INTERROGATIONS #2
9/16/2004
General Perspectives II: Critical Realism

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1. Fabian Pfeffer

While I feel that critical realism generally fits sociological reasoning quite nicely, I understand from R. Pawson that its application rules out most forms of 'variable sociology'. Is this correct; and if yes, is this necessarily so? Where else will we draw empirical evidence from?

As a (future) Sociologist, I feel relatively comfortable with the central claims of critical realism. They seem to nicely parallel common sociological reasoning and theory constructing. What else is sociological investigation if not 'digging deeper', searching for underlying mechanisms and structures. The idea of 'the stratified reality' and the call to search the *explanans* on a lower level in order to elucidate the *explanandum* on a higher level are merely other words for what e.g. Coleman advocates by his macro-micro-models. (Besides: the opposite direction of explanation is also put forward). Anti-reductionist defenses of the independent existence of each 'layer of reality' also seem to be a sociologist's motto. (I will leave the issue of experimentation and closed systems untouched here; even I expect this to be a crucial obstacle for the application of critical realism to social sciences).

How sad on the other hand, to hear that the complexity of mechanisms at work cannot be traced by standard quantitative methods used in social scientific investigations. The mechanisms under discussion are "generative mechanisms [which are] not the same thing as a spurious cause or an intervening variable … A mechanism is not thus a single variable but an account of the constitution and behavior of those things that are responsible for the manifest regularity." (Pawson: 130) How then can we discover the 'underlying forces' if not by searching for a lower-level variable? The solution proposed is to "isolate closed systems experimentally to see if they correspond to the *conceptual systems* we have modeled theoretically" (Pawson: 154). Let us suppose now, that we have elaborated a thorough theoretical concept, found single (other-level) variables which capture our concepts adequately, mapped these variables to the concept and finally examined the correspondence of the model with empirical reality by statistical analysis (our alternative to experiments). What is so wrong about it? What if we were so lucky (or so good) to match the 'independently existing mechanisms'? Could the thoughtful use of variable-work be a kind of twisted 'transcendental arguing'? If not, I have difficulties in seeing what else would count as empirical evidence. Neither Bhaskar's reference to theoretical models in order to answer this question nor Pawson's idea of network models "entrenching new models into the existing system" (isn't this just 'paradigmatism'?) convince me.

[I don't think that the implication of Pawson's account of realism is that variables – in the straightforward sense of quantitative measurement – no longer has any place in social science. The associations among variables will always provide the raw material for the elaboration of the theoretical models of generative mechanism that explain how those associations are generated. In one of the examples given by Pawson, the quantitative relationship between variations in temperature and pressure in gas are crucial for any account of the relevant mechanisms. What the mechanisms in fact do is explain this

quantitative association. So, the research program requires postulating such underlying mechanisms. The question then is how do we validate such claims? Pawson, like many realists, invoke the experimental strategy as the best way of doing this. Statistical research of a properly designed sort can be viewed as a good second-best strategy in the context of open-systems where experiments are impossible. The notions of "controls" in a statistical model attempt to simulate an experiment. Of course this is always only partially successful, but I don't think it has a logically different status from an experiment.]

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2. GOKCEN COSKUNER

1. Bhaskar asserts that the concept of activity dependence of social structures enable social scientific work, rather than limit it. He argues that the hermeneutic dimension of social life is the necessary starting point for social science and that the self-referential character of sociology encourages a beneficial methodological reflexivity, which is less evident in the natural sciences. (Philosophy of Science, p. 134-135) **How does being self-referential work in advantage of social scientific work? [I think the idea here is that it makes social scientists more acutely aware of how their relationship to the objects of study affects the observations they make of those objects. Our observations are always a form of intervention in which we are at least partially constituting the things we observe. The reflexive aspect of sociology makes sociologists aware of this to a greater extent, perhaps, than natural scientists.]**

2. Benton and Craig provide a hierarchy of level for sciences. (philosophy of science, p 126) The way of the ordering of the sciences are justified in terms of the way the mechanisms characteristic of each level are explicable in terms of those of the next one below it. And according to realists reality is stratified where at the bottom level is the empirical level of observed events and at the top is the real world mechanisms, powers etc. which science seeks to discover. **So from a realist point of view is there a relation between the two stratifications? [Interesting question. I think a key issue here is the problem of reductionism. If each science were reducible to the science at a "lower"/deeper level, then these two stratifications might collapse into one. I think the antireductionist argument means that each science retains its own specificity, and therefore each involves a set of specific explanatory problems of mechanisms/events/experiences.]**

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3. Matt Dimick
Realism, Anti-Realism, and Critical Realism: What's At Stake?

