collapse removed (what previously had been taken to be) an authoritative basis from which to launch fundamental criticisms of mainstream economics. Instead, there was a wholesale loss of confidence by philosophers in the authority of philosophy of science to pronounce on what constituted good or bad/pseudo (economic) science. (While critics of the orthodox consensus could turn to the burgeoning post-Kuhnian sociology of science and rhetoric literature, such criticism now came at the cost of the radical dismissal of the possibility any objective science.)

Within the disarray that prevailed in post-1970s philosophy of science one doctrine seemed to many, if not most, analytic philosophers to offer a way forward: 'naturalism in its 20th century American incarnation' (to use Roth's (2013, p. 646) phrase), has 'become widely popular only in the last few decades' (Papineau 2015, p. 4). Previously Quine (1951) had presented one of the strongest criticisms of the then prevailing 'positivist' philosophy of science, or what has been termed the Received View. However, his criticism (Quine 1969) was so radical that it applied not only to the Received View but also to most of the 1970s philosophy of science (despite Popper and Lakatos regarding themselves as rejecting positivism). Although Kuhn and Feyerabend were left standing, Quine's criticisms wrecked not only the Received View but also the 1970s platform. As Hands (2001) emphasized, no longer did any simple rules determine good (economic) science.

The sea change in the philosophy of science in the 1980s had, by the 1990s, found its way into economic methodology. Consequently, there developed, what Hands (2001, p. 394) has termed, a 'New Economic Methodology' which drew not only on philosophy of science but also on 'science theory' or social studies of science e.g. Kuhnian-inspired sociology of scientific knowledge (SSK) and the rhetoric of science. He explains that the purpose of coining this new term is to signal the emergence of a far broader approach to economic methodology appealing to a 'much wider range of scholars' than before. He argued that if effective 'cross-fertilization' is to take place, contributions to this new field could no longer remain narrowly bound within their own discipline. No longer could philosophers of science such as Philip Kitcher who 'employ[ed] economics in their philosophical work' completely disregard 'two hundred years of methodological debate' about the status of economics as a science. Instead, there now needed to be mutual recognition of related efforts in the new field. Hands hoped the label 'New Economic Methodology' would help foster the necessary 'cross-fertilization' for the new field to develop (p. 395).

Twenty years later, it would be interesting to see the results of an empirical evaluation of just how far the New Economic Methodology has developed as the multidisciplinary enterprise Hands had envisaged. Contrary to Hands's envisaged 'horticultural diversity in the post-positivism garden' (to use Dupré's 2003 phrase) it seems to me that naturalism in its various forms occupies a very large central part of the garden. Only in the outlying areas do we find beds of realism, science studies such as SSK and rhetoric. Whatever is the case in New Economic Methodology, it is clear that in economics there is only one approach that qualifies as scientific (legitimate) work: that of (mathematical) modelling, measurement and testing. It may be coincidental that this approach conforms to what (at least some versions of) naturalism prescribe as the only way of doing scientific work. However, I argue that (philosophical) ideas can and do influence the practice of economics. So, while horticultural diversity may be blooming, these outer beds seem to have virtually no influence on the practice of economics.

However, what seems to have happened is that in the aftermath of Quine the field of a 'naturalist' approach to the philosophy of science is positively blooming. According to Papineau (2015, p. 1) 'naturalism' is widely viewed as a positive term in contemporary analytical philosophy.

I will look more closely at the meaning of 'naturalism' in later sections. This is important since the term has no very precise meaning: there are many versions of naturalism. However, a range (from weak to strong) of 'philosophical commitments of a generally naturalist stamp' can be discerned (Papineau 2015, p. 1). As regards the weak commitments, it has been part and parcel of modern economics from at least Adam Smith. Ontologically, it has historically involved a commitment to the notion that there are no supernatural entities beyond our 'natural' world so that reality is exhausted by 'nature'. Quite what is meant by 'nature' is more difficult to articulate and so leads to some strong ontological commitments. One well-known stance is to conflate nature with materialism or physicalism or the subject matter of the physical sciences: 'there is nothing more' to the mental and social realms 'than arrangements of physical entities' (Papineau 2015, p. 3). (This would seem to say goodbye to economics as currently practiced.)

Epistemologically, a strong commitment is the (Quinean) notion that there is no knowledge to be gained from (traditional or non-naturalist oriented) philosophy prior to science. Philosophy is 'continuous with science' (Quine 1969, p. 126). This implies that the methods of the natural sciences are the only means of gaining knowledge. The pretensions of 'philosopher kings' (and 1970s philosophers of science) to have knowledge that is somehow gained independently of science is laughed out of court. All this leads to the conclusion that social scientists can and should apply the methods of the natural sciences to the human and social world.

In this paper, I take as given that the influence of naturalism in both philosophy and the social sciences has increased markedly in the last 20 to 30 years. I argue that it is this influence that helps explain the elimination of dissent in economics. By the elimination of dissent in economics I mean not only the marginalization – and virtual elimination – of schools of economic thought alternative to 'mainstream' economics such as the 'old' institutional school of Veblen, Mitchell and Commons (that prevailed in interwar America), the Austrian and Marxist schools as well as various forms of Post Keynesian economics critical of mainstream macroeconomics. I refer also to the derogation of the study of 'orthodox' history of economic thought. As a consequence, ideas from the past that may conflict with current-day economics are left to gather dust on library shelves. (I will not be concerned with the neoclassical school since it is too closely tied up with today's mainstream economics. However, it is interesting to note that Mirowski (2009, pp. 112-115) distinguishes three 'sub-schools' of postwar American neoclassical microeconomic theory: Chicago, Cowles and MIT.)

Braybrooke (1998, p. 838) has outlined three schools within the philosophy of social science: the naturalist, interpretive and critical. Rosenberg (2016, p. 11) distinguishes between two epistemologies: naturalism and interpretation while Hollis (2002, pp. 1-22) distinguishes between Enlightenment naturalism and 'understanding'. I take both these distinctions as corresponding to Braybrooke's first two schools. However, I follow Braybrooke in arguing that 'the different contentions of philosophers on the subject of social science demand something like the three-schools distinction' (p. 839). I argue that the increasing pervasiveness in economics of the naturalist school since around 1980 helps explain the change in the practice of economics from around 1980 to today and, in particular, the marginalization of schools of economics alternative to the mainstream.

