CRITICAL REALISM IN ECONOMICS AND OPEN-SYSTEMS ONTOLOGY: A CRITIQUE

Abstract: This paper examines the treatment of ontology offered by Critical Realism. Three main criticisms are made of the Critical Realist treatment of open systems. It is argued that Critical Realism, particularly in the project in economics emanating from Cambridge, UK, tends to define systems in terms of events. This is shown to be problematic. The exemplar of a closed system provided by Critical Realism of the solar system is shown to be flawed in that it is not closed according to the closure conditions identified by Critical Realism. Second, the negativity of the definitions adopted is problematic for heterodox traditions attempting to build positive programs. The dualism of the definitions is also inconsistent with Dow's approach. This has ramifications for the coherence of Post Keynesianism. Third, the definitions tend to polarize open and closed systems and ignore the degrees of openness evident in reality. This polarization of systems leads to polarized methodology and unsustainable arguments to reject so-called closed-systems methods.

Key words: open systems, closed systems, Critical Realism, Post-Keynesianism, dualism

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I. INTRODUCTION

There has been an increasing use of the term "open systems" to describe the complex and unpredictable environment faced by economists and economic agents (Setterfield, 2000; Lawson, passim). "Open Systems" has even been advanced as a potential basis for heterodox economics (Hodgson, 1988; Dunn, 2001; see also Downward, 1999), and has arguably become a tacit assumption therein. However, Hodgson (2000) and Mearman (2002a) have argued that it is somewhat undeveloped and that, therefore, it would seem inappropriate for heterodox economics to be based squarely on the concept (cf. Dunn; Dow, 2000). Even if "open-systems methodology" remains only one of the pillars of heterodoxy (as in Downward, 1999; Lee, 2002) it still requires development. This paper deals particularly with the ontology of open systems.

The paper aims to develop the concept of "open systems" by offering a constructive critique of the increasingly influential Critical-Realist view. Critical Realism (C.R.) is of course a variegated literature, including the work of Downward (1999, 2003), Lee (2002), Finch and McMaster (2002). However, this paper holds C.R. in economics is dominated by a "Cambridge school" view developed by Tony Lawson (1997; 2003) and others, mainly those (mainly at Cambridge University) closely influenced by Lawson.[1] This view has figured prominently in the literature.

[1] Other significant members of this category are Clive Lawson (1996), Pratten (1996), Runde (1996), Lewis (1996), Ingham (1996), Siakantaris (2000), Faulkner (2002), Fleetwood (2002) and Pinkstone (2000). Setterfield (2000) also has this connection, but has arguably moved in a different direction.

Arguably, "open systems" is, along with ontological depth, one of the two most important concepts in this Cambridge school view.

This paper argues that there are three problems in this Cambridge school's treatment of open systems: 1) it is dominated by event-level definitions – which also reflects an underdeveloped concept of "system;" 2) it emphasizes negative definitions; and 3) it tends towards polarizing definitions. This is shown to be problematic in many ways: it weakens the ability of C.R. to engage in constructive work, it raises questions about the possibility of coherence for Post Keynesianism, and it leads to polarized methodological accounts on various issues. The paper proceeds in the order of these criticisms.

II. DEFINITIONS OF SYSTEMS IN TERMS OF EVENT REGULARITIES

Definitions

There is a small range of Critical-Realist definitions of closed systems. Closed systems are variously defined as being "cut off" from external influences (Collier, 1994: 128; Bhaskar, 1978: 69); "isolated" (C. Lawson, 1996); where outside factors are "neutralise[d]" (Collier: 33); and in which all disturbances are anticipated and "held at bay" (1997: 203). The net result is that one mechanism alone operates (Collier: 33), unaffected by other mechanisms (Lawson, 1997: 203).[2] The obvious example of such a scenario is the experiment. Thus Collier (249-50) and Lawson

[2] It would be clarifying to define "mechanism." Bhaskar (1978:51-2) writes, "a generative mechanism is nothing other than a way of acting of a thing. It endures, and under appropriate circumstances is exercised, as long as the properties that account for it persist".

(1999a: 216) effectively equate closed-systems methods as experimental (and open-systems as non-experimental).[3] Strictly, the focus on isolation is incorrect, since experimentation imposes requirements inside the isolated area. Archer (1998: 190) notes that even in isolated environments, the nature of humans means that the "closure" of experimentation cannot be achieved (see Mearman, 2003a). These problems are such that Bhaskar (1986: 101) claims that closed systems are "impossible" in social science.

Additional (partial) definitions of a closed system in C.R. are that relations within a system are stable (Beed & Beed, 1996: 1099); and that conditions are imposed on the individuals in the system (Bhaskar, 1978: 69). A fully closed system is where all individual and system criteria for closure are satisfied (Bhaskar, 1978: 104), which suggests both a regularity of behavior (Bhaskar, 1978: 253, n. 1) and a homogeneous – unchanging and uniform – environment (Lawson, 1997: 218); consequently, transformative action is impossible in a fully closed system (Bhaskar, 1986: 31). According to C.R., closure is achieved when specific closure conditions hold. The most significant of these are the Intrinsic Condition of Closure (I.C.C.) and the Extrinsic Condition of Closure (E.C.C). The I.C.C. requires that the object in question has a constancy or constancy of change, such that elements "inside" the system are stable enough to be identified. The E.C.C. entails that no outside forces impinge on the particular object or system, or that any external effect is constant.

[3] Here, "experimental" refers to a situation in which a scientist intervenes to isolate and control a specific mechanism. Thought experiments are not included. This is slightly unsatisfactory, because thought experiments are also important to science; and because thought experiments might also involve the isolation and control of mechanisms, albeit in thought.

Many of the Cambridge treatments also emphasize closure conditions. However, this paper argues that despite the apparently variety of definitions, the Cambridge school definition of open and closed systems has been, and is being increasingly, restricted to one. That is, closed (and hence open) systems are defined in terms of events and their regularity.

Consider the following definitions of closed systems: Closed systems can be identified when the symmetry thesis (of explanation and prediction) holds (Sayer, 1992: 130), or where there is a warrant for eduction (inference to particular instances) (Bhaskar, 1986: 30). Closure also means that there is a one-to-one relationship (isomorphism) between mechanisms and events (Lawson 1994a: 517). This implies the main definition of a closed system: a unique relationship between antecedent and consequent (Bhaskar, 1978: 53); a stability of empirical relationships (Collier, 1994); or a constant conjunction of events. This definition flows from the Humean conception that only regular successions between events – not underlying mechanisms (should they exist) – can be identified. Thus causality is conceived as merely correlation, which in turn calls for the identification of event regularities between isolated atomistic states (Rotheim, 1999: 73).

Lawson identifies closed systems as being where the formula "if event of type X occurs, then event of type Y will occur" (where X and Y can be scalars, vectors, or matrices – Lawson, 1994a: 507, n. 9). More recently, Lawson has modified this further to take into account the common practice of completely specifying the conditions under which closure holds. Thus, closed systems conform to the formula, "if X, then Y, under conditions E" (Lawson, 1989a: 63; 1995a: 15). Such event regularities could be either deterministic or probabilistic (Lawson, 1999b: 273), which in the latter case means that events will be in regular succession within some well-behaved probability distribution (Lewis and Runde, 1999: 38; Lawson, 1997: 76).[4] In this case, the closure is stochastic (Lawson, 1997: 153-4). Lawson repeats this event-level definition in his most recent work (Lawson, 2003: 5, 15, 23, 41, 103, 105, 143, 222, 306).[5]

Other instances of this event-level definition of closed systems in terms of constant conjunctions of events have appeared in the literature: Lawson (1996: 407), Pratten (1996: 439), C.Lawson (1996: 451, 459), and Lewis (1996: 487). Of course, all of those authors descend from Cambridge University. Rotheim (1998: 326, 329-331) and Lawson (1998: 359, 369) reiterate the definition. Clearly the event-level definition is not the only one offered by C.R. However, it is argued that this definition, particularly in those (mainly at Cambridge) influenced by Lawson, has begun to dominate. This is shown most clearly in the definition of an open system.

Bhaskar (1978) defines open systems as the lack of "regular" (33) or "invariable" (73) succession; no unique relationship between variables (53); or a non-invariance of empirical relationships (132). For Sayer (1992: 122), openness entails short-lived or non-existent regularities. Essentially, an open system is identified as, "Not 'if X then Y'" (Collier, 1994), or as where there are no constant conjunctions of events (Bhaskar, 1989: 16). This has been adopted by the recent literature on C.R. in economics. Therein, open systems are defined as where there are no event regularities

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[4] Unfortunately, neither Lewis and Runde nor Lawson define clearly what is meant by a well-behaved probability distribution. Both seem to define a "well-behaved" distribution as symmetrical with a mean of zero, such as the (standard) Normal. This seems to be the way in which Lawson (1989b) interprets the E.C.C. in econometrics.

[5] Lawson (2003:15) does develop his concept of closed systems. He distinguishes between closure as concomitance and closure as causal sequence. However, the closure is still identified via the event level.

(Pratten, 1996: 23, Rotheim, 1999; Lawson, 2003: 79, 82, 119, 223-4)[6] or as systems lacking sharp (i.e., precise) stable event regularities (Lewis and Runde, 1999: 38).

Critique

It was argued above that the Cambridge school of C.R. in economics mainly defines open/closed systems in terms of event regularities. Before showing how this is problematic, one other point should be made. Pratten (1996: 426) writes, "In critical realist contributions such regularities are referred to as closures." Further, Pratten (431) criticizes neo-Ricardian economics for its use of "givens" as closures, where such givens are dependent on regularities. Rotheim (1998: 331) also claims that closures are constant conjunctions of events (see also Lawson, 2003: 41). However, this can only be correct under that highly specific definition, but not under the other Critical-Realist definitions of closed systems above. Under these definitions, it cannot be correct to define closed systems as event regularities. For, an event regularity is not equivalent to closure; it is suggestive of closure. As evidenced by the ICC and ECC, closure occurs beneath the level of events. The action of the mechanisms under particular circumstances, perhaps in effective isolation, is the closure, which creates the event regularity. The claim conflates the empirical with the real: this is known as empirical realism, a flattening of ontology. It also suggests that the epistemic fallacy has been committed: what exists is reduced to what is known. It also suggests actualism, defined as the denial of the existence of underlying mechanisms and

[6] In fact, Lawson (2003) spends little time defining open systems. However, he does also define them in terms of the presence of multiple causes or conditions (42, 56, 125, 229, 233) or where reality is highly internally related (229).

acknowledges only actual events or experiences (Collier, 1994: 7). Empirical realism, the epistemic fallacy and actualism are all explicitly denied and rejected by C.R. These contradictions arise here because of the event-level definition of closed systems.

However, it might be argued that the classification of systems in terms of event regularities merely reflects the Critical-Realist logic of retroduction, which begins at the level of events and moves downward (in ontological terms) to the level of the real, generative mechanisms. The centerpiece and genesis of C.R. is the transcendental deduction of the stratified, open, nature of the world from the experimental activity of scientists (Bhaskar, 1978). The actual activity of experiment in natural science (and the relative failure of experimentation in social science) shows the generality of openness and effectively allows C.R. to presume the presence of open systems. This holds unless there is evidence to the contrary, namely event regularities, however local or brief. Thus, it might be expected for C.R. to focus on the level of events. Moreover, Bhaskar (1979: 138) maintains that C.R. is the only philosophy of science that takes constant conjunctions of events as neither necessary nor sufficient for explanation in natural and social science. In economics, Lawson and others have argued that the discipline is dominated by methods which unjustifiably presuppose the existence of event regularities; they have identified the search for event regularities as the sine qua non of orthodox economics.[7] Econometrics is the best example (Lawson, 1989b, 1997, ch. 7; Pratten, forthcoming). The event-level

[7] Viskovatoff (1998) questions Lawson's characterization, arguing that there are (at least) two methodologies in orthodox economics: one, he claims, pays no attention to empirical outcomes. Lawson (1998) disagreed. Mearman (2003a, 2003b) argues that the language of some orthodox economists, such as Sutton (2000), Morgan (2002) and others, implies a concern for mechanisms.

definition has high rhetorical value. Therefore, it is to be expected – and it is consistent with the aims of the Critical-Realist project – that the Cambridge group emphasizes the event-level definition.

Furthermore, it might be argued that the existence of event-regularities is key; that the use of the term "closed system" is incidental. However, if the use is incidental, why does it occur? Lawson (1989b, n. 11) claims that the concept owes much to the General Systems Theory (G.S.T.) of von Bertalanffy (1950). However, it is clear that G.S.T. and C.R. differ greatly: for example, the former stresses the existence of entropy as defining a closed system, while the latter makes virtually no reference to entropy. Presumably, then, the influence of G.S.T. is by analogy; but if C.R.'s definition of closure is contrary to G.S.T.'s, is the analogy rendered inappropriate? Clearly, analogy does not require identity: the analogy would have no work to do; however a contradiction would seem problematic. For example, entropy (and closed systems) is associated with disorder and presumably a messy event level, whereas, for C.R. the closed system is defined in terms of event regularity. This does not make C.R. incorrect, but it does raise the issue of the basis on which C.R. claims ownership of the term "closed system."

Furthermore, neither of these arguments in defense of C.R. (if they stand) would justify the apparent ignorance of the level of the real in the definition of the open/closed system. Critical-Realist methodology is two-sided. Certainly, in Critical-Realist practice, phenomena, be they crises, localized event regularities, or rough and ready patterns of events (so-called "demi-regularities"), etc., are usually the starting point in investigation. From the empirical level, real mechanisms are retroduced. However, this is followed by the empirical assessment of these hypothesized mechanisms. This entails that the empirical is reconstructed from the real. Thus, some attention must be paid to the real level, to determine how those mechanisms produce the empirical. This requires extensive consideration of the workings of the system in question; i.e., its real causal mechanisms set in structures; and its structure, boundaries, etc., to illuminate exactly how the empirical is generated. Thus, there is a need for a more positive definition of the open system, which includes both the real level and the event level. This argument is complementary to Brown, Slater and Spencer's (2003) argument that Critical-Realist abstraction is weak in terms of reconstituting the concrete.

Mearman (2002a, b) argues that this problem stems from the use of the term "system" in the Cambridge school of C.R, which effectively ignores the two-part nature of the term: specifically, an open system is a <u>system</u> that is <u>open</u>. More broadly C.R.'s notion of the system is relatively underdeveloped. Indeed, Bhaskar (1978: 73) claims that system "carries no independent semantic force."[8] This seems to prove the point. Moreover, there is no clear picture of the term in C.R.[9] At times it refers to mechanisms, or the "structures" wherein they reside. In a sense this is accurate, since a closed system is one in which only one mechanism (that in question) operates. Similarly, an open system is where a mechanism operates but is open and subject to other mechanisms. However, this ignores the fact that the mechanism is at the real level, but generates events at the empirical level. Both of these levels are part of the system, thus, just as the system cannot be reduced to the events, nor can it be reduced to the mechanism. Moreover, when Lawson (1999c: 5) writes, "The aim [of

[8] Lawson (2003:15-6) also downplays the significance of the term "system," claiming that the definition of the system is merely dependent on the area or time over which an event regularity can be found.

[9] Other economists drawing on C.R., such as Downward (1999), Lee (2002), Brown, et al (2003) and Mearman (2002b) are trying to address this apparent lacuna.

experiment] is to engineer a system in which the actions of any mechanism being investigated are more readily identifiable," he differentiates clearly between a system and a mechanism. Lawson (1994b: 279) identifies system and structure separately, further weakening the equality. In short, the notion of system seems underdeveloped and this makes constructive research and the reconstitution of the real more difficult.[10]

The problem of the event-level definition can be illustrated by reference to the claim, common in C.R., that the solar system represents a rare example of a closed system found outside experimental control (Bhaskar, 1978: 65; Lawson, 1996: 407, 411; Runde, 1996: 472-3; Lawson, 1999c: 4; Pratten, 1996: 23). This is because it exhibits, or at least approaches, complete event regularity. However, if one of the criteria for closure is, as stated above, that the symmetry thesis holds, the solar system cannot be a priori a completely closed system, since there is the possibility that an unpredictable asteroid could disrupt planetary motion. Runde (474) recognizes this: "Of course, even the regular movements of the planets is itself contingent on the planetary system remaining undisturbed (and by most accounts, eventually, the system will be disturbed). But it is a system that, relative to our own life histories, changes so slowly as to be imperceptible." It is commendable that Runde considers

[10] Lawson (1989a:71) does offer a definition of a "system," in terms of a combination of structures. Second, he defines a totality and differentiates between it and a system: arguably in terms of the extent of internal relationality between structures. Furthermore, the dialectical turn of CR has produced the concept of "totality," which is a system of internal relations (Bhaskar, 1993:405), with an intensive and extensive margin (125). Clearly this captures better the notion of "system," except that the totality seems to be completely internally related, whereas the system includes externally related elements. Bhaskar (95-6, 127, 269, 273; 357) distinguishes between open and closed totalities but these are distinct from the concept of open (and closed systems) used, which remains: "Systems where constant conjunctions of events do not occur" (401).

the issue; however, his claim about imperceptible change is inconsistent with C.R. To claim that because we perceive the universe as stable, it must be so, reduces existence to knowledge and commits the epistemic fallacy. Furthermore, Runde classifies the system according to what has happened, rather than what might potentially happen. This is somewhat inevitable, because we rely on ex post descriptions; but his is a description of the events only and ignores the potentialities within the objects of the system. This suggests actualism.

Moreover, Runde has shown that the solar system is not closed: it is at least potentially subject to disturbances. Thus, the E.C.C. does not hold. Yet the E.C.C. is held to be necessary for a regularity and hence a closed system. Only by defining the system in terms of past events does the E.C.C. hold. Moreover, the I.C.C. does not hold either. Specifically, an assumption of the I.C.C. in the solar system would require the underlying constancy or constant rate of change of the entire system and indeed the universe. However, such an assumption would be bold: Monastersky (2002) claims that proponents of the so-called "inflation" and "M" theories of physics agree that the rate of expansion is unknown. Furthermore, Collier (1994: 244) admits that cosmology studies changing entities – new tendencies emerge as the structure of the cosmos changes. For example, he notes, "Big Bang" theories postulate differences mechanisms operating immediately after that event. Of course, assuming a constant rate of expansion of the universe seems reasonable; however, it entails an assumption that is known to be quite possibly at odds with reality. When others do this, C.R. accuses them of instrumentalism (Lawson, 1989b). The key point is this: in the solar system, neither the E.C.C. nor the I.C.C. seem to be satisfied; these conditions are necessary for a closed system; yet C.R. claims that the solar system is a closed system because of the regular actual recorded movements of the planets. The claim appears to be actualist, contrary to depth realism. Clearly, the event-level definition of systems is limited and limiting.

Finally, it should be noted that Lawson (1995b: 267) leaves open the possibility of local closures, even if by "chance," i.e., even if there was no prior basis to believe that regularity determinism or stochasticism held (see also Lawson, 2003: 15). This is significant. If closures are defined as "if X, then Y," by chance, then a system can be called closed, without knowing anything about that system, merely its outcomes. Moreover, as a general case, it is possible for two external forces, acting on the mechanism inside a system, to exactly cancel out each other. If the mechanism inside the system were constant, the outcome would be to produce a regularity. Yet the system is clearly open (in a broader sense) because of the impact of the external mechanisms. Thus, again, "if X then not Y" seems not the best way to define an open system.

III. NEGATIVE DEFINITIONS OF OPEN SYSTEMS

Several of the definitions of open systems offered above were of the form "if X, then not Y" or variations on that. Here, these are called negative definitions because they stress an absence of a condition, specifically of a regularity. A positive definition would be classed as one that stresses the presence of some particular. In addition to the negative definitions, presented above, open systems have been defined in C.R. as where closure conditions fail to hold (Downward, 1999: 17), or that internal and external parameters are nonconstant (Sayer, 1981: 138). In partial defense of the Cambridge school within C.R., other definitions of openness are similar. Kaldor (1972) suggests openness, in his concern that, contra orthodox models, constraints (for instance on consumers) would not be binding (see Hahn, 1989: 55); and also that changes in organization, for instance, create possibilities for further change (see Hahn, 1989: 49). Grunberg (1978: 542) equates openness with a lack of constants (and with complexity) and therefore with the inability "to ascertain invariant relationships." Keynes (1973: 262-3) conducts thought experiments on the effect of money wage reductions in 'closed' and then 'unclosed' systems, which are national systems affected by foreign economic factors.

Dow (passim) defines an open system, again, effectively as "not closed," as does Downward (1999). Dow offers essentially the reverse of her definition of the closed system. Thus, in an open system, not all constituent variables are known, structural relations are not all known or knowable, and traditional logic is not applicable (Dow, 1996: 14). Relatedly, Olsen (2000), writing economics informed by C.R., seems to define an open system as being incomplete, or not fully specified by the theorist. This mirrors Setterfield (2000), but also some orthodox definitions, which define a closed system as complete, where all variables are modeled Hendry (1995: 310).

In fairness, C.R. does offer "positive" definitions. For example, Collier writes, "In open systems…a multiplicity of mechanisms is operating, conjointly bringing about a series of events, which would not have been brought about by any proper subset of those mechanisms" (1994: 43-4). Thus, outcomes are complexly co-determined (Collier: 62) by a "plurality and a multiplicity of causes" (Bhaskar, 1978: 72). Therefore the same mechanism can lead to different outcomes (i.e., rather than "if X, then Y," the result would be "if X, then any one of $Y_1,\ldots,Y_n$,"); and an outcome can be produced by a number of mechanisms.

Interestingly, several authors (Dow, 1996; Setterfield, 2000; Lewis and Runde, 1999) have argued that openness of a social system can be identified by the existence of real choices for individuals (in the sense that the outcome of the choice is not predetermined).[11] Bhaskar (1979: 114) defines this as where "the agent's activity makes a difference to the state of affairs that would (normally) otherwise have prevailed." Significantly, also, Lawson (1995b: 265) argues, "social structure is human agent-dependent: it is only ever manifest in human activity. Thus, given the open nature of human action, the fact that any agent could always have acted otherwise, it follows that social structure can only ever be present in an open system." Clearly, social structure would disappear without humans – although, it is not created by the specific humans present at that time (Archer, 1995) – but to assume this, it is necessary that humans have choice.

Dow (1996) defines an open system, in addition to the criteria above, as one with fuzzy or indeterminate boundaries. This is a departure from the C.R. definition: even though the E.C.C. might imply a boundary, it says nothing explicit about it: indeed, the notion of the system boundary is essentially absent in C.R. Dow also notes that an open system is identifiable by imperfect ordering, i.e., with a degree of disorder (Dow: 14). This is opposite to the G.S.T. concept of closed-systems being associated with randomness (in the usual sense). Disorder is perhaps only present in C.R. in that a non-invariant empirical relationship might imply disorder.

[11] This contrasts to orthodox models of the consumer. Given preferences, prices, income, the assumed rationality of the consumer, and the assumed goal of utility maximisation, the rational agent in orthodox economics has no real choice: there is only one possible outcome for the consumer; moreover, they are assumed to be unable to change any of the variables relevant to their decision (DeUriarte, 1989-90).

One final definition to consider is Kapp's (1968), which, in one way, corresponds exactly with an aspect of the G.S.T. definition: that an open system is one that receives (and survives on) impulses from outside. It is difficult to identify this aspect in the Critical-Realist definition since there, a) "system" is not well defined, and b) the spatial aspect is de-emphasized (Bhaskar, 1978: 76-7). Another aspect of Kapp's definition is that in open systems there is an interaction between sub-systems. This is perhaps akin to the common notion that larger systems (sometimes considered open, in other work, closed) do comprise sub-systems. Again, for the same reasons as stated immediately above, it is difficult to conceptualize this notion in C.R.

In spite of the large apparent variety of definitions presented, it is argued that in economics, the definitions that dominate the discourse of open systems are negative. This is particularly the case with regard to the 'Cambridge school' of C.R. in economics. To reaffirm this, it should be noted that Lawson (2003), while offering definitions of open systems in terms of, for instance, multiple mechanisms (above), he also defines open systems in terms of unpredictability (100), unsusceptibility to closure (62), lack of event regularities (see above), and the impossibility of experiments (84). Moreover, most of the positive, i.e., not simply the opposite of a closed system, definitions discussed above are from outside C.R. They also achieve more than the negative Critical-Realist definitions above, which remain basically at the level of events, by discussing the domain of the real, specifically the nature of the structures to be found there.

It has been argued that the dominant 'Cambridge view' definitions of open systems are negative. The key issue is whether this is problematic. Arguably it is the nature of argument that concepts develop in this way. Clearly, it is common to define an unfamiliar object in terms of the familiar. Moreover, in the development of C.R.

called Dialectical C.R. (after Bhaskar, 1993), a key concept is that absences can be causal and that their existence is significant. However, this paper finds the negative definitions to be problematic for two main reasons. First is the issue of the nature of heterodoxy. It is the nature – indeed, the literal definition – of heterodoxy that it opposes the current orthodoxy. However, the danger for heterodoxy is that its would-be critics often reduce its definition to being "not orthodoxy" or "not neo-classicism" (cf. Walters & Young, 1997; Lawson, 1994a). Clearly, heterodox economics is more than this: Marx, Veblen and Keynes (for example) were all involved in criticizing the orthodoxy, but they also offered criticisms of contemporary society, and (all perhaps except Veblen) offered a constructive alternative program. Nonetheless, the negativist perception persists and is damaging to the heterodoxy.[12]

The second problem relates to the substantive consequences of the negative definitions. The mode of construction and development of the definition are significant. Often a "dualist" process occurs. From Dow (1996: 16-17), dualism is "…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." The unfamiliar is defined in terms of the familiar by placing it in opposition to it. Often the similarities between the two are ignored. For example, one might define irrationality in terms of rationality, missing intermediate concepts. Indeed, as Mearman (2003c) argues, a central point of Dow's (1990, 1996) work is that such dualism leads to errors. The argument here is that the standard realist definition of openness tends to dualism, i.e., it has an unfamiliar concept, "open

[12] One of the principal tasks of heterodox economists and organisations is to develop coherence and/or develop arguments against the necessity of coherence. This remains, in spite of the clear fragmentation of the orthodoxy (cf. Viskovatoff, 1998; Caldwell, 1982).

systems," and a familiar (via orthodox economic and scientific practice) concept, "closed systems."

Hence, definitions of open systems tend to begin with definitions of closed systems. Mearman (2003c) shows that even when the concepts are poles, i.e., defined in terms of each other, as in the case of open and closed systems, the relationship between the two terms can be severed and two strictly distinct categories can emerge. It is argued below that this sometimes occurs in Critical-Realist argument regarding open systems. There are two general and serious consequences for C.R. First, Mearman (2003a, c) argues, following Dow, that the conditions for dualism are usually not met in open systems. Dualism requires atomism, certainty and closure for its mutually exclusive, exhaustive and fixed categories: none of these are usually to be found in open systems. Thus, by adopting a dualist definition of open systems, open-systems proponents are engaged in closed-systems thinking. Of course, dualism is not incorrect *per se*: a door might be open or closed; however, it could also be 'ajar'. Crucially, though, it is held that very often dualistic categories are either incorrect or eliminate useful possible categories. Second, therefore, there is a conflict between Dow's position and C.R. This has implications for the goal of coherence for Post Keynesianism (cf. Mearman, 2001a; Downward, 1999; Dow, 1999). This reinforces the problem of negativity identified above. Thus, for these two reasons, more positive definitions of open systems are needed. From section II, these positive definitions should not be restricted to the event level.

IV. POLARITY OF OPEN/CLOSED

The argument of section III implies a conflict between Dow's position and C.R. on the question of a dualist categorization of open and closed systems. However, this is only strictly the case if the categorization is invalid in reality: some dualistic categories might in fact be correct; however, for Dow, this is either unlikely, and/or there are not epistemological grounds for arriving at those certain categories. So, the immediate question is whether open/closed systems should be treated as strictly separate. For, often in C.R., it seems as if a system is not completely closed, then it is inescapably open, rendering closed-systems methods totally impotent. For example, Lawson (1999d) insists that econometrics is only valid in strictly closed systems. Thus, there is a need to investigate two points on two different levels. It needs to be established whether C.R. is unjustifiably dualist in its treatment of the distinction of open-systems/closed-systems (1) ontologically and (2) methodologically. If this is so on either count, this is problematic, for the reasons given earlier. If the treatment is dualist on one count but not on the other, then this is a disjuncture between the two, which seems problematic for a realist perspective.

Ontology

Open/closed systems might appear to be a clear dual, given that the two concepts are defined most often simply as opposites of each other. This can be investigated further by examining the polar extremes, perfect openness and perfect closure. Recently, the polar view has been suggested, by for example, Rotheim (1999: 75). Lawson (1994b: 276) suggests "two extremes – strict event regularities or a completely non-systematic flux – merely constitut[ing] the polar extremes of a potential continuum." Later (277) he proposes "a continuum of outcomes…ranging from closed systems of constant conjunctions of events to an inchoate random flux." Consistent with section II, the definition is in terms of events. Arguably, it is unlikely that either extreme actually exists in reality. First, Collier (1994: 33) claims, "no system in our universe is ever perfectly closed." Above, it was shown that even the solar system is not closed. Therefore, the prime example of naturally occurring closure given by C.R. is invalid. Lawson (1997: 203) demurs somewhat, claiming, "the goal of perfect closure … cannot always adequately be engineered; indeed it may very rarely be."

At the other end of the spectrum, Critical-Realist authors have clarified that openness refers neither to a complete arbitrariness of events Rotheim (1999: 75) nor to an inchoate flux (see Lawson, 1994b: 276). Indeed, Cottrell (1998) criticizes C.R., in that significant regularities are in fact found in the social world. An example might be that one works and then gets paid. Lawson (1998) replies that, indeed, people go to work and are paid after working; but they can go home and perform the same activity for no pay. Therefore, the strict regularity "if work, then get paid" fails to hold.[13] This is not to deny that much of the time "if work, then get paid" does hold, just not always and everywhere. Other reasons might be that people work voluntarily, or that some crisis occurs which prevents payment (Argentinean public service workers and Iraqi soldiers are recent examples). In reality, this is the openness that Lawson (et al) discusses, not inchoate flux. Again, all these arguments are presented in terms of event regularities, supporting the argument of section II.

Practically, there is no prospect that either perfect openness or perfect closure exists. Between the two theoretical extremes lies everything of practical interest.

[13] Clearly this assumes a broader definition of work than, for example, "leave the home to work on another's property." This is contentious but consistent with the Critical-Realist treatment of "work" (Bhaskar, 1978:194-5).

Nevertheless, the language of C.R. focuses on the extreme cases. C.R. accuses orthodoxy of clinging to methods based on the perfect (non-existent) closed system. However, C.R. also uses the perfect (but unachievable) closed system in order to construct its alternative. The contrast between on the one hand astronomy, and on the other, every other discipline, serves a rhetorical purpose and has rhetorical value. Rather than envisaging the spectrum of open-closed systems as a continuum, bounded by theoretical if not practically attainable extremes, C.R. treatments tend to begin with the notion of a (perfectly) closed system and look for instances whereby the event regularity fails to hold. Where this is the case, the system is classified "open." However, there is clearly a difference between a system in which there exists a mechanism that occasionally operates, whereas the system is otherwise stable; and one in which there is a chaotic mess of sporadically active mechanisms, continually combining in novel ways. In the former case, there remains a good chance of developing knowledge, whereas in the latter, that chance seems remote. However, both would be called "open" and both would fail to exhibit event regularity. Again, the discussion is in terms of events (section II) and negativity (section III).

In fairness, at other times, Critical-Realist treatments have acknowledged the existence of such "partial closure." Partial closure can have a variety of meanings. Sayer (1992: 124) defines quasi-closed systems as "producing regularities that are only approximate and spatially and temporally restricted." The definition suggests two types of partial closure. One is defined in terms of spatial or temporal specificity. This definition suggests a large, open mass segmented into smaller closed systems. This is close to the Critical-Realist notion of "local closures." This is a sense in which C.R. often speaks about partial closure. Bhaskar (1978: 78) argues, "for experimental science to be possible the world must be open but susceptible to regional closures."

Similarly, "A closure is of course always relative to a particular set of events and a particular region of space and period of time" (Bhaskar: 73). This corresponds with Sayer (1992) above. This notion of historically and spatially specific closures is consistent in C.R., from Bhaskar (Bhaskar, 1978; 1979: 128; 1986: 27) and developed by, for example, Lawson in two of his most important contributions. First, with regard to econometrics Lawson says it is legitimate to investigate whether, "in certain conditions some closed-systems methods or whatever could contribute to enlightenment" (Lawson, 1999d: 8) (emphasis added). The conditions in question suggest local closures.

Lawson's second contribution of import, the identification of the usefulness of rough and ready patterns, called demi-regularities, relies on the premise that local closures are possible (see Lawson, 1997: Ch. 15). As Lawson (219) notes, demi-regularities are "a special situation of the open world [in which] certain mechanisms (whether natural or social) reveal themselves in rough and ready patterns…[but] it is a special case of this special situation that the patterns produced correspond to strict event regularities…" (emphasis in original).[14] Clearly demi-regularities are the result of a form of partial closure. Indeed, they possess two senses of partial closure. The latter case seems to suggest a complete closure in a specific space-time position as discussed above, whilst the former suggests an incomplete closure. Therefore, the concept of demi-regularity suggests a meaning of closure as meaning "closed to an extent." For example, Bhaskar, (1989: 185) claims that biology deals with *quasi-*

[14] Lawson (2003:105-6) clarifies his concept of demi-regularity. It can apply to any rough and ready pattern, even when the pattern involves a deviation from an expected regularity.

closed systems.[15] An example given is the study of the life cycle of an organism; however, the reasoning is not explicit. This definition of partial closure suggests, therefore, that partially-closed systems are ones in which the event regularity is apparent yet not strict. Such a partially-closed system might have merely evolved; or the system has had closure introduced, making it more closed than before. One such source of greater closure might be an institutional feature, such as a rule, habit, custom, or convention. Indeed, Sayer (1992: 124) writes, "Many forms of social organization tend to approximate regularities in patterns of events by enforcing rules…" This captures the concept of partial closure in the first sense very well. Lawson (1993: 175) suggests this sense when claiming that when people fall back on conventions, this creates "a significant degree of structural stability" even under uncertainty. Again, it should be noted that these claims can only be made by going beneath the level of events.

What should be clear immediately is that this nuanced approach does not justify any simple strict dualistic treatment of the ontology of open systems. It would seem that any Critical-Realist treatments which hold that once perfect closure is impossible, distinctions between the different open systems available is lost, would seem to be invalid. Moreover, it would seem that Lawson in particular has understood this. This is particularly in the light of his emphasis given to the concept of demi-regularities. Demi-regularities, it is clear, lie somewhere in between the closed system of the experiment and the chaos of a perfectly open system. Thus, there is a role for a concept of partial closure. It is also clear that it is useful to think of systems as lying at

[15] Bhaskar (1978:253, n. 1) makes the same point: "…it is clear that some systems, such as biological ones, are more nearly closed (reveal a greater degree of regularity of behaviour, or recurrence of syndromes) than others…" Again the definition is in terms of event regularities.

some point on a continuum. This suggests, in turn, that there are degrees of openness of systems. Thus, once we move from the perfection of the experimental closure, there are a vast number of slightly different points at which we can stop. Moreover, if the perfect closure is viewed as impossible, then the difference between possible closures and demi-regularities seems to be much smaller. Thus, Cottrell's (1998: 352) argument that Lawson overstates his contrast between orthodox event regularities and his own demi-regularities does seem to be valid: there appears to be a difference in degree rather than in kind between them.

Methodology

The most powerful contribution of the Critical-Realist project in economics has been to demonstrate the importance of ontology and to reorient economics such that the clear disjuncture which exists between reality and the methods employed to investigate it should be at least reduced. Thus, methodologies that presuppose strict closure should not be relied upon to understand extremely open environments. This seems reasonable. However, it is argued that the Cambridge school of C.R. in economics has tended to adopt a strategy of rejection of "closed-systems" methods. There are several arguments against this (see Mearman, 2003a) but one in particular is implied by the above argument. If systems are open to differing degrees, then it is likely that methods are too; and therefore the key is to fit methods to situations. Moreover, just as it unjustified to conflate systems that are not completely closed into a single category of "open-systems," it is unjustified to conflate methods that presuppose some degree of closure into a category of "closed-systems methods" and to reject these. The components of this argument will now be examined.

The first component is that the Cambridge view entails a strategy of rejection. Two examples are employed to make this case. Pratten (1996) provides one. He argues that ultimately, neo-Ricardian economists are faced with the choice of abandoning many of their methods or face being rejected by Post Keynesians (439). His argument is that although they wish to do realist, open-systems work, neo-Ricardians are trapped into thinking in terms of closure (435, 437). These conclusions are based on his argument that neo-Ricardians engage in closure as a "first step" in analysis (431-2). This follows from such features of neo-Ricardian analysis as the use of "given" values of certain factors. Pratten is correct to argue that there are certain assumptions in the neo-Ricardian analysis that are questionable from the perspective of open systems. The assumption of a pre-determined long period would be one example.

Lawson's (1997) treatment of econometrics provides the other example of rejection. Lawson argues that regression analysis is based on the unwarranted assumptions of underlying homogeneity and of being able to exclude factors not selected for the analysis. His claim is valid. This leads Lawson to argue that a) econometrics should be restricted to conditions under which there is complete closure; b) econometrics should be redefined in terms of descriptive statistics; and c) other methods should be employed which are not guilty of closure. One such example is his notion of "contrast explanation" (see Lawson 1997, 2003).

The objections to both arguments are the same. In both cases, it seems to be argued that because the techniques and theories in question seem to involve some closure, but the reality is open, the techniques are automatically invalid. This is problematic for several reasons. First, these arguments are effectively collapsing together techniques and effectively ignoring differences between them. Neo-Ricardian analysis does indeed involve some imposed closure; however, this is significantly less than in neo-classical analysis. This ignores the greater realism in neo-Ricardian analysis. Moreover, Trigg (2003) argues that in neo-Ricardian analysis, methods are employed so as to suit circumstances and that this is evidence of an awareness of openness. One charge against Pratten here is that he ignores the way in which the closures, such as they are, have been introduced. In short he ignores what Mearman (2002a) has termed "process openness," under which the model or technique should not be judged merely on its form but on the method of its creation.

With respect to Lawson, clearly, there are many different types of econometrics, yet they are conflated and rejected except in highly specific circumstances. However, as Downward, Finch and Ramsey (2002), Downward and Mearman (2002) and Mearman (2003a) –responses derived from C.R. but reacting against the strategy of rejection entailed by the Cambridge view – have argued, there are elements of closure in all methods, including contrast explanation, such as the introduction of closure (I.C.C.) necessary to envisage an entity as relatively enduring (Downward, 1999), or the assumption of qualitative invariance involved in quantification. Furthermore, to analyze open systems, strategies must be developed, which inevitably amount to partially closing, in either sense, in thought, an open system. Dow (1996: 14) claims "an open system can be segmented into sub-systems which can be approximated to closed systems for partial analysis, but which are always open organically to influences from other parts of the overall system." Setterfield (2000) adopts the same tack, citing Kregel's (1976) stratagem of "locking up elements without ignoring them." Setterfield describes this as a "conditional closure." An obvious example of these techniques is that a Critical-Realist abstraction necessarily involves a focus on what is real and essential (Lawson, 1989a) to the temporary exclusion of other factors, regarded as transient or insignificant. However, these influences cannot be legitimately completely excluded, as they might interfere at any point with the operation of the real and essential (Mearman, 2001b).

This shows that although there might well be greater openness in some techniques, they are not free from closure. Thus, Lawson cannot claim to have open-systems techniques and Pratten cannot claim to avoid any first steps of closure in substantive analysis. Moreover, given that there are degrees of openness of reality, there are circumstances under which some more "open" techniques are less suitable than more "closed" variants. By treating all types of econometrics as the same, this ignores the fact that non- or semi-parametric techniques, for example, involve less closure than parametric techniques (Finch and McMaster, 2002). Thus, as Mearman (2003a) argues, a strategy of a priori rejection on the basis of openness is unsustainable.

CONCLUSIONS

This paper has examined the influential treatment of "open systems" by Critical Realism, which has been prominent in recent economic literature. This paper has argued that a Cambridge view of C.R. in economics is identifiable and distinctive. It is argued that this Cambridge view has three problems in its treatment of open systems: 1) it is dominated by event-level definitions – which also reflects an underdeveloped concept of "system;" 2) it emphasizes negative definitions; and 3) it tends towards polarizing definitions. These problems create difficulties in trying to develop methodology and substantive work informed by Critical Realism. The event-level definition is certainly effective for criticizing orthodox economics and for focusing discussion onto ontology. The event-level definition might also act as a rough guide for identifying possible closed systems. However, it hinders analysis by ignoring the nature of the system. It is also shown to be an imperfect guide to openness. Possibly the greatest problem with the Critical-Realist treatment is its polarizing treatments of existing methods. This mode of argumentation is clearly intended to criticize the orthodoxy. However, it has been shown a number of times that this argument is unsustainable. What is needed for the project informed by Critical Realism is the construction of a positive, nuanced treatment of systems. This will also involve a more complete definition of system, which moves beyond simply classifying systems by their event patterns. Such a definition will likely incorporate treatments of systems from other literature (Mearman, 2002a, b).

Word count (Text including notes; excluding title page and references): 7931

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