What's at stake between the rival views of realism, anti-realism, and critical realism? As between realism and anti-realism, one of the debates would seem to be whether the goal of science is to provide a "true" picture of the world. For realists the answer is "yes," for anti-realists, the answer is "no," the goal of science should be to provide *useful* knowledge of the world, whether that knowledge contains parts or elements (e.g., unobservable entities) which are later shown to be false. If the debate is kept within these terms, I'd have to side with the anti-realists, since whether we can know if our knowledge really is *true* seems to be purely a philosophical point, the issue having no bearing beyond that. On this account, critical realism would seem to be a kind of anti-realism, since it claims that knowledge is *fallible*, that "claims

are always open to refutation by further information" and because one of the goals of science should be pragmatic: to assist in emancipatory projects. **[I think you are conflating the epistemological issues and the ontological issues here. Critical realists believe that knowledge is fallible – which is an epistemological position – but the knowledge is still knowledge about real mechanisms that exist independently of our knowledge – which is an ontological claim. I don't think this makes them anti-realists, just anti-certaintyists.]**

Another issue surrounds the knowledge status of theoretical entities (some of which I think critical realists would regard as "structures" or "deep structures"). Collier says that one of critical realism's claims about knowledge is *counter-phenomenality*, meaning that "knowledge of the deep structure of something may not just go beyond, and not just explain, but also *contradict* appearances." Since this is a claim that is supposed to be distinctive about critical realism, presumably an anti-realist would argue that knowledge would not contradict appearances or would reject deep structures outright. Is an anti-realist committed to this view? It doesn't seem inconsistent for an anti-realist to take a pragmatic approach to theoretical entities, to simply say that we can't know *truly* that they exist or operate they way they do. And although deep structures may imply knowledge that contradicts appearances, neither critical realists nor anti-realists would argue that the testable implications of these deep structures would contradict all empirical observations, otherwise the theory would be irrefutable or infallible. So on the one hand I'm not sure that there is much at stake here, unless the issue becomes again whether we can know the "truth" about theoretical entities. **[I think for an anti-realist there would be no meaning in the view that reality could contradict appearances, since there is no "reality" generating the appearances. As for the critical realists, it has never been very clear to me precisely what this idea of counter-phenomenality really means. If it is just that appearances can be deceptive – that a straight stick stuck at an angle into water looks bent or the sun appears to set – then I don't think it is a very big deal. All that means is that there is a specific relationship between (a) the causal mechanisms linked to the object under investigation and how it generates observations, and (b) the causal mechanisms linked to our perceptual apparatus and how it registers those observations. In these instances these two causal processes generate "deceptions". But it is always the case that these two kinds of causal mechanisms are operating, but only sometimes do they produce contradictory appearances.]**

On the other hand, critical realism may have something on anti-realism if certain anti-realist or strict empiricist views lead one to take certain *attitudes* toward theory construction and scientific explanation and inquiry. It seems plausible to me that if one harbors heavy skepticism about unobservable entities, then in the process of theory formation, one may overlook explanations that rely on unobservable or theoretical entities to fill gaps in empirical knowledge (the transphenomenality point). **[The realists point is not that unobservables fill gaps in explanation – which suggests that in principle there might be no such gaps. Rather it is a claim that an account of mechanisms inherently implies postulating a generative process in which all we can observe are its effects. This is always the case, because an observation is an effect. "Unobservable" just means "that which generates the effects we call 'observation.'"]** Or such skepticism may lead one to the presumption that appearances usually aren't contradictory, and therefore blind oneself to explanations that imply just that (the counter-phenomenality point). Such seems to be one of the points Marx makes about the fetishism of commodities (at least part of the inspiration for the claim about counter-phenomenality), where Marx criticizes "bourgeois" economists for taking the "natural" appearance of value as a property of things for granted, rather than seeing value as a property of social relations, a historical or institutional product (deep structures). And this argument seems to be one implications of

Collier's claim that empiricism "flattens" the distinction between experience and experiment (p. 32). So a third issue between critical realism and anti-realism may be attitudes toward building explanations and undertaking scientific investigation. Perhaps critical realism has something here.