The paper divides into sections. Section 1 documents the changing character of economic methodology and economics over the period from around 1980. Section 2 briefly explains the relevance of Braybrooke's three-schools distinction for elimination of dissent in social science. Since Braybrooke's critical school draws on both naturalism and interpretation, it then focuses on the history of this fundamental divide in the philosophy of social science. Section 3 begins with some introductory remarks on empiricism and positivism before turning to Quinean naturalism. Section 4 deals with post-Quinean naturalism where different philosophical commitments are distinguished and problems discussed.

Given that naturalism may not constitute the ultimate truth, or be the theory of everything that some of its extreme advocates seem to think it is, underscores the importance of actively promoting Braybrooke's three-schools distinction in the practice of economics today if future dissent from Braybrooke's non-naturalistic schools is not to be eliminated from economics.

1 The changing character of economic methodology and economics

Economic methodology

Hutchison (1996, 1997) has described how the views of philosophers (and philosophically-minded economists) have long influenced methodological thought about economics. On the one hand, he explains, there has been the influence of rationalism or, more accurately, apriorism (with its emphasis on deductive reasoning) on the methodological writings of Ricardo, Senior, Cairnes (revived long after by Robbins (1932, 1935)). On the other hand, the classical empiricism of Hume, Locke and Berkeley (with emphasis on inductive reasoning) influenced Smith, Jevons, Marshall and more recently John Maynard Keynes (Hutchison 1998).

In the 1930s logical positivism began to influence economic methodology. Hutchison's (1938) 'positivism' reacted to Robbins's a priorist stance that the fundamental postulates of economic science were 'self-evident' propositions the truth of which were not subject to empirical verification. Any testable implications indicated only their domain of application. It remains unclear whether Robbins followed an empiricist-inspired deductivism (the Millian tradition) [or the] antiempiricist deductivism of certain Austrians [e.g. von Mises] (Hands 2001, p. 39). In the 1970s the falsificationist views of Popper influenced economic methodology (Blaug 1980). Thereafter Lakatos's notion of research programs with a hard core and protective belt was used to defend neoclassical economics.

As mentioned in the Introduction, the sea change in the philosophy of science following Quine (1969) led by the 1990s to a sea change in the methodology of economics. Now, attempts to apply the philosophy of Popper or Lakatos to economics met the same fate as had earlier befallen the Received View. The new 'naturalistic' perspective is nicely reflected by Davis and Hands's (2011, p. 1) remark that until the late 1990s, economic methodology could be understood 'as the successive application of series of different philosophical views regarding the nature of good scientific thinking'.

While there undoubtedly has been a major change in the nature of economic methodology, I argue that this has not always been because 'economic methodologists began to set aside the normative

methodological agendas associated with philosophical imports' (Davis and Hands (2011, p. 1). As mentioned earlier in the Introduction, I argue that, Hands's hoped-for interdisciplinary New Economic Methodology largely failed to materialize. Instead, what led to 'economic methodologists' turning to study 'the methodological reasoning they found implicit in economists' thinking' (p. 1) was simply the continued influence of a new philosophical import, namely, 'naturalism'. Moreover it was no longer 'economic methodologists' doing this studying but ('naturalistically'-inclined) philosophers of science. Rosenberg (2009, p. 57) himself refers to the state of economic methodology 'before we philosophers got hold of it' which he dates as Rosenberg (1976). Hands (2001) recorded the change in the character of 'economic methodology' that had taken place by the turn of the century. The volumes (Boumans and Davis 2010; Davis and Hands 2011) were among the last to have 'economic methodology' included in the title. Thereafter the process of 'philosophical imports' eventually led to the 'methodology of economics' being transformed into the 'philosophy of economics' where naturalistically-inclined philosophers sought to apply their naturalistic approach to economic science (Reiss 2013, Ross 2014). The former subject of 'economic methodology' had been 'naturalised'.

I must once again point out that naturalism has many versions. Some of the naturalization of economic methodology has not been the result of 'philosophical imports'. For instance, some naturalistically inclined philosophers - Mäki is the outstanding example – look first to economics and thereafter try to make philosophical sense of what economists were doing. This is possibly because he studied both economics and philosophy at undergraduate level (Mäki 2009, p. 69).

However, some philosophers seemed to have had no qualms about applying their own normative methodological agenda to economics. For example, Kincaid (1996) sets out the 'philosophical foundations' necessary for the social sciences. He further explained the nature of the required philosophical import, namely 'naturalism'. According to Kincaid this encompasses the view that 'the social sciences can *only* be good science by meeting the standards of the natural sciences' (Kincaid 1996, p. 3, italics added).* 'Standards' seems to refer to the methods and 'virtues' (e.g. predictive success, objectivity) of the natural sciences (Kincaid 1996, pp. 3-15 and 50-51). Root (1998, pp. 672-3) points out that philosophers, while content to describe natural science, continue 'to prescribe as well as describe [social] scientific practice. Kincaid's (1996) book is prescriptively rich; where the methods of confirmation or explanation used in the social sciences are not those of the natural sciences, Kincaid argues that they ought to be'. Regarding this type of prescriptive naturalism, a substantial aspect of the change in post-1990s 'economic methodology' has been due, *pace* Davis and Hands (2011), to the continued influence of 'the normative methodological agendas associated with philosophical imports' (rather than science theory).

Economics

In the 1960s and 70s, according to Backhouse and Cherrier (2016) highly abstract 'pure theory' held the highest status in academic economics (e.g. general equilibrium theory, core micro and core macro). However, by the 1990s, they point out it was 'applied' empirical work that earned the most esteem. For example, winners of the AEA's Clark Medal conducted empirical rather than theoretical work. Already in

1993 the 'remarkable resurgence of empirical economics over the past decade' drew praise in the citations for the medal.

Since the 1990s economics at the cutting edge (more than in the textbooks) has seen the development of a huge number of new and rapidly expanding fields e.g. applied microeconomics, experimental economics, behavioural economics, evolutionary economics, neuroeconomics and agent-based computational economics. These new sub-disciplines have been characterized by much interdisciplinarity as well as by applied empirical, rather than abstract theoretical, work. An example of this is Kahneman's work in devising ingenious experiments in psychology that led to the 2006 Nobel Prize in economics. Kincaid and Ross (2009, pp. 10-27) list four new research areas: (1) 'number crunching' (reducing reliance on high abstractions and permitting greater weight to be given to empirical testing of hypotheses), (2) game theory (and asymmetric information models – for which Akerloff, Spence and Stiglitz shared the Nobel Prize, Binmore 1998, Camerer 2003, Bowles et al. 2005), (3) 'interdisciplinarity' (Bruni and Sugden 2007, Camerer and Loewenstein 2004, Kahneman 2006 Nobel Prize, Maynard Smith 1982), and (4) experimentation (Vernon Smith's Nobel Prize 2006, Guala, 2005).

One reason for this change in economics was historical and technical. The post-war development of economics preeminently took place in the United States, a society long influenced by a tradition of empiricism (and pragmatism) inherited from the great British empiricists. Within this atmosphere, the vast increase in the availability of, and computing power to manipulate, empirical data sets not surprisingly led to a field day for empirically-inclined economists who had, despite Friedman (1953), continued to feel uncomfortable with the radically unrealistic assumptions of neoclassical economics.

Another reason, Mirowski (2009) has argued, was historical and political. Whereas the economic problem in classical economics had been production and in neoclassical economics had been the static allocation (exchange), from around 1950 it became knowledge, in particular with showing how market organisation dealt with obtaining knowledge (and information) more efficiently than that of central planning. He traces this post-1950 concern to the Socialist Calculation Controversy of the 1920s. Out of this developed the game theory of Oskar Morgenstern and the neoliberalism of Hayek's development of the idiom of 'market place of ideas'. It was these ideas of Hayek's, according to Mirowski, that led to the cognitive revolution. Further historical events that help explain the change in economics concern the post-1989 de-industrialization of 'the older developed economies' and 'the across-the-board neoliberal push to commercialize science and privatize education on a global scale' (2009, p. 104).

However, I argue that over and above these historical technical and political events, the influence of philosophical ideas was a further reason for the change in economics. Here I refer to Keynes's (1936, pp. 383-4) closing lines in which he argued that it is 'the ideas of economists and political philosophers' rather than 'vested interests . . . which are dangerous for good or evil'.

Hands (2003, pp. 551-52) disagrees with the view that philosophical and methodological ideas have influenced the actual practice of economics. One of the main reasons he advances is Blaug and Hutchison's well-known Popperian-inspired complaints 'about the lack of empirical testing and testability' in economics. While these criticisms may have been true of the economics they referred to, as I have detailed above it is no longer true of economics as practiced since the 1990s. This, I argue, is because positivism *did* influence economics, albeit with a lag. For example, Boland (1991) argued that the influence of positivism on economics was (in 1991) virtually all pervasive. Kincaid (1998b, p. 559) has argued that positivist ideas 'strongly influenced' the practice of economics. Kincaid and Ross (2009, p.

27) view Samuelson's *Foundations* as 'the high church expression of mainstream postwar philosophy of science among economists'. I argue that it is precisely because naturalism retains a number of key positivist conclusions (see pp, 15-16) that the post-1990s practice of economics has increasingly emphasized the importance of empirical testing and testability. Blaug and Hutchison's comments no longer apply to the economics of today (although I'm not sure they would approve of the naturalist direction it has taken).

Therefore, I argue that over and above these historical, technical and political events, a further reason for the modern diaspora of social and economic research has been the influence of naturalist ideas. In the same way as positivist ideas have been held to have influenced pre-1990s economics, so I argue naturalist ideas have helped oil the wheels of the change in post-1990s economics. It is not to say that the influence has been direct or even 'causal'. However, it is surely no coincidence that most of the new fields mentioned above (experimental, behavioural etc.) cohere with the worldview put forward by naturalism. For, as Hands pointed out, there are many versions of naturalism and a worldview is one of them (see Kincaid 1996, p. 4).

Mäki (2009, pp. 88-89) argues that a worldview, or what he terms a vision of 'the way the world works (*www*)', operates as an ontological constraint on economic theories and functions mainly negatively. Proposed theories (presumably such theories include the schools of economic thought alternative to the mainstream) that do not meet the *www* constraint applicable to the mainstream, will be considered unfavorably or not at all.

This is how the ontological constraint starts playing an epistemological role, ruling out ideas not worthy of belief, acceptance or further exploration. Much of disagreement and criticism between economists of different persuasions boil down to differences in their respective *www* convictions. Therefore, those disagreements cannot be understood or resolved by way of empirical testing only; the call is for ontological investigation and argument (Mäki 2001, 2008). (Mäki 2009, p. 89)

In the next section, I argue that the recent changes in both economic methodology and economics (sketched above) are best understood when related to the fundamental divide in the philosophy of social science between 'naturalism' and 'interpretation'. While much mention has been made of 'naturalism', it is its relation to 'interpretation' that leads to a deeper appreciation of the issues involved.

2 The fundamental divide in the philosophy of social science

Perhaps the most basic question about the nature of economics, or more generally of the social sciences, has been put by Manicas (1998, p. 847): 'is [human] society "natural" or is it "conventional", a historical product of human activities which vary across time and space'? Putting it another way, the question is whether social scientists (e.g. economists) can and should employ the methods of natural science.

Manicas argues that the seeming insuperability of the ideas of the Copernican and Newtonian revolutions gave rise to the notion that society itself is 'natural'. If society is natural then it is part and parcel of the natural world, and hence subject to the methods of the natural science. Indeed, long ago

Quesnay and later Comte took the new paradigm in natural science as the model for developing a social science. On the other hand, the idea that society is 'conventional', implies that 'humans have collectively [politically] made [constructed] society and can thus remake it'. That is, (individual) human beings are viewed as standing apart from nature, capable of thinking and acting independently of the natural world and thereby able to intervene to affect both nature and society. This implies that it is (independent) ideas embodied in art, music and literature that have led to social action (e.g. revolutionary change in societies).

Braybrooke's three-schools distinction

In order to have a framework for judging how to answer the question of a fundamental divide, it is helpful to draw on Braybrooke's (1998) distinction between three schools within the philosophy of social science. In addition to naturalistic and interpretive schools (which will be discussed below), Braybrooke distinguishes a 'critical school'. While it finds 'the other two schools shot through with bias', it tends to agree with the interpretive school 'in resisting naturalistic methods', although it sometimes uses them. It tends, in general, to 'repudiate naturalistic inquiries' (p. 845). Its concerns are, for example, that naturalism may represent as harmonious societies that are in fact in difficulties or distress. Or, it might interpret such distress in a manner that is useful to the interest of those who benefit from it (p. 838). Consequently, while it draws on both schools, the critical school is distinguished by its (a) questions about ideological bias; (b) 'the thesis that there is no attempt at social science without commitments to political values'; (c) commitment to the project of social emancipation; and (d) denial that 'any study of social phenomena can be value-free, in particular on the issue of emancipating people from the oppressions of current society' (pp. 843, 839).

The three-schools distinction helps explain Mäki's (2012, p. xiii) proposition that economics is and always has been 'a chronically contested discipline'. That is, it explains the philosophical roots of this contestation. Furthermore, the point of his three-schools distinction, Braybrooke (1998, p. 845) explains, is to provide the basis for a sceptical perspective (amongst all this contestation) about the claims of any single school to represent the only correct approach to the philosophy of social science.

Given that the critical school, according to Braybrooke, has no approach to the philosophy of social science distinct from those of the naturalistic and interpretive schools, the difference between these two schools becomes the fundamental divide in the philosophy of social science.

The fundamental divide in the philosophy of social science

Rosenberg (2016) labels this fundamental divide in the philosophy of social science the issue of 'naturalism versus interpretation'. On the side of 'naturalism' he includes empiricism and positivism. Since these will be discussed in a later section, the focus for now is on 'interpretation'.

The interpretive tradition has been traced back as far as Giambattista Vico who, according to Braybrooke (1998, p. 841), argued that human beings inherently understand social phenomena since these are 'of their own making'. (In Manicas's terms society is artificial, constructed, and therefore revisable by human beings.) The tradition includes Max Weber (and his concept of *Verstehen*) and the work of Husserl and Heidegger who stressed the subjective features of human experience. It was championed by Peter Winch in his *Idea of a Social Science*. Hollis (2002, pp. 16-20) traces the tradition back to notions of the German idealist thinker Wilhelm Dilthey for whom only mind or mental states exist. According to Rosenberg (2017, p. 33), the interpretive tradition 'closely embraced' Kuhn's *Structure of Scientific Revolutions* especially his argument that science is a social activity. (Since naturalists view society as 'natural', some naturalists interpret Kuhn as having adopted naturalism rather than 'interpretation'.) Rorty (1989), he says, argued the case 'for a radical version of interpretativism'.

As part of its long history, 'interpretation' has drawn somewhat more on rationalism than has 'naturalism'. It is to reason (rather than sense experience, divine revelation or institutional authority) that we look in our attempt to gain knowledge. For example, our intuition of self-evident propositions provides us with knowledge. This seems to have been the basis for Robbins's (1932, 1935) ideas on the subject. Certainly, Knight (1935) emphasizes the intuitive basis for our knowledge, while Knight (1940) defends an interpretive ('non-positivist') approach to economics. 'Interpretation' also draws partly on idealism (only minds exist). It is idealism which is the opposite of naturalism (only nature exists), and not supernaturalism (only the divine exists) (Quinton 1999, p. 565).

As a short cut to understanding the interpretive tradition, I feel the best way is to follow Rosenberg's (2016) explanation of the fundamental divide between naturalism and interpretivism discussed under the next sub-heading. Readers should be careful not to interpret this as a reflection of Rosenberg's 'take' on economics as a science. Concerning this, Rosenberg (2009, pp. 55-67) argues that economics, along with all the social sciences are biological sciences 'devoted to the study of the causes and effects of the behaviour of members of a particular species, *Homo sapiens*' (p. 60).

The point of departure for interpretive inquiry is that in social science as opposed to natural science we deal with conscious beings (not external objects), with interpreting human behaviour as purposeful action, the result of desires and beliefs. As such, we gain knowledge when we make human action intelligible, that is, when we understand the meaning of people's actions either individually as intentional or as following social *rules*.

Rosenberg (2016) points out that social scientists have long sought a 'third way', some happy medium between these two polar opposites. The outstanding example is Max Weber who argued that social scientists can and should uncover both causal regularities (as in naturalism) as well as interpretive meanings (*Verstehen*). In this vein, economists have long interpreted expectations (beliefs) and preferences (desires) as causing human actions. However, he argues, a 'third way' may well result in 'an incoherent position' (p. 29). For him, naturalism and interpretation are simply two irreconcilable approaches (pp. 28, 69-70).

Rosenberg's explanation of why the fundamental divide between naturalism and interpretation is irreconcilable

According to Rosenberg (2016, p. 12), the methods of the natural sciences are successful because they uncover laws (models, empirical regularities or generalizations). Only laws can provide knowledge of the future so crucially enabling prediction and control (p. 12). Laws are also important because science seeks causal explanation. Causation requires a law or empirical regularity i.e. a causal rather than an accidental sequence of events.

Such laws are nothing more than statements of constant conjunction of distinct events' (p. 13). That is, they are contingent regularities obtaining only for greater or lesser periods of time. They are not the result of the operation of hidden causal powers. If causation is nothing more than constant conjunction then 'our knowledge of individual causal sequences is justified only if we can successfully predict further effects when we observe their causes' so that prediction is the *sine qua non* of causal knowledge (p. 14).

However, over the course of the past one hundred years' (Rosenberg 2016, p. 31) it has been argued that 'the aim of the social sciences must be interpretation', that they should or can explain human behaviour in a non-causal or interpretative way. Interpretive inquiry deals with interpreting human behaviour as purposeful action. As such, we gain knowledge when we make human action intelligible, that is, when we understand the meaning of people's actions as intentional. This is the 'folk psychology' approach familiar to all economists. Rosenberg claims that the following general statement seems to lie behind such explanations:

[L] If any person wants some outcome, d, and believes that action, a, is a means to attain d, then x does a (p. 39).

For interpretationalists, the belief/desire model works because beliefs and desires are reasons for actions, that is, they justify (interpret) them and show them to be rationally intelligible in 'the light of the agent's beliefs and desires' (p. 43). What connects beliefs and desires to actions (and social institutions) (p. 121) is that [L] is interpreted as a rule, social norm or practice and not a causal law. Unlike a causal law, rules can be broken and breaking them leads to punishment (p. 125).

Institutions and social roles are 'constructed' out of rules. Since they are constructed they can be changed, revised, reformed or overthrown (p. 134). It would be a mistake to take them as facts fixed and independent of human decisions (p. 138). Social institutions are far from natural but are instead artificial and revisable (p. 138). Furthermore, we need to recognize that the scientific method, like other social institutions, is a human construction with no special claims to objectivity (p. 139). The (natural) scientific method is not the only, or even appropriate, way to acquire knowledge in social science. Interpretations provide knowledge because they provide intelligibility and intelligibility, not prediction, is the mark of knowledge' (p. 170).

For naturalists such as Rosenberg the belief/desire model can work only if beliefs and desires are viewed as causes of, rather than reasons for, behaviour: that is, if [L] is interpreted as a causal theory. However, Rosenberg points out that there are serious obstacles to treating [L] as a theory comparable to one found in the natural sciences. The issue revolves around just why and how desires and beliefs explain actions (p. 38). For one thing, a very strong *ceteris paribus* assumption is required. (For example, in

economics we require that desires, or preferences, remain constant.) For another, if explanation is to be causal it must be based on a law, not a rule.

However, Rosenberg argues that in economics [L] is mistakenly treated as a causal law. In a causal law the causal links must be contingent: 'when event a causes event b it must be conceivable for either to have occurred without the other having done so' (p. 58). They must be 'open to rejection as a result of empirical findings about contingent matters of fact' (p. 59). Yet in 'folk psychology' economics, desires, beliefs and actions are logically connected, not contingently connected by [L] (p. 57). It is not an empirical law but merely a principle that reflects our conception of ourselves as rational agents (p. 58).

The fundamental problem, says Rosenberg, is that intentional concepts are faulty concepts. They are not a 'natural kind' term (p. 188). Natural kind terms reflect 'real divisions in nature' (p. 22). To use Aristotle's phrase, they 'carve nature closely to the joints'. Intentional concepts seem to be in the same category as Freud's 'repression', Marx's 'alienation' or, in the physical sciences 'phlogiston'. Intentional theories are simply dead ends.

Rosenberg argues that the two views on how to do social science are so different as to be irreconcilable (2016, p. 28). Those who disagree, says Rosenberg, argue that 'our conception of the nature of scientific theories needs to be changed until it can accommodate [intentional explanations] as causally explanatory regularities' (p. 70). Certain naturalists reject Rosenberg's interpretation of naturalism and his dismissal of the possibility of a science of society. They argue that in their conception of the nature of scientific theories intentional behaviour can be accommodated so that, in principle, there is no barrier to a (naturalist) science of society (Kincaid 1996, pp. 191-221).

3 Quinean or first-generation naturalism

Pre-Quinean naturalism, from Comte through Mill to positivism (or the Received View) to 1970s philosophy of science, was naturalistic in the sense that it was believed the methods of the natural sciences -- searching for causal regularities etc. -- applied to the study of social phenomena might result in greater success in this field than had hitherto been the case. Quinean naturalism was quite different in that it made the radical assumption that social phenomena are part of the 'natural' world. Furthermore, they are no different in any respect to any other natural phenomena. It therefore adopted an entirely new approach to the philosophy of social science: human beings can (and prescriptively therefore should) be studied in exactly the same way as the rest of the 'natural' world.

Quine is generally regarded as the key figure of what has been described as 'naturalism in its 20th century American incarnation' (Roth 2013, p. 646). Regarding (Quinean) naturalism, a lot obviously hangs on what is meant by 'natural' or 'nature' or 'naturalism'. While Hume and the positivists regarded themselves as rejecting metaphysics, Quinean naturalists made explicit metaphysical commitments regarding 'nature'.

Ontologically, he adopted physicalism (Kincaid 1996, p. 197). This involves a thesis about the 'causal completeness' of the physical realm: every physical effect is fixed by a fully physical prior history. This implies that 'if mental and other special causes are to produce physical effects, they must themselves be

physically constituted' if they are to produce physical effects i.e. the physicalist doctrine that 'anything that has physical effects must itself be physical' (Papineau 2015, pp. 5-6). For them, everything (including all social phenomena) that exists is part of 'nature' where 'nature' is understood as referring only to the subject matter of the physical sciences. Such ontological commitment implies that minds and mental states (belief, desire) don't really exist.

These ontological commitments imply that Quine rejects the Humean empiricist view that individuals have access to the external world via their mental states i.e. privately perceived sense data. Talk about mentalistic notions such as 'experience' or 'observation' (notions involving meaning – and therefore interpretation) is rejected in favour of talk about natural entities such as a neural input (e.g. a stimulus) or a linguistic entity (e.g. an observation sentence) (notions involving reference). (Orenstein 1998, p. 5).

Epistemologically Quine adopts the radical view that dispenses with any role whatsoever for philosophy 'pronouncing on what constitutes knowledge' (Roth 2013, p. 648) and especially any normative or prescriptive role (telling practicing scientists what to do): instead he views philosophy as 'continuous with science' (Quine 1969, p. 126), an idea 'widely accepted' (Kincaid 1996, p. 21). Quine argues that epistemology has no autonomous role: it 'collapses' completely into (empirical) psychology that explains 'how people actually arrive at their beliefs' (Luper 1998, p. 725). To support this argument, he draws on his demolition of the analytic-synthetic distinction (all statements are empirical and hence revisable) as well as his holism. Quine argued that the positivist analytic-distinction was no dichotomy: analytic (linguistic) statements could not clearly be separated from synthetic (empirical) ones. Instead, all statements are entwined in a 'web of belief'. All contained an empirical element so that there are no a priori truths. 'Since conceptual matters are not entirely distinct from empirical ones, philosophy of science can no longer be a purely conceptual enterprise' (Kincaid 1996, p. 20).

To many heterodox economists and others critical of mainstream economics it seemed that the 1970s challenges of Kuhn, Lakatos and Feyerabend to the empiricist-oriented Received View provided both an authoritative platform for overcoming the hegemony of positivist ideas, dismissing economic orthodoxy, as well as a welcome means of promoting the agendas of their alternative schools of economic thought. However, any thought of methodological help in this respect was dealt a body blow with the advent of Quinean naturalism. What happened instead was that although Quine's intervention was fundamentally critical of positivism (and directly led to its demise in the 1950s) it was not critical of traditional empiricism in order to dismiss it (*à la* especially Kuhn and Feyerabend) but in order to improve it as a means of developing a more sophisticated form of empiricism. In this vein it is to be noted that Quine's critique of positivism has been viewed as that of 'an empiricist reforming empiricism' (Orenstein 1998, p. 6). Far from the demise of positivism undermining orthodox economics, Quine's intervention provided the philosophical basis for the growth of a new, more vigorous and increasingly sophisticated empiricist philosophy of science in the latter half of the twentieth century.

4 Post-Quinean or second-generation naturalism

Among the factors that have spurred the growth of post-Quinean naturalism in the second half of the twentieth century were several historical changes that favoured its development. These revolved

around a sharp rise in the postwar status of science. First there occurred the ‘cognitive revolution’ in the natural and social sciences in America in the 1950s and the development of cognitive science (i.e. artificial intelligence, cognitive neuroscience, cognitive psychology – Hands 2001, p. 143). Following Chomsky’s (1959) criticism of Skinnerian behaviourism, it allowed concepts banished from psychology under positivism such as belief, meaning, desire and intentionality to be reintroduced (Hands 2001, p. 143). Explanation could refer to unobservable entities. It led to new findings about preferences and expectations (‘the two causal variables in economic choice’) being introduced into economics (Rosenberg 2009, p. 66). The development of the digital computer and the notion of the mind as a type of information processing unit added further impetus to the cognitive revolution.

Second, developments in biology meant that it took over the status that physics had held for science in the early twentieth century (Hands 2001, p. 155). In line with Quine’s suggestion that there is ‘encouragement in Darwin’, some naturalists have turned not to physics but to biology. Indeed Kincaid (1996, p. xv) cites as a ‘direct cause’ of his naturalist ‘convictions’ the discoveries of Watson and Crick (1953). As Rosenberg (2009, p. 65) points out ‘developments in biology began rapidly to add predictive content and considerable testability to the theory of natural selection in the years after 1953’. These developments also meant that it was increasingly difficult for ‘interpretivists’ to argue that human beings should be studied differently to the rest of nature. Increasing evidence showed similarities between human and non-human animals.

The many versions of second-generation naturalism

As Braybrooke explains, the point of his three-schools distinction is to warn against the danger represented by any one school claiming to encompass the views of the other two schools since he views these as making claims about society that the other school does not make. Should this happen it would threaten the independent existence of the other schools. Given the influence of philosophic thought on the practice of economics, and given that this thought has mostly influenced the economic mainstream, the threat of naturalistic ideas may lead to increasing marginalization of non-naturalistic schools of economics alternative to the mainstream becomes increasingly likely. In time, schools of thought -- and hence dissent in economics not sanctioned by the mainstream -- would be eliminated.

Versions of naturalism less threatening to non-mainstream economics

However, the threat of naturalism becoming dominant is not as dramatic as may seem from the above. This is because there is a wide range of versions of naturalism, only some of which threaten dissent in economics. Those that do not seem to threaten (at least directly) schools of economic thought alternative to mainstream economics are those with weaker philosophical commitments to naturalism or realists with commitments different to many naturalists. Here the outstanding example is Mäki (2009). While many naturalists subscribe to some form of physicalism, for Mäki the ontology of the

social world does not consist of ‘quarks and photons, magnetic forces and black holes’. Instead it consists of ‘the commonsense world of stones and trees, chair and tables’ (2009, p. 87).

Economics views the world more ordinarily, largely comprising familiar entities of folk psychology and commonsense social observation. The world of economics is the ordinary world of firms and households, preferences and expectations, money and prices, wages and interests, contracts and conventions, inflation and unemployment, imports and exports. Economic models refer to a world furnished with what I have termed *commonsensibles*, thus they do not postulate unobservables in the same sense that much of physics does. (Mäki 2009, pp. 87-88).

Other naturalists with weaker commitments or among the non-self-declared naturalists are Cartwright (1983), Dupré (1993), Kitcher (1992) and Hausman (1992). For example, Kitcher (1992) is concerned with the project of trying to preserve the possibility of a normative epistemology: philosophy is not completely continuous with science. He acknowledges that there may be constraints that prevent ‘cognitive progress’. Among the many problems is that of the theory-ladenness of observation. Similarly, Mäki (2012, p. xvi) argues that using economics to ‘naturalize’ the philosophy of science [economic science] is ‘an open issue’ requiring a role for ‘philosophical reflection’.

Versions of naturalism more threatening to non-mainstream economics

Among these are those with ‘strong’ (ontological) i.e. physicalist commitments: the positions of Dennett (1969), Rosenberg (1976) and Papineau (1993) seem more threatening to non-naturalist schools of the philosophy of social science and thereby to non-orthodox schools of economics. These commitments result in rejection of the interpretive approach that sees behaviour as the result of purposeful action. While such behaviour is still included in analyses, it is treated as merely epiphenomenal.

Likewise, those with ‘strong’ (epistemological) commitments (Kincaid 1996) also appear threatening. (Also troubling are those more enthusiastic than usual about ‘naturalism’ (Kincaid 1996, p. xv; 2012, p. 8; see also Kincaid and Ross 2009, p. v)). These naturalists often view epistemology as having mainly, if not only, a descriptive (as opposed to a normative role): they fully accept Quine’s dictum that philosophy is ‘continuous with science’. Kincaid (2013, p. 796) seems to adopt this dictum since he explains that, for him, a naturalist approach ‘takes social science and the philosophy of social science to be continuous . . . issues in the philosophy of social sciences are in the end empirical issues’ with conceptual matters taking second place. These naturalists, then, reject any ‘extrascientific gold standard for adjudicating knowledge claims’ i.e. they dispense with *any* role for philosophy in pronouncing on what constitutes knowledge (Roth 2013, p. 648). ‘There is no useful mission for the philosopher of economics to perform that an economist [scientist] could not in principle perform at least as authoritatively’ (Ross 2014, p. 6).

Although Quine, Kuhn and Lakatos ‘were instrumental in the transition from positivist philosophy of science’, their philosophical views and arguments about how science should proceed have been superseded by (this version of) ‘naturalism’. Writing from a holistic perspective, Kincaid (2012) rejects much of Quinean philosophy. Firstly, there is the case of the Duhem-Quine thesis concerning the underdetermination of theory by data. While Kincaid accepts, with Quine, that a hypothesis cannot be tested on its own but only together with a host of interrelated theories and assumptions, this does not

mean that 'evidence only bears on theories as wholes' (p. 6). Instead, he argues, it is possible to test individual theories. 'Indeed this is how the overwhelming majority of scientists view the overwhelming majority of their own research results' (p. 6). Secondly, the '1970s' notion that facts may be theory-laden presents no philosophical problems. It 'does not mean that every piece of data is laden with whole theories, and does not prevent the triangulation and piecemeal testing of specific hypotheses characteristic of good science' (p. 6). Thirdly, he rejects Quine's rejection of explanations which invoke meanings/mental states. It is, he explains, due partly to Quine's version of physicalism. In this version his physical facts refer only to 'medium-sized physical objects when there may be richer sets of facts that are relevant (Kincaid 1996, p. 198). Quine also rejects such explanations because, he says, meanings cannot be rendered into the precise language needed for a formalized theory (p. 195). However, even the natural sciences do not meet this requirement: the biological category of 'species' is not determined by the physical facts. In light of these considerations, Kincaid argues that 'the relationships between theories, applications, and tests propagated by Quine, Kuhn, and Lakatos look like philosophers' fantasies' (p. 6).

Both these 'strong' (ontological and epistemological) positions wholeheartedly reject the positivist tradition as well as that of Quinean naturalism which 'remain in the positivist tradition' (Kincaid 1996, p. 192). One of the tenets rejected was the positivist notion that philosophers could find general criteria to demarcate science from non-science. This was found to be 'a misplaced enterprise'. Any general demarcation criteria would be too abstract on their own. Instead deciding on whether an area is scientific or not requires a case-by-case approach 'based on specific empirical knowledge' (Kincaid 2012, p. 7).

Another tenet rejected rather than modified concerns the positivist notion that social factors are irrelevant to the practice of science. Instead it is argued that 'science is a social enterprise', although not 'a mere social construction'. As a social enterprise we need to empirically examine 'how science actually works' rather than look to the logic of explanation and confirmation. Concerning the view that it is socially constructed, we need to examine, for example, whether power relationships etc. influence science. This, however, is an empirical, not a philosophical, question (Kincaid 2012, p. 7).

Nevertheless they retain, or are sympathetic (for very different reasons) to, certain important positivist conclusions. Consequently, certain positivist tenets found to be unsupportable have been relaxed and modified rather than rejected. Moreover, there remains a general sympathy for positivism amongst these naturalists. For example, while Ross rejects 'positivist phenomenalism (and reductionism)', he claims that there are aspects of positivism worth preserving such as the unity of science and verificationism. 'There is nothing automatically wrong with positivism . . . Carnap, Reichenbach, Schlick exemplified the best and most worthy ambitions of philosophy' (Ross 2005, p. 118). 'Positivism, like other bygone intellectual movements, is probably inaccurately treated by its current detractors' (Kincaid 1996, p. 18). The core notions of positivism 'formed a lasting legacy' (p. 19).

Key positivist tenets retained (in modified form) by naturalists with ‘strong’ commitments

(1) the unity of science: ‘Individualist demands for reduction reflect a simplistic and outmoded picture of scientific unity’ (Kincaid 1996, p. 142). Kincaid argues that reduction (from macro to micro) is not the only route to the ideal of a unified science’. The ‘unity-without-reducibility model is a more accurate account of how scientific theories at different levels relate . . . [and also one that] leaves the social with an essential place’. Kincaid (1996, p. 5) contends that if a ‘real social science is possible’ then ‘biological accounts of human behavior must be consistent with – and, better yet, integrated with - those of the social sciences, just as biology must be integrated with chemistry’. ‘If individualism is true, we have support for the positivist idea that there is really only one big science, namely, physics. Holism argues for a more complex . . . account of how the sciences fit together (p. 8).

(2) a prescriptivist role for methodology: while the call is no longer for social studies to adopt a single scientific method, it is held that ‘good science must follow the standards of the natural sciences’ (p. 4). However, this is not to be interpreted as ‘an eternal conceptual truth’ since the social sciences might teach us something about scientific virtues (p. 7). Hands (2001, p. 403) responds to heterodox economists’ complaint that the New Economic Methodology does not provide a platform from which to criticize the practice of economics. I argue that (at least certain versions of) naturalism seem to have filled up the vacuum left by the demise of the old rules-seeking methodology so decried by Hands. For naturalists with ‘strong’ commitments, while there is no single scientific method to follow, the rule is clear: ‘follow the “standards” of the natural sciences’. As Dupré (2003, p. 534) points out, for many critics economics has a ‘questionable’ scientific status. This sort of rule seems to me to attempt to distinguish the kind of economics that has ‘questionable’ scientific status from the kind that does not. But this, seems to me, to attempt too much, namely, to solve what remains ‘a central problem in contemporary philosophy of science’ (p. 534).

(3) scientism: ‘If any knowledge tells us the way the world is, it is scientific knowledge’ (p. 5). The natural sciences are a paradigm of rational investigation’ (p. 29). Scientists clarify concepts ‘in ways far more sophisticated and empirically disciplined than the traditional philosophical practice of testing proposed definitions against what we would say or against intuitions’ (p. 6). Ross (2005, p. 16) espouses ‘the charms of scientism’ ‘without blushing’. This belief in scientific progress leads to a wholehearted embrace of an absolutist or Whig interpretation of the history of economic thought (Ross 2005, p. 15). This seemingly dismisses the criticism of such an interpretation, not only of Butterfield (1931) himself but also of economists who have highlighted this problem (Blaug 1968, Freeman et al. 2014). ‘Science is supposed to be self-corrective’ (Rosenberg 2016, p. 299).

(4) science as an objective enterprise: Kincaid dismisses arguments that science is inherently value-laden: ‘Nothing here shows that [non-epistemic] values can or must permeate good science’ (p. 46). ‘Values can cause bias in some cases but not in others . . . [they] can both lead to better science and to bad science’ (Kincaid 2012, pp. 16-17).

However, these ‘strong’ versions of naturalism contain (philosophical) problems that are not easily resolved. For example, Rosenberg (2016) identifies a problem involving intentionality, while Strawson (2018) identifies a problem involving consciousness.

Intentional behaviour

Kincaid puts forward a view of physicalism that seems to overcome Quinean problems. He writes: ‘as an ontological view [it] makes two basic claims: (1) that physical entities “exhaust” what there is . . . and (2) that the non-physical facts “supervene” on the physical’ (Kincaid 1996, p. 151). This version of physicalism, he says, supports ‘a kind of dependence of the mental on the physical without having to defend the implausible [Quinean] assumption that mental terms can be defined in or reduced to physical terms’ (Kincaid 1998a, p. 487). However, the fact that desire, belief and action are all intentional remains a problem for naturalists committed to a supervenience-enhanced form physicalism.

The meaning of intentionality in philosophy differs somewhat from its ordinary meaning which contrasts intentional, or purposeful, with accidental, behaviour (Rosenberg 2016, p. 55). In philosophy the variables of desire, belief and action are all intentional in that they have ‘propositional *content*’, that is, they ‘*contain*’ – in some sense - propositions (pp. 60-61). Thus, there cannot be a belief without a proposition believed (p. 62). In this case it would seem that the physical matter in our brain would somehow have to ‘contain’ (store and retrieve) statements representing the way the world is. But this seems to involve an absurdity. It would be something like saying that each of the cards in (old fashioned) library card catalogues directly represents a book on the shelves when, as we know, they represent the books only because library users interpret them as doing so (pp. 62-63). And, it is only because the library users interpret the cards as having a meaning, that the cards represent the books. The issue then revolves around the problem of how physical matter can symbolize or represent, that is, be about the world – how such matter can represent the world to itself (p. 63).

Consciousness

This long-standing problem in philosophy has been addressed in recent years by Dennett (1969, 1987, 1995). Ross (2005, ch. 2) adopts Dennett’s theory of intentionality and indeed a ‘package’ of ‘Dennettian’ philosophical ideas (p. 36). For some, Dennett’s theories deny the existence of consciousness, that is, the subjective character of experience. Strawson (2018) quotes Dennett’s response to the existence of consciousness: ‘subjective conscious experience – the redness of red, the painfulness of pain – that philosophers call *qualia*? Sheer illusion’. Strawson calls this ‘Denial’ the silliest view ever held in human thought’ (p. 8). What Denialists need is a ‘sharp kick’! These Deniers assume – unlike early 20th century naturalists such as Russell – ‘that we know enough about the physical to be certain that experience can’t be physical’. Strawson agrees with Russell that we can’t rule out this possibility. While physics makes statements (in terms of quantities and relations) about physical reality, it tells us nothing about the *stuff* of reality (p. 6). This is the point that destroys the position of the Deniers.

Conclusion

In this paper, I have attempted to speak to economists and economic methodologists rather than philosophers of science. Accordingly, my aim has been to provide account of ‘naturalism’ to a readership that probably has only a vague idea of the concept of ‘naturalism’. As a means of clarifying this concept I have followed Rosenberg (2016) in juxtaposing the two traditions long-established in economics (at least

from the time of Adam Smith) of 'naturalism versus interpretivism'. If the reader gains some basic idea of the 'debate' then my aim will have been fulfilled.

Consequently, I leave for a later paper an explanation of how the approach of naturalism, via its promotion of economics as an objective science and derogation of the value of abstract theorizing, not only reinforces the status quo in economics but actively militates against the expression in economics of dissent from schools of economic thought alternative to the mainstream. Furthermore, it militates not only against dissenters from orthodoxy but even against those with no axe to grind, namely, orthodox economists studying the history of economic thought. If this trend continues, it will eventually lead to the elimination of dissent in economics. While naturalism views this as a good thing, at least from the perspective of Braybrooke's three-schools distinction it will lead to 'an impoverished picture of human life' (Braybrooke 1998, p. 842).

Accordingly, I have argued that certain forms of naturalism threaten dissent within the philosophy of social science. This argument hinges on the assumption implicit in Braybrooke's three-schools distinction. This is that within each of the three schools there are philosophical claims (and worldviews) about the nature of society unique to that school (or that cannot be adequately dealt with within the framework of another school). It follows that the claim of any one school to resolve the issues raised by the (unique) claims of another school must be rejected. Any school making such a claim would mean that the worldview, for example, of one school simply becomes replaced by the worldview of another. In this way, ideas that might have arisen from the school that has been taken over is no longer articulated: dissent has been eliminated.

The next link in my argument depends on the assumption that (philosophical) ideas can and have influenced the actual practice of economics. Furthermore, it depends on the assumption that the influence of the philosophical ideas of first positivism and now naturalism have impacted mainly orthodox mainstream economics, rather than alternative schools of economic thought. I have attempted to substantiate both these notions in section 2.

A philosophical claim unique to the interpretive school (as depicted in Braybrooke's three-schools distinction) concerns a notion of meaning that underlies the view that economic (human and social) action is the result of individual purposiveness. In this case, and from the preceding argument, claims to adequately deal with meaning and intentionality from a non-interpretive school such as naturalism must ring hollow. Consequently, Kincaid's (1996, p. 192) claim that meaning 'poses no obstacle in principle' to naturalistic science must be rejected.

To the extent that it becomes generally accepted within economics that the notion of meaning can be adequately dealt with in terms of a naturalistic approach to economics, dissent arising from schools of economics closely linked to the ideas of Braybrooke's interpretive and critical schools will be eliminated.

Readers may well ask, if a monopoly of 'naturalism' is to be avoided, does this mean that looking to the 'interpretive' school is the only option? What are we to do? Here I follow Glock (2008, p. 114) since his ideas seem to me to apply at least in some way to economics.

Glock is critical of the 'naturalistic' turn in contemporary analytic philosophy that, he says, displays a 'tendency to treat many fundamental issues as settled once and for all, and a predilection for technicalities irrespective of their usefulness'. Similarly, contemporary economics treats fundamental

issues such as the determination of the level of aggregate output and employment as settled by the outcome of the neoclassical synthesis and ignore Post Keynesian protestations to the contrary. I also treats uncertainty as risk ignoring the Austrian stress on the importance of subjectivity in economic decision-making. Its predilection for technicalities is seen in the attempt to subject every (trivial) economic problem to econometric analysis despite results so vague as to be virtually useless.

Furthermore, Glock says, there is 'a general attitude that those who . . . dissent from the naturalistic orthodoxy or demand explanations of an established jargon, for example, are simply unprofessional'. Likewise in economics those who dissent from the naturalistic orthodoxy or demand explanations of say, rational expectations theory, are simply unprofessional.

So, once again, what to do? While of course 'interpretive' ideas as alternative to 'naturalist' ideas would now be possible, this does not mean 'interpretive' ideas are the only option. Glock looks to a 'recovering some of the virtues of twentieth-century analytic philosophy'. Economists (and particularly economic methodologists) could look to remedying Hands's (2001, p. 10) admission that he neglects in his book 'any serious discussion 'of the methodological ideas of economists like Fritz Machlup, Joseph Schumpeter, Tjalling Koopmans, or Wesley C. Mitchell'. Many more can be added to this list e.g. Knight, Viner, Hutchison, John Maynard Keynes etc. As Hutchison (Hart 2002, p. 369) says, 'if philosophers try to tell economists what they really mean they may not be right. I want to derive the foundations of the subject from *inside* the subject' (Hutchison 1938: 16-18). Contrary to Davis and Hands (2011, p. 1) claim that 'normative methodological agendas associated with philosophical imports' have begun to be set aside, from within certain forms of 'naturalism', some philosophers are continuing to tell economists 'what they really mean and they may not be right'.

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Note: To cut down on my list, I have omitted various well-known works in methodology and philosophy.

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