‘OPEN SYSTEMS’ AND ECONOMIC METHODOLOGY

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Abstract:

This paper discusses some of the methodological implications of an ‘open-systems’ reality. It presents a possible ontology of open systems which draws on various literatures including, but not limited to, Critical Realism. The paper then extrapolates from the ontology to a set of methodological arguments. Many methods in economics presuppose a degree of closure in their operation. Deductive logic is discussed in this context. This constitutes a disjuncture with reality. It could be argued, therefore, that these methods should be rejected. However, an open-systems methodology is also an open system and it will reflect the impact of other literatures. Thus, based on fallibilism and an avoidance of dualism (in Dow’s terms) rejection of so-called ‘closed-systems methods’ is not an option. Also, given the preponderance of closures in available methods, this would leave little scope for investigation. Thus, a strategy of triangulation should be adopted.

Keywords: methodology, open-systems, triangulation, Critical Realism, deduction, dualism

JEL codes: B4, B5, P0, Z0

OPEN SYSTEMS AND ECONOMIC METHODOLOGY

1. INTRODUCTION

This paper discusses the concept of 'open systems', which has recently become extremely influential. In particular, it has been embraced by non-mainstream[1] approaches, such as many (Old) Institutionalists (Hodgson, 2000, Eberle & Hayden, 1991, and Veblen (Mearman, 2002a)); Post Keynesians (see Downward, 1999; Dunn, 2001); 'Austrians' (see, for example, Shackle's (1955) description of the 'kaleidic' world, as subject to massive unforeseen changes as the result of small shifts in individual action); and Marxists (Tsuru, 1997). These schools of thought have all recently been influenced by Critical Realism (CR) (see Lawson, 1997, 2003a), in which 'open systems' is a pivotal concept. Moreover, orthodox economists are also concerned about identifying underlying mechanisms which are difficult to identify in a complex open environment. The work of Hendry (1995), Sutton (2000), Hoover (2002), Morgan (2002) and even Friedman (1953), plus the language of ceteris paribus descended from Marshall are evidence of this. 'Open systems' is a concept encompassing economics.

However, despite this recent prominence, Hodgson (2000) and Mearman (2002b) argue that the concept of open systems is underdeveloped. This paper aims to begin to remedy this. The paper is concerned with two questions: first, what is mean by the concept of 'open systems'? Second, what is the nature of 'open-systems methodology'? Section Two of the paper presents a sketch of a possible open-systems ontology, which augments the influential Critical-Realist treatment with material from General Systems Theory, Sheila Dow and others. Therefore, the ontology presented reflects the intellectual history of the concept. The remainder of the paper addresses the nature of an 'open-systems methodology'. The paper

argues that open systems have several important implications for economic methodology. Here, deductive logic is discussed; several problems with its application in open systems are highlighted. Deductive logic exemplifies a group of 'closed-systems methods' because it entails the assumption that closed systems exist in reality; or they impose an assumption of closure on a reality. A commitment to open systems implies that the power and reliability of many standard elements of economics are much reduced. However, several arguments are presented against outright rejection, because of the fallibility of knowledge, but mainly because rejection is shown that all methods presuppose closure. Thus, rejection of methods which impose closure would leave economists without research tools. A strategy for combining closed- and open-systems methods is then discussed.

2. ONTOLOGY[2]

This goal of this section is to present a general picture of an open system. The notion of an open system has a long intellectual heritage in various disciplines. The ontology presented here draws on accounts that have been found in economics. The premise of the section is that existing accounts of open-systems ontology are partial and that a more satisfactory and complete account might be achieved by combining existing treatments. This description of reality is provisional and fallible and by no means the only one possible; however, an ontological commitment (Ardebili, 2003) is made to this ontology. What features are salient to an open-system ontology? Figure 1 provides a brief overview of how an open system might be represented.

Figure 1 here

The starting point of the account is one of the foundational arguments of Critical Realism. That is, Bhaskar (1978) presents a transcendental deduction of what the world must be like for experiment to be possible. Bhaskar argues that the experimenter must be searching below the level of events for some structure, embedded in which is some deeper causal mechanism(s), the action of which cannot normally be controlled (and therefore triggered by the scientist) or observed, because it operates in a complex of other mechanisms. Therefore, reality is stratified, such that events and experiences are found on the surface, generated from below by causal mechanisms. Essentially, this paper accepts this argument as valid and applicable to the social world.[3]

Perhaps controversially, this paper holds that there is a belief, common to economists, in a domain of mechanisms, in some sense set apart from events and experiences, but not easily accessible because of the openness of the environment. There are certainly differences between the various accounts, of course. However, this language is quite consistent with Marxian accounts, which stress underlying forces of capitalism. For (Old) Institutionalists, the mechanism in question might be a habit of thought. Austrian economists might envisage the causal mechanism as residing within the individual. Of particular significance in this context are nominally orthodox accounts, such as that of Hendry (1995), Hoover and Siegler (2000) and Sutton (2000), which use a language of mechanisms or causal structure and stress the need for quasi-experimental situations. Sutton, for example, cites Robbins (1932) as demanding some underlying mechanism (2000: 12). He also cites Haavelmo as searching for mechanisms (2002: 17). Sutton (2002: 19) discusses systematic and disturbance mechanisms. He asks: 'Are there economic mechanisms that force the level of concentration in some industries to be very high…?' (2002: 65). He also states that the goal of theory 'is to try out

fitting some model with a view to uncovering the mechanisms that are driving outcomes in some particular data set' (2002: 92). Morgan (2002) uses similar language.[4]

What are the sources of 'openness' in this type of system? It was claimed previously that reality involves strata, many of which contain causal mechanisms. One source of openness is that one stratum of reality can exhibit emergent properties rooted in but irreducible to the strata below. For example, the properties of water cannot be explained in terms of the properties of hydrogen and oxygen (Bhaskar, 1979). Economic mechanisms cannot be merely reducible to the physical. Evidence for emergence can be inferred from the human genome project (see Wade, 2001). In addition, strata can interact in non-predetermined ways. Physical urges within humans are combined with emotional and rational factors to produce human action. Economic and social structures, such as organisations and their rules, interact with the individual agents within them. Consequently, mechanisms might have a tendency, or way of acting, if triggered. However, each mechanism interacts with other mechanisms. Thus, when one mechanism is triggered, its effects are combined with other mechanisms. In this way, if an event occurs and an event is triggered, it is likely that the effects of the mechanism will be affected by other mechanisms. It should be noted that events do trigger mechanisms, and hence the arrows in the diagram are double-ended.

The analysis thus far has drawn extensively on Critical Realism. However, it can be argued that the Critical-Realist treatment of open systems is limited (Mearman, 2002a, 2003a). This is because, for instance, its concept of 'system' is redundant. Moreover, economics literature has made use of other concepts of open systems which emphasise the notion of system (see Dow, 1996; Eberle and Hayden, 1991; Hodgson, 2000, Boulding, 1971, Georgescu-Roegen, 1971). Drawing on that literature, the system as conceptualised here, is conceived as having

boundaries; however (from Dow), these boundaries are fuzzy, changing, malleable (by agents) and permeable (from General Systems Theory). Moreover, there might be sub-systems within the larger system, which might exhibit the same properties as the larger system (Dow). These sub-systems interact; this means that the mechanisms within each sub-system also interact. This is facilitated by the permeability of the system boundary. Lastly, it is argued that as the system is moving through time, mechanisms (within and outside the system) will operate intermittently.

Thus, events in open systems will be triple conjunctures: they are the product of the interaction of strata and mechanisms within the system, and with mechanisms from outside the system.[5] What is clear is that in an open system, as formulated here, it is unlikely that neat regularities of events will spontaneously arise. The presence of intermittent mechanisms means that, through time, it is unlikely that outcomes in the system can be predicted, or that regular successions of events will occur. Therefore, the description of open systems presented here is consistent with the Critical-Realist definition of open system, but it is explicitly different from it. Lawson (passim) and Critical Realism argues that open systems can be defined in terms of a lack of event regularities. However, Mearman (2002a, 2003a) argues that this Critical-Realist definition of open systems is problematic, partly because it tends to be dualistic and encourage negativist categorisation.

Crucially, in this formulation, a closure occurs whenever one of the elements of openness described here is removed or is not present. Given that there are so many sources of openness and closure, it is possible to have some present and others absent; this suggests that degrees of closure are possible. Furthermore, in contrast with Critical Realism, the closure is not defined as, or in terms of, event regularities.

3. METHODOLOGY

The account of methodology offered here proceeds on the basis of the ontology presented in Section 2. It is argued that an open-systems methodology, i.e., a methodology consistent with the concept of open systems, will have two main features. First, it will reflect directly the elements of the ontology just discussed. Second, if reality is best conceived in terms of open systems, then it seems best to conceive of its components in those terms. So, an open-systems methodology will itself be an open system. These two components are discussed in turn.

3.1 Direct implications of open-systems ontology for methodology

The first, obvious implication of the philosophical ontology just described is that in economic investigations, those ontological features should be assumed to hold and/or to be sought. It might be objected that this engages in excessive a priorism. However, other approaches to economics adopt the same stance: for example, CR takes Bhaskar's (1978) original analysis of an experiment to claim that economists should search for the relatively enduring causal mechanisms, set within entities, within structures, most relevant to explaining economic phenomena (see Lawson, 2003a: 154). Obviously, this could mean that economics examines a far greater number of mechanisms and indeed phenomena that it currently does. An open-systems economics would be much more a 'broad economics' (Stretton, 2000: ch. 5). Most significantly, though, the aim of economics is to identify economic mechanisms which generate economic phenomena.

The essential methodological issue at stake is the extent to which economists take seriously the implications of the ontology and are less prepared to make assumptions that bypass the ontology. The position of this paper is that there should not be a large disjuncture between the methods and the object. This follows directly from a commitment to a basic realism, which most economists share. Methodology should be appropriate to the object of study. Thus, experiments are employed only where possible. In fields such as geology and meteorology, experimentation is not possible, or not useful, so other methods are used. There is some evidence that economists agree on this; but there are differences of opinion on what is an appropriate departure from that ontology. Some economists will argue that it is necessary and acceptable to 'close' the system (in the sense of section 2). This means that they will attempt to somehow remove a (set of) source(s) of openness. An experiment can be interpreted as an attempt to introduce real closure into the system; however, often the closure (as defined here) is only by assumption and is not real. The introduction of an error term into a regression equation is an attempt to mimic the effect of sealing off the system, or in terms of Figure 1, rendering the boundary solid and impermeable. Some economists, such as those influenced by Lawson, will regard this as unacceptable. Others will argue in its favour. Friedman (1953) offers an extreme form, which almost rejects the relation between the object of enquiry and its methods of investigation. Instead, his guiding principle is predictive instrumentalism.[6] This methodological contrast clearly applies to the contrast between orthodox and heterodox economics, but equally also to the delineation of approaches within heterodox traditions. For example, it is contentious whether the seemingly closed-systems neo-Ricardian approach sits within the seemingly open-systems methodology of Post Keynesianism (cf. Pratten, 1996; Downward, 1999). To reiterate: the crucial issue is whether the methods discussed are appropriate to the ontology committed to. In each case, where the methodology makes assumptions that appear to violate the ontological commitment, this provides grounds for

regarding the method as somehow flawed. Logic in open systems will be discussed to exemplify the relevant issues[7].

3.2 Logic in Open Systems

Commonly, economists utilise logic of two types. Induction is the movement from the specific to the general. If one hundred observed swans are white, the inductive inference is that all swans are white. Via the formulation of the problem of induction, it has been well established that such an inference is inappropriate, because it makes unjustified universal assumptions about reality. Induction is invalid in open systems, because it makes the assumption that reality is homogeneous. In Critical-Realist parlance, induction assumes that the Intrinsic Condition of Closure (ICC) holds – that the object remains constant or changes at a constant rate.

However, while the problem of induction is well known, deduction is regarded as altogether more robust. Deduction is often expressed in the form of a syllogism, incorporating a number of assumptions which lead to a conclusion. An example would be to assume that Peter is a fish; all fish are cats; and that therefore Peter is a cat. The assumption 'all fish are cats' indicates moving from the general to the specific; this is the case in the deductive-nomological model; however, this is not necessary; assumptions can in fact be quite specific. Clearly, syllogisms and other deductive forms are extremely common in economics. For example, if we assume that a price has fallen; and that demand curves are downward sloping; then it would be concluded (predicted) that quantity demanded will increase. This is of course the familiar orthodox formulation of demand.

An essential feature of a deduction is that one moves directly from the assumptions to the conclusion. In terms of Figure 1, in a deduction, the mechanisms are replaced by assumptions, and events are replaced by conclusions. The movement from the assumptions to the conclusions is assumed to be direct, perhaps linear. In addition, of course, there is a missing (unstated) ceteris paribus assumption. The mechanism in question (the mechanism of demand, which, in the orthodoxy, tends to mean that people demand more of a good in response to a price change) is treated as being isolated from other mechanisms. Thus, the introduction of an assumption like 'the person is feeling rather pessimistic today' could alter the conclusion. Other prices that the consumer faces might have risen, thus triggering countervailing mechanisms, which potentially affect the outcome. At this point, the implications of open systems on deduction should become apparent. Ceteris paribus acts to close the system (in the mind of the economist) in two ways. First, it allows economists to assume that over the period in question, the mechanism in question does not change (the ICC applies). Second, other mechanisms are excluded: in terms of Figure 1, the boundary around the syllogism (system) has become solid and impermeable.

Of course, orthodox economists are quite aware of the considerations above. Indeed, the following argument is standard in Economics textbooks: the demand curve is constructed on the basis of ceteris paribus; of course, ceteris is never really paribus, but it is a reasonable assumption to make; and it is necessary if the analysis is to move forward (cf. Hahn, 1973, on equilibrium). Again, of course, if an ontological commitment has been made, then according to realism, there must be an assessment of whether the assumption made is reasonable. It is uncontroversial to claim that Demand curves do not really exist. A similar argument can be made about homo economicus. No explanation is offered in terms of the physiology of the individual; its psychology is reduced to 'rationality' of a very narrow form. Homo

economicus might not exist. However, orthodox economists hold that such assumptions aid analysis. This is explored below.

More significant presently is the general question about the validity of deduction. Let us imagine the syllogism again. It might contain two assumptions and lead to a conclusion. It has been already argued above that a deduction assumes that the mechanism in question is enclosed by a solid, impermeable boundary and thereby isolated from other effects; i.e., it is closed off. In Critical-Realist terms, the Extrinsic Condition of Closure (ECC) holds. However, in reality, in an open system (as defined here), the ECC does not hold: the boundary of the system is permeable. Therefore, other mechanisms might enter from outside the system.

However, there are other ways in which the deduction is rendered invalid. First, two mechanisms in a system might be internally related; i.e., they are mutually constitutive. Most deductions (and economic and econometric models) assume the independence of the explanatory/causal mechanisms. However, if they are interconnected and perhaps internally related, the nature of the relationship will affect the outcome of their combination. For example, economic and philosophical theories might both be causal in creating a person's beliefs. If the two branches of theory are considered independently, then the deduction of the beliefs might be calculated quite mechanically from them. However, if there is interaction between the two branches, then the calculation of the outcome of the combination will no longer be a simple be mechanical procedure, because one mechanism affects the composition of the other in undetermined ways.

Another problem of deduction in open systems can be seen if the mechanisms reside in strata. For, if the strata have emergent properties, this changes the outcome of the deduction. For

example, philosophy and economics are mechanisms in the creation of ideology. However, because philosophy shapes (in some way) economics, they can be conceptualised as being on different strata. For example, economic theory A' is based on philosophy A. Therefore, if philosophy A has characteristic 'x', then it seems reasonable to deduce that economic theory A' must also express philosophical characteristic 'x'. However, this assumes closed systems. With emergent properties, the higher stratum (economics) is rooted in but irreducible to the lower stratum (philosophy). The higher stratum has, by various means, developed characteristics of its own that are not contained in the lower stratum. Indeed, one cannot move directly from the lower to the higher stratum. Thus, it is not possible to deduce the higher stratum from the lower. Therefore, economics A' might not express x, but instead might express x in a modified form, x', or even express a different proposition 'y'.

Also, the effects of unknown or hidden assumptions beneath the known or stated assumptions disrupt the deduction. This is particularly so if the stratum of assumptions is rooted in even lower strata, but if nothing is known about those lower strata and their effects. In this case, it is no longer possible to simply deduce from the assumptions to the conclusions. For example, an orthodox economist might also consider some Christian values true. However, it is not a simple matter to reconcile, say, their belief in the Laffer curve (based on their economic beliefs) with their (Christian) belief that people should give up their riches. For, if it is accepted that orthodox economics is erected on positivism, that philosophy precludes a belief in gods and thus complicates the economist's belief in the Christian religion. Similarly, even if one knows of the economic and political strata in a country, physical elements in that place might have an impact. A hurricane can ruin economic policy making. Thus, it is not straightforward to derive conclusions based on assumptions, when there are deeper assumptions which would affect the outcome.

Thus, because of relations between mechanisms (assumptions) in a system (syllogism); because of the effect of other, interceding causes (assumptions, facts, ideas) on the system; because of (the possibility of) the existence of emergent properties and of unknown lower mechanisms; then, the effect on the outcome (the conclusion) of the initial mechanisms cannot be deduced simply from those mechanisms. Thus, for a deduction to be undertaken unmitigated requires an assumption that none of those other effects can occur, or that they are negligible. This might or might not be a trivial assumption. Indeed, for a realist, it can be regarded as a highly dubious assumption, and therefore a dubious method. It might then be argued that deduction should be thrown out of any open-systems methodology. This conclusion would pose several problems for many economists. Orthodox economics relies on such logic. Indeed, it is possible to interpret Marshall's warning (1890: 773) against long chains of logic as reflecting a concern about openness. Moreover, some heterodox economists use deductive systems. The Post Keynesian, Paul Davidson is one example. Davidson (passim) highlights major axioms that, according to him, are thrown out by Keynes: the axioms of gross-substitutability, ergodicity and the neutrality of money. Subsequently, a general (Post Keynesian) theory contains fewer axioms than an orthodox theory. However, the argument above clearly makes such a position problematic. For, there is no reason to assume that any such system is closed from other influences. Indeed, those that advocate Davidson as exemplifying open systems must reconcile this claim with the argument above.

3.3 Open-Systems Methodology as an Open System

Economic methodology is an object in the world. Thus, if the world comprises open systems (albeit closed to different extent in each case), then economic methodology should be

conceptualised as an open system. Economics can be considered as a stratified entity (Dow, 1999). So, economic theory might simply be one layer in a larger structure which involves ontology, methodology and policy application. Economic practice by agents, such as policy, can be seen as the consequence of various factors. A government policy might have been created (at least partly) by an economist, who him/herself has been informed by a theory, and therefore by a methodology, including ontology and epistemology. This is the case even if the economist is unaware of this. So, there is a clear notion of the methodology creating the conditions for the theory, which in turn creates the conditions for the policy choice. However, as a welter of literature on government policy would argue, policy is also affected by political factors. Policy determination is open: it is determined by neither politics nor economics, but by a combination of them, plus other factors.

It would also seem reasonable to imagine that methodology as having a permeable and otherwise undetermined boundary (cf. Dow, 2003). Therefore, the open-systems methodology will not merely reflect the ontology above.[8] Other epistemological literature which seems consistent with the open-systems argument (and, perhaps, some which does not) will influence an open-systems methodology. It will also be affected by other philosophical thought and by politics. For example, Dow (1996) argues that 'vision' has an impact on methodology and on theory. Indeed, Mearman (2002b) claims that Dow's Babylonian approach is a synthesis of two mechanisms, so to speak: the concept of vision (which, for Dow, is akin to ontology) and the sociology of science literature (for example, Kuhn's work). Dow emphasises 'vision' and seems to argue for the desirability of values. Indeed, the Critical-Realist Sayer (2000) (perhaps with the developing Critical-Realist theory of ethics; see Collier, 2000) seems to be attempting to create an extra layer in CR that echoes Dow's (1999) argument that 'vision' provides a fundamental stratum of her Babylonianism. This is

what allows her to talk in terms of a political economy, i.e., as being associated with some, if not a single specific, political position.

Dow (2003) also discusses 'schools of thought'. Such a position can be understood in terms of the open-system ontology offered here. Many economists might subscribe to open systems. For example, Austrians and Post Keynesians, express similar views on issues such as uncertainty (which clearly reflects open systems). However, Austrians and Post Keynesians reach extremely divergent opinions on substantive issues based on very different theoretical approaches. This partly reflects a difference of opinion on what is important; i.e., their values.[9] In this way, an open-systems approach can be seen as encompassing various paradigms. This claim is somewhat controversial. Orthodox economics in particular has claimed that there is a strict distinction between facts and values (the positive/normative distinction; for example, Friedman, 1953). That distinction is standard fare in economics textbooks.

Less controversial is the claim that outside factors influence economics. One need only point to the influence of mathematics (Weintraub, 2001), physics (Mirowski, 1989), biology (Hodgson, 1999), systems theory (Boulding, 1971) and psychology (Kahneman & Tversky, 1988) on economics. Moreover, there is the increasing importation of economic frameworks into other subjects, such as political science (Niou and Ordeshook, 1991), history (Pope, 1981) and sociology (Heckathorn, 1984). Clearly in these latter cases, the disciplines are open systems. Clearly also economics has left its own domain and its effects are felt elsewhere. Thus, it too is open. So, even one who rejects the apparent conflation of fact/value in the claim above, can observe some openness of economics and to economics from other disciplines.

3.4 Should 'open-systems' economists reject 'closed-systems' methods?

At this point we return to the crucial methodological question. Deduction was analysed and found strictly to be inconsistent with a commitment to an open-systems ontology. As Downward and Mearman (2002), Finch and McMaster (2000) and Downward, Finch and Ramsay (2002) have shown, technically the same argument can be made with respect to the act of quantification, and by extension, to all quantitative methods. Dow (1996) argues that in considering open systems, it is necessary to segment them into smaller, more manageable, and closed, systems. However, a strict interpretation would argue that realist economists who believe in open systems should reject such 'closed-systems methods': because the methods introduce closure, strictly they are not appropriate in an open-systems methodology, i.e., one which takes seriously open-systems ontology in its choice and application of methods. However, as is argued below, that is not the position of this paper.

Open-systems economists, i.e., those who aspire to an open-systems methodology, cannot and should not reject closed-systems methods. This position is based on several arguments. First, Mearman (2002a, b) argues that in fact the open/closed distinction is not strict. This is evidenced by Lawson's concept of the demi-regularity: if a closed system is characterised by a complete regularity and an open system by a lack of a regularity, the demi-regularity might indicate a partial closure. As Mearman (2002b) argues, partial closure can be of three types: a universal incomplete closure; a local complete closure; and a local incomplete closure. Also, Cottrell (1998) argues that regularities of the sort Lawson denies are quite commonplace. Lawson responds to show that even if a regularity seems to be complete, it is most likely subject to change or exception. One way to interpret this debate is that there is a range of

open-closed systems. It is also reasonable to hold, given the analysis (above) of deduction and other methods, that some methods are more 'closed' than others. Based on the realist demand for ontological licence already mentioned, it would seem reasonable therefore to insist that methods which are more closed be used in situations of higher closure, but that methods of less closure be used in situations of more openness.

In section 3.3, it was argued that an open-systems methodology would itself be an open system. Thus, other methods or concepts can have impact on an open-systems methodology. Of course, as well as the account offered above there is an existing literature which might be interpreted as reflecting open systems. Dow's (1990) work on dualism is one example. Dow defines dualism as '…the propensity to classify concepts, statements and events according to duals, as belonging to only one of two all-encompassing, mutually-exclusive categories with fixed meanings' (1996: 16-17). Such categorising implies specific ontological assumptions; and therefore, by extension, for dualism to be realistic, those features must exist. However, Dow argues that these necessary conditions include atomism; external relationality of objects (so that they can be thought of as strictly separate); a particular form of methodological individualism (for the same reason); a mechanistic approach, under which economic agents are assumed to act in uniform, formulaic, pre-programmed, hydraulic ways, aiding their categorisation as either 'x' or 'y'; and a constancy of structure such that a fixed category can be imposed on the object. Dow (and others) argue that these assumptions are contrary to (for example) the advances made in post-Newtonian physics; to the philosophy of internal relations; to the apparent fact of the openness of the world (as evidenced by the need for, and attempts to establish, experimental situations); and to the possibility of organic relations and entities, of emergent properties and of real conscious intentionality. Significantly, these latter

elements are consistent with open systems as described above. Thus, Dow argues that in open systems, there is no ontological licence for dualism.

In addition to these ontological concerns, Dow argues that these features of reality have sharp epistemological consequences. Given the complex and differentiated world, the quest for certain categories such as true/false and rational/irrational is likely to be fruitless. If we also acknowledge literatures which question the notion of a certain truth, there is clear support for questioning of dualistic categories. Dow (passim) notes that the 'New Physics', for instance that of Heisenberg, emphasises the basic uncertainty of knowledge (and the world) given that 'empirical observations are shown to be probabilistic, but, being observer-dependent, are also uncertain. All observations, therefore, fall into the "undecided" or "uncertain" category' (1990: 144). Additionally, Dow draws on literature advocating fallibilism. In a changing world, even given the role of paradigmatic rules and conventions, new knowledge claims are continually being produced and existing ones are examined and revised; categories are continually changing.

This discussion of dualism is relevant for two reasons. First, it shows another direct consequence of considering the world as comprising open systems. Second, and more importantly in this context, it shows that the strategy of rejection of techniques is unsustainable. The economist committed to open systems cannot reject techniques s/he regards as inconsistent with closed systems. This follows, because in a non-dualist process of categorisation, the terms 'reject' and 'inconsistent' are avoided; as argued above, the strict open/closed distinction lacks ontological support and would be avoided. Anything more than a temporary neglect of a (set of) technique(s) should be avoided. Thus, calls to abandon econometrics and other formalistic tools should be avoided. Of course, so too must

descriptions of some techniques as 'scientific' and others as not. Taking seriously open systems has implications for all economists.

Additionally, there are practical reasons for avoiding the rejection strategy. In short, if economists were to reject techniques such as econometrics because of (what are perceived as) their imposition of closure on an open reality, then, quite simply, they might be devoid of available appropriate techniques. Downward (1999) has argued that all techniques involve implicit closure: specifically, one must assume at least the constancy of the object under study (defined as the ICC). As Outhwaite (1998: 289) notes, a certain endurance of structures we has to be assumed, otherwise every individual time/space moment would require its own theory, and social science would effectively be impossible. Abstraction involves at least the assumption that an entity is identifiable and persistent. Other methods presuppose even more. Deductive logic clearly involves the assumption of the ICC; but as shown in section 3.2, it also assumes the syllogism can be isolated from external factors. Quantification involves the abstraction from qualitative differences and an assumption (rather boldly) of qualitative invariance (see Georgescu-Roegen, 1971). Lawson (1989) shows that an OLS equation involves both intrinsic and extrinsic closure. Given that case study methods, ethnography and other methods also involve many of these elements, they too must also introduce some closure. They impose less closure than an OLS equation, but nonetheless they do involve some closure. Finch and McMaster (2002) and Mearman (2002b) argue that Lawson's (1997, 2003a) proposed alternative to traditional methodology, so-called 'contrast explanation' (CE) also contains closure via its quantification and implicit use of trends, probability distributions and the concept of 'significance'.

4. ECONOMIC RESEARCH IN OPEN SYSTEMS

The analysis above has potentially far reaching implications for practising economists. The crucial argument of section 3 is that an open-systems methodology must take seriously the reality of open systems. Economists must accept that in open systems, some of their current methods are problematic. Indeed often the great difference between economists is not their view of reality, but their response to the reality as they see it. The essential difference between Lawson and others, and orthodox economists might be, therefore, that Lawson et al are less prepared to compromise their ontology by advocating methods which are contrary or inconsistent with it.

Section 3.4 argued that all methods of investigation involve some closure. In an open-systems reality, methods which involve closure imply a compromise by the researcher. This should be recognised: there is a disjuncture between closed-systems methods and reality. This does not mean a lack of correspondence between the reality and methods – they cannot correspond – but that the methods introduce inappropriate closures. However, all methods seem similarly flawed. Thus, inferences from every method should be treated cautiously. Given that, as argued above, there are different degrees of openness, in some systems, there will be more closure than in others. In such systems, methods which are more closed will have more power than they would in more open systems. This is a point long recognised by users of statistics in other social sciences, and recently realised by economists (see Journal of Economic Perspectives 2001). For example, where there are not grounds for using fully parametric statistics, or cardinal data, statisticians recognise that semi- or non-parametric statistics, or nominal or ordinal data, have more power. In this way, Lawson is correct to argue that his method of CE might be superior to econometrics, because it imposes less closure on an open

world; however, he also acknowledges that where closure has been achieved, econometrics can be used. Indeed, econometrics should be used in such circumstances, because it has greater power than other methods.

It is crucial, therefore, to match the degree of openness of the reality to the openness of the methods. One problem with that proposal it is difficult to establish the existence or extent of a closure. Lawson (passim) opts to identify closures by way of strict event regularities: the existence of a strict regularity implies a closure; its absence implies an open system. However, there might be other ways to test such things. Econometricians might claim that tests such as the Dickey-Fuller are relevant here. The use of dummy variables in statistics implies institutional and/or historical analysis: the introduction of a dummy variable often follows the identification of a significant event, such as a price shock, war or new piece of legislation. They are therefore a way of partially dealing with intermittently acting variables and thereby openness.[10] Further, Keynes (1973) advocated the pre-testing of economic material, for instance via Lexis' method. Under this method, small sections of the data were examined for their homogeneity. These methods might allow economists to reduce the disjuncture between the open reality and their methods; this would increase the power of those methods and, thereby, of the economist's inference.

The economist's interpretation of his/her own inferences is crucial here. Mearman (2002a, b) argues that a theoretical element can be evaluated differently depending on the means of its construction. An equation, might, therefore, have been constructed with the most thoroughly open-systems principles in mind; or it might not have been. However, the difference is crucial. A crucial factor in the use of an equation is in its interpretation. Does the econometrician acknowledge that the equation is highly faulty and distinctly unrepresentative

of the reality; or does s/he plough on regardless? An econometrician who ignores poor diagnostic tests would be scolded for doing so; yet it seems permissible for an economist to ignore obvious deficiencies in their method, for the sake of expediency. A realist abstraction comprises components which are thought to be really existing, not fictional entities designed merely to aid the analysis. However, an abstraction involves the possibly unwarranted introduction of a closure: it assumes the persistence of the object; and it involves the ignorance of the relatedness of the object to other objects. This may be inevitable and indeed necessary. Nonetheless, the act of abstraction involves closure for which the economist lacks ontological licence. However, the realist abstraction involves only that type of fictional assumption; it does not make fictional assumptions about the entity itself. Where the object itself is also fictional, the abstraction constitutes a 'double fiction'. In an open system, a realist economist should avoid such a double fiction. An abstraction (or any other method) which avoids the double fiction will have more power than those which do not. Further, whenever the double fiction is employed, it is crucial to recognise and allow for this during inference. Thus, while Senior (1836) and Mill (1836) both utilised homo economicus, Mill was aware of its fictional nature. Consequently, Mill's analysis would have more power than Senior's.

Clearly, such a methodology employs the concepts of 'weight' borrowed both from Keynes (1973) and from descriptive statistics. This notion of weight is crucial. In very open systems, methods predicated on complete closure (for example, traditional econometrics) have a low power and should be awarded a low weight in contributing to the inference. In Keynes' terms, the closed-systems techniques have less relevance and contribute to our relative ignorance and help us little in developing a degree of rational belief in a hypothesis. Weight attached to semi-parametric, non-parametric or descriptive statistics, or to qualitative methods is lower in

these conditions, as the power of those methods is much less than parametric econometrics (Siegel and Castellan, 1988). However, where measurement issues become crucial, as in Finch and McMaster (2002) above, this relative weight will change. For if the parametric assumptions and conditions for constructing interval scales underlying parametric econometrics are not met, then their power is reduced and the weight attached to them is reduced. One must then appeal to other methods. To increase the weight of parametric econometrics, pre-estimation methods could be utilised, as discussed above. One obvious objection to this scheme would be that it ignores the warning of 'garbage in, garbage out': if the validity of the use of parametric econometrics depends on closed systems, it might be argued that outside closed systems, their results are meaningless. Thus, they would receive a weight of zero. However, this is a difficult position to take, since once again it would lead to an argument that all empirical work is meaningless. Thus, one would be forced to revert to arguments of weight, which would appear to imply a non-zero weight for econometrics. Crucially, by extension, this argument applies equally to deduction, abstraction, and other methods which seem to presuppose closure.

However, even if the power of methods is increased in the ways discussed above, it remains the case that because of the disjuncture between methods and reality, and because of fallibilism, it is clear that no one method can be relied upon to produce reliable inferences. This suggests at least a pluralism of methods; and a method of triangulation (Downward; 1999; Downward and Mearman, 2002, 2003; Olsen, 2002). Downward and Mearman (2003) argue that a combination of qualitative and quantitative methods, plus a mixture of quantitative methods, might be beneficial. To be clear: this is not merely the collection of more studies using the same technique. Such a strategy could lead merely to the compounding of the flaws of the technique. Rather, by combining the different techniques in the act of

inference, the flaws of one technique will be counteracted by the presence of another technique. Of course this might not happen. It is also possible that one technique's flaws will reinforce the flaws of another. However, arguably this effect can be avoided or diminished if the techniques are combined in an appropriate way. It is a requirement of future research to formulate ways in which triangulation becomes a more rather than less reliable method of making inferences.

5. CONCLUSIONS

This paper has addressed the notion of open systems in economics. Necessarily, given the space constraint, this large issue has only received an introductory treatment. However, a number of issues have been raised which should inspire future research. A number of arguments have been made. First, a basic sketch of what is meant by an 'open system' was offered. This ontology borrows various literatures, including Critical Realism, Post Keynesianism and systems theory. It is consistent with the Critical-Realist definition of open systems, in terms of a lack of event regularities. However, it attempts to fill lacunae in the Critical-Realist definition. Some Critical Realists will object that the event-level definition has powerful consequences in the critique of orthodox economics. However, the ontology offered here offers a more nuanced concept of closure and more easily accommodates the notion of partial closure. Further research is necessary to elaborate on the basic philosophical ontology offered.

The ontology has many methodological implications. This paper bolsters existing critiques of economic methods such as deduction, abstraction and econometrics. However, it presents a range of arguments against their rejection. Furthermore, the paper suggests a more flexible

approach to economic research. One implication is the employment of methods from other disciplines. This reflects again that economics is an open system. There is of course a huge range of such possible alternative methods. Lawson has advocated explaining contrasts in data. Bhaskar (1979: 48) argues that in periods of crisis, real structures might be more clearly discernible than usual. Case study has become more popular recently (see for example Blinder, et al, 1998). Case study avoids some of the problems of closure by focusing on a small group or even individual, requiring that the homogeneity required is more likely to be achieved. However, of course, any attempts to generalise from these studies are problematic. Downward (1999) has utilised surveys on pricing; Hall and Hitch (1939) provide an historical precedent for this. Olsen's (1996) work on economic development uses various ethnographic and participant observer methods. These methods are not unproblematic, but have been employed widely in other social sciences.

However, the paper eschews unstructured eclecticism. Indeed, the guiding principle of open-systems methodology advocated here is that if economists believe in realism, they must be concerned about the disjuncture that exists between the open-systems ontology and the closed-systems methods employed to investigate it. It is argued that most economists accept the fact of open systems, but respond differently. Some 'open-systems' economists are negative about traditional methods because they are less prepared to compromise their ontology than are others, typically in the economic mainstream. However, it was also claimed that all current available methods impose closures on reality. Economists concerned about open systems (that is, most economists) should acknowledge the flaws in all methods and recognise that in some circumstances, some methods are superior to others, and in others, they are not. There is no best method. Rather, methods should be triangulated and accorded

weights consistent with the extent of their disjuncture with the reality. These arguments apply to all economists.

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[1] As a simplification, the paper will treat the terms 'orthodox', 'neo-classical' and 'mainstream' as synonyms. This is clearly questionable, but seems appropriate in the context of the paper.

[2] This ontology is explained in greater depth in Mearman (2002a, b).

[3] One objection to Bhaskar's argument is that his ontology merely reflects the privileging of science (Harding, 2003). Bhaskar (1979) tacitly acknowledges this by making his deduction anew, from any human activity whatsoever. Feminist authors arguably use such an ontology of structures in their analysis (Lawson, 2003b).

[4] Several authors have argued that in fact, orthodox economists revert to a flat ontology of events or experiences (Lawson, 1997; Fleetwood, 2002). Given that, economics becomes the search for successions of events (often under very specific conditions) amidst great complexity; but the successions are often interpreted as causal relationships (see Lawson, 2003a: 15). Therefore, orthodox economists might maintain the desire to find mechanisms in the form 'if X then Y', even if they do not expect to find these easily identifiable patterns of events. It might also be held that the mechanisms in orthodox economics are merely patterns lying at the same level of events, hidden by the complexity of other patterns. Alternatively, orthodox economists are accused of

reducing the underlying factors to atomistic elements. Obviously, if as some argue, orthodox economists reflect a positivist heritage, many might insist that the mechanisms are observable. However, this applies to many economists (see Lee, 2002).

[5] In terms of their perception, events are at least quadruple conjunctures, as systems of perception and interpretation act upon the material and phenomenal aspects of the event.

[6] However, Friedman's position is also one which is highly problematic from an open-systems perspective. That follows because prediction is *per se* problematic in open systems. If specific mechanisms are not operative all the time, and if it were not known when they would be operative, it would seem unlikely that predictions should be correct. If mechanisms change, or other mechanisms emerge from them, and if this is not itself predictable, then again the predictability of the outcomes of the systems would seem to be unpredictable. Now, of course, there is nothing to stop an economist making a prediction; indeed, policy economists must make them. It is not the case that a prediction cannot be correct. However, because of the ontological reasons given, in addition to epistemological difficulties in predicting, it remains unlikely that the prediction will be correct. Moreover, this can be the case even if the theory offered is a good one. Thus, the case for falsification of a theory based on predictive power is weak in open systems. This suggests that most economic theory should be based on the desire to explain.

[7] Mearman (2003b) extends the analysis of Siakantaris (2000) of experimental economics. Experimental economics attempts to mimic closely natural science experiments by isolating a single mechanism and by controlling its operation. Mearman argues that experimental economics is problematic in open systems, largely because of the strong assumptions it makes about the nature of the experimental situation and the subjects tested therein.

[8] Ardebili (2003) following Archer (1995) is quite correct to state that the methodology is contingent on a specific social ontology. However, it is beyond the scope of this paper to investigate this effect.

[9] It might also reflect differences in ontology. First, Austrians tend (at the risk of over-generalisation) to reject notions of social structure and instead adopt a version of methodological individualism. The individual and its structure are the driver of change and the generator of events; and it is therefore the individual that is the key to the explanatory analysis. Second, Austrians tend to ignore questions of power. In contrast, Post Keynesians often envisage society a system of positions, which have practices attached to them. Clearly, such positions could have power in some cases; in others, not.

[10] Clearly, also, there have been other developments in econometrics that might be interpreted as trying to take into account concerns about openness. Martingales, for example, take into account heterogeneity of reality; random coefficient models might capture some of the changes to mechanisms, or adjust for their non-operation; however, one must ask why a coefficient would be random.