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4. Ricky Leung

My questions come primarily from the section 'Reality as Differentiated' (p. 128-130). First, it seems that whether a scientific experiment is possible depends largely on how much the causal mechanism(s) of interest can be isolated. As the passage suggests, however, even if we can perform the necessary isolation, we might notice that the relationship between the elements in our artificial isolation is not very meaningful in a natural setting, where other mechanisms exist and interact with the mechanism of interest. In this sense, is there still any good reason for us to make conclusions about "a mechanism in isolation"? Is Bhaskar's distinction between open and closed system intended to provide some clarifications? If so, how? **[The open/closed system contrast is meant to be a specification of the context of research – only in the former is an "experiment" possible. Research is still possible in open-systems, but it becomes much harder to build strong evidence in support of claims about specific mechanisms, since there will always be more co-present mechanisms in play in generating the observations than in a more controlled experiment. This does not destroy the possibility of generating knowledge of open-system mechanisms, it is just harder to defend the claims one makes].**

Also, on p.133, why did the authors suggest that "Bhaskar's treatment of society as a continuous transaction between intentionally acting human agents and the social structures ... neglect human embodiment ... as participants in human social life"? Is this because once structures and agents are treated as ontologically distinct (as opposed to collapsed together), then an 'embodied' position is impossible? **[I am not sure I entirely understand your question (the elipses made it a bit hard to see precisely the question). The continuous transaction view makes two claims: a) without a structure no action is possible, and b) those structures only exist by virtue of the actions which they structure. This "dialectic" is what is meant by the "continuous transaction." I think ""embodiment" is meant to suggest that structures can become internalized in a way that gives them some kind of reality even in the absence of ongoing interaction. They become part of psychological structures (or perhaps even physiological structures) that have ontological continuity without ongoing transaction. A simlar point could be made about nonhuman entities that impact on social practices - -these have an existence that is not continually constituted through action. I am not at all sure that all of this matters for much.]**

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5. Elizabeth Holzer

How does critical realism help us to evaluate the effectiveness of conventional social scientific methods: survey analyses, ethnography, or laboratory experiments?

Consider this especially in light of the following concepts in critical realism:

1. The observation of the regular joint occurrence of events is *not* sufficient for demonstrating a causal connection. Instead, two events are causally linked only if a fundamental "necessity" links them together (Pawson 1989:130).
2. An open system is one in which "mechanisms coexist and interact with one another in contingent ways;" a closed system is one in which mechanisms exist in isolation—naturally or artificially (Benton and Craib 129). Causal mechanisms work (equivalently?) in either system.

With the demanding notion of causality, would critical realists consider statistical analyses of survey data that traditionally illustrate correlation to be "unscientific?" **[Systematic observations that establishes empirical associations would only be "unscientific" if it were claimed that the association was sufficient to establish a causal connection. In the mechanisms/events/experiences trilogy experiences (which basically means: observations) are crucial in specifying what needs to be explained. The only claim is that to count as an explanation, mechanisms must be posited.]** Or are researchers just directed to apply a theory-laden explanation to correlative findings. (Also, I'm not quite sure what counts as sufficient evidence of a fundamental necessity, which makes it harder for me to speculate on the likely response). **[Because of the thesis of fallibility of knowledge, there is never really "sufficient evidence" to have certainty about "fundamental necessity." What you do is make an argument about a mechanism capable of generating the events (=effects of mechanisms) which are registered by the observer as observations/experiences and then adjudicate among rival accounts of the operative mechanisms in a given situation. A medical diagnostic problem would be a good analogy: you observe symptoms. Those observations always involve some kind of observational mechanism interacting with the biological events generated by underlying causes, and ask what are the possible underlying mechanisms that could generate the events registered as observations? You have sufficient evidence when you can effectively adjudicate between rival possible mechanisms.]** Also, it seems intuitively more feasible to understand fundamental necessities that may work beneath the level of appears through in-depth study of a phenomenon—the appeal of ethnography is it leads to deeper understanding of a phenomenon than survey statistics of laboratory experiments (ideally), but critical realists advocate evaluating potential causal variables in isolation. Which implies that ethnography is actually the *worst* of the three methods. The desire to isolate variables seems to a large extent in tension with the desire for deep understanding, which requires *context* and *detail*.**[I think there is – perhaps – some confusion here about objectives of different sports of research. You are absolutely right that an ethnography may provide the greatest possibility of deep insights into the mechanisms generating some specific phenomenon. I think the question is what enables you to learn *from* a case study rather than simply *about* a specific case. Again, take the medical analog: when you study a concrete patient you have to take into account all sorts of issues in context and detail in order to make a proper diagnosis and come up with an appropriate therapy. But the inventory of possible mechanisms that are in play in the "open system" of the patient are primarily the result of a different kind of research, one that tries to isolate the effects of specific mechanisms (so that they can become part of the menu). I think that ethnographies are much more likely to generate robust knowledge of a more general sort when they are in close articulation with other kinds of research, research that attempts to "isolate" ion one way or another certain clusters of mechanisms, for otherwise it is very easy for ethnographers to make serious mistakes and misidentify causes. We will read about a good example of this in a piece by David Latin later in the semester.]**

It seems as though the open system/closed system discussion could be extrapolated into a defense of laboratory experiments à la Milgram or Zimbardo. But social psychologists critical of laboratory experiments have argued (convincingly) that within a laboratory setting, there are rules of social life that distort the artificial settings—for example, participants in the Milgram obedience experiments rather than believing they were inflicting mortal harm on the "learner" at the behest of the "authority figure" could legitimately have assumed that the legal authorities of Connecticut oversaw the experiment and the "authority figure" was constrained in his orders by the rule of law, and thus would not order them to kill the "learner." **[You're absolutely right. But there are possibilities of randomized experiments in natural social settings, not just in labs. In special circumstances you might be able to adopt a new form of work organization in one factory and not another, for example, and randomly assign workers to each "treatment." And, of course, there are sometimes quasi-experiments in the world.]**

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6. Brett Burkhardt
Critical Realism

According to Pawson (1989), the Bhaskarian goal of experimentation is the successful empirical activation and isolation of a single mechanism. This allows experimenters to penetrate to the level of the "real" and observe mechanisms at work. **[Strictly speaking what you are observing are the effects of the mechanism at work; the actual mechanism is not itself directly observed, just as set of events it generates. The idea of the experiment is that by varying the treatment you have better reason to believe that the effects observed are being generated by the posited mechanism and not something else.]** The process of experimentation begins with a theory of a mechanism (i.e. a *conceptually* closed system). An experimenter must then create a situation (i.e. an *empirically* closed system) that can demonstrate the existence of the mechanism under study. Pawson emphasizes this counterintuitive process by noting that evidence is "manufactured" to confirm a theoretical model of the conceptually closed system.

Pawson presents the study of pendular motion as an example of this process. Upon realizing that a pendulum does not always empirically follow the path predicted by existing theory, the empirical conditions of the experimental pendulum were altered to fit the theory. In this case, the existing theory seems to have been salvaged, but diminished in scope. That is, researchers maintained the theoretical mechanism, but had to stipulate that it only applies under specific conditions (e.g. short swings of the bob, or when "symmetrically shaped 'jaws' or 'cheeks' [are] rigidly fixed on either side of the plane of motion of the pendulum…(142).").

In seeking out empirical cases that confirm a given theory, is it the case that Bhaskarian experimentation necessarily leads to explanations (or isolations) of more precise, yet less generalizeable, mechanisms? **[More precise, yes. But in a way, <u>more</u> generalizable, at least in the sense that one will have confidence that the mechanism in question will have the tendency to generate the effect in question.]** This seems like a feasible prediction. After all, experimentation requires the removal from the empirical system of all extraneous variables (or mechanisms). What we learn, then, is that mechanism X operates only under conditions in which mechanisms A, B, and C are absent (regardless of whether we think mechanism X still exists without being active).

If this is true (and perhaps even if it is not), critical realist experimentation seems to leave researchers with an overwhelming (insurmountable?) task. What knowledge do we gain from identifying various highly particular mechanisms that exist in the "real" world? Critical realists

acknowledge that mechanisms can have countervailing effects that cause other mechanisms to be latent. They also acknowledge general interactions between mechanisms. Thus, a natural step to follow the experimentation described above is experimentation that isolates two (or more) mechanisms; essentially, isolating *a single interaction* while controlling for other interactions. This isolation of mechanism interactions seems to be the only way of gaining practical insight (at the level of "events") through critical realist experimentation. However, if "first order" experimentation has the effect of confirming an increasing number of highly particular, ungeneralizeable mechanisms, then "second order" experimentation on mechanism interactions seems to be a terribly complex and overwhelming, if not unachievable, task. **[You are completely right that in the world interactions among mechanisms is of fundamental importance. The world is not additive: knowing that mechanism X generates A, and mechanism Y generates B, does not show that when X and Y are both present they will generate A and B. This is why interactions are so important and exciting in science, including social science. In practice it is not so hard to develop convincing evidence about such interactions.]**

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7. Matt Desmond

What is a mechanism? Accordingly to Pawson, a mechanism is not a "variable" but an "account" (p. 130), which "explains causal laws" (p. 138). Collier claims that Bhaskar's reading of a mechanism is "that to which a law refers" (p. 43), and he goes on to point out the generative nature of mechanisms to build on each other while "digging deeper," the fundamental scientific task (p. 49). We might restate that a mechanism is a *theory*, then, such as the theory of atomic number and valency, the theory of electrons and atomic structure, etc. (Collier, p. 49), and thus is a context or a system of a cause, but not a cause in and of itself. **[I'm not sure about this. I think we have theories about mechanisms. The theories are meant to identify the generative process by which events are produced and then registered as observations/experiences. So, I don't think a mechanism is a theory – although, of course claims about mechanisms are theories.]** It also seems that a mechanism can be understood as a "structure" in whole or in part. Collier writes, "In asking about the structure generating some power of some entity, we are asking about a *mechanism* generating an *event*" (p. 43), and later continues, "A generative mechanism, we might say, is that aspect of the structure of a thing by virtue of which it has a certain power" (p. 62). Both views seem problematic, however, when thinking about experimentation. To a critical realist, the key purpose of experimentation is to isolate mechanisms (Pawson, p. 142; Collier, p. 33 & 36), and in this sense a mechanism sounds much more like a cause or a variable than an account of causes or variables. **[A mechanism is a cause, where "cause" is understood as the effect-generating process, but not a "variable" in the ordinary sense. The emphasis on experiment is to solve a complexity problem which befuddles our ability to clearly see the effects a given mechanism is supposed to generate. Experiments make certain kinds of observations possible – the kinds of observations that should occur if our theory about the mechanisms were correct. I think this** Further, how does one isolate a structure? Further still, if mechanisms are beyond appearances (transphenomenality) and counterintuitive (counter-phenomenality) (Collier, pp. 6-7), and holistic (Pawson, p. 135), then how can one isolate a mechanisms if its nature is relational? **[The isolation is of a particular mechanism in a system with many mechanism – this is what "treatment" means in an experiment. One is not isolating a mechanism from its effects.]**

Further, philosophers of critical realism assert that mechanisms are not instrumental models or "analytical toolkits" but rather are *real*. Bhaskar calls them "enduring mechanisms of nature" (Pawson, p. 138). This implies that mechanisms are not theoretical constructs, but rather existing systems of relations, and the scientist's job is the work to find them: "We not only have to work to make the mechanism appear, we have to work to make ourselves capable of understanding the appearance" (Collier, p. 54). Mechanisms are not built but "triggered." I have two questions that follow from these claims. First, if the real is the mechanism, and the mechanism is theory, then in some situations a difficultly arises when attempting to explain the real outside of the theorist. **[But I think you are incorrect to say "the mechanism is a theory." Our accounts of mechanisms are theoretical, of course, but the attempt is for those accounts to be of something real.]** Would we have Marxism without Marx? **[We would not have Marxism, but we would have capitalism, class relations, contradictions of accumulation, and the other elements of Marxist theory. Given the relative weakness of social scientific theories and the specific mixture of science and ideology in all social science (but perhaps most poignantly in Marxism) it is unlikely that the specific inventory of mechanism Marx identified would have been identified by someone else. But note the contrast with Darwin. Surely without Darwin we would still have evolutionary biology.]** Second, Collier claims that for Bhaskar, different scientific approaches should be understood as different mechanisms: "…'aerodynamical', 'biological', 'economic', etc. ought not to be regarded as differentiating distinct kinds of events, but as differentiating distinct kinds of *mechanisms*" (p. 47). If this is the case, could we deduce that different approaches within disciplines are also mechanisms—social psychology, economic sociology, ethnomethodology, in sociology for example—and thus, competing ideas emerging from various faculties are not the product of assorted approaches or research postures but rather multiple mechanisms, and in turn, multiple "realities"? **[I think this is basically right, with a few caveats. That is I think Marxism and feminism identify different mechanism – roughly class-mechanisms and gender-mechanisms – and both exist in the world and generate events, which in turn are registered as experiences/observations. It is not that there are multiple realities, but that reality is generated by multiple intersecting and interacting mechanisms.]**

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8. G.C.
Does experiment-ism differ from empiricism?

"Where a genuine causal mechanism has been isolated as a closed system, we can say 'every time A occurs, B follows', as in Humean causality" (Collier, 34).

To have isolated a mechanism (Humean cause) is to have a closed system.

How do we recognize a closed system?

Norman's experiment with the magnetized wire "allowed the earth's magnetism to operate on it unimpeded"(33). Could it not also have been the case that Norman had a very solicitous guardian angel that invisibly *made* the needle behave as it would have in accordance with the (mechanism of the) earth's magnetism? **[Of course this could be true: there can always be other possible mechanisms that generate the effects we observe in the most closed of experiments. And the**

problem only gets vastly worse in open-system observations. The key issue in your example would then be: is there any effect of the angel that would be different from the mechanism postulated by Norman? If there is never any way of adjudicating between the two proposed mechanisms than either we conclude they are really the same proposed mechanism or there are no stakes in adjudicating between them. I suspect the angel example would quickly become unscientific – in the Popperian sense -- if the mechanism were spelled out: no conceiveable observation with or without the experimental treament could contradict it.]

Indeed, Collier admits that we can only know fallibly that other mechanisms are not interfering and that "in fact, no system in our universe is ever perfectly closed, but experiments can approximate close enough to closure for the purposes of science"(33). If experiments are only a mere approximation of perfect closure, may we not just as well call them imperfectly closed, i.e. open? Is this merely to indicate the same ineradicable residuum of doubt as Hume?

Hume still looked both ways before crossing the street. I do not have to remove the sort of doubt about how storks manage to exist when I am not directly perceiving them before I go about the business of critiquing the thesis that storks deliver newborn babies to their mothers.

It seems that the critical attitude never fully does away with the dogmatic attitude. To say, "babies are delivered by storks, but I could be wrong" is no more or less critical than "babies develop in their mothers' wombs before being born, but I could be wrong". Wittgenstein was intrigued by a paradox discovered by Moore: "There is a fireplace in the room, and I don't believe it". This is not technically a logical contradiction, for a statement about a room and a statement about the contents of my beliefs are two separate things. But how separate? Can we not help but being a bit thoroughgoing in our affirmation of what we affirm, when we affirm it?

At the beginning of his book, Collier indicates that, like the pub patron we encounter, "those fifty million or so of our fellow citizens who are not arts or social science graduates" are philosophical ciphers. He then goes on to quote Gramsci, that "everyone is a philosopher", even if only unconsciously. Does it follow that everyone else should study philosophy like arts and social science graduates do?

[I am not entirely clear what your objection is – if any – to the critical realist claims about mechanisms generating events, and the relevance of experimentation in providing evidence in support of such claims (or adjudicating between rival claims). I don't see how the fallibility problem undermines the characterization of explanatory goals; it just questions the certainty with which we know when we have succeeded.]

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9. Dan Warshawsky

This week's readings engage us in a study of critical realism. As noted in the literature, critical realists claim that phenomena exist independently of the observer while specific mechanisms are internal to the process of observation. Towards the end of Ted Benton and Ian Craib's discussion of critical realism, they interrogate whether Marxism maintains many of the essential components of critical realism. Is the Marxist philosophical approach to social inquiry intrinsically critically realist, and what connection, if any, is there between critical realism and

emancipatory politics? Secondly, how has Marxism (as a part of critical realism) been perceived during moral relativism discussions in recent decades?

Intrinsic to Marxism, is increasing class consciousness (as a result of increased knowledge), and eventually utilizing this new power as means to create structural change in society. Roy Bhaskar emphasizes this connection between knowledge and emancipation. Bhaskar notes that the capitalist system is intrinsically misleading as ideas like freedom and equality are illusions. One's ability to sell his or her labor power is not necessarily correlated with abstract concepts of freedom and equality. Rather, there are false beliefs which pervade in order to maintain status quo. As Benton and Craib note on page 136, "The wage form, then, is misleading: it entails false beliefs about people's real relationships. But, more importantly, these false beliefs are actually engendered by the relations themselves, and, as ideological legitimations, they play a part in maintaining the coercive relations they disguise" (Benton and Craib, 136). Although there seems to be a strong connection between false beliefs and the existing power structure of post-industrial capitalism, more is at stake with critical realism. **[This version of the Marxism formulation – with the implicit *functionalism* in the explanation – came under a lot of criticism in the early 1980s. It is one thing to observe that ruling cvlass actors propagate ideas which serve their interests. This is basically the argument in *The German Ideology*: if you are powerful you control the means of intellectual production and see to it that ideas which serve your interests are dominant. The commodity fetishism idea, however, is different. Here there is a spontaneous production of mystification by the social relations themselves: distortions of the production process are generated by the relational practices within the production process in just such a way as to stabilize what would otherwise be quite fragile relations. This just sounds too good to be true: the oppressive relations by themselves generate precisely ideological effects they need for their own reproduction. It seems to me that if these social relations spontaneously and intrinsically (by necessity) generate mystifications, it should be as likely that they would be system-undermining as system-preserving mystifications.]**

When studying the connection between emancipatory politics and critical realism, morality, especially questions of moral relativism inevitably surface. Some critics of Marxism believe it is just another dogma that has its own set of inflexible truths. When one delves into Marxism and critical realism, the decision to accept objectivity as possible is ultimately a personal "leap of faith." Yet, this discussion about Marxism as dogma is secondary as the main focus of this interrogation is to place Marxism within the critical realist framework, if possible. As noted by Andrew Collier, Marx's use of empirical data in terms of specifically defined ideas give it the appearance of experiments, even though Marx never intended to create experiments in the social human world. This gives Marxism a less positivist and more critically real quality.

Is Marxism strongly associated with critical realism? Its focus on emancipatory politics and objective truths and falsities resemble many of the central characteristics of critical realism. In all, I would say that Marxism is one of the quintessentially critically realist philosophies. Additionally, I do not think that this is any surprise as it resurfaced in academics during a time when many "critical" social sciences were abandoning a neo-classical economics, part of a different type of realism. In all, critical realism's close connection to Marxism exemplifies how central emancipatory politics are in critical realist academic discourses. **[I think the main reason Marxism is close to critical realism is its commitment to a layered notion of the social world, with appearances being generated by underlying processes, sometimes called "essences." This aspect of Marxism is distinct from the additional functionalist claim that the appearances actively mask the reality in ways that reproduce the reality, as opposed to simply are distinct from the underlying reality.]**

10 Mark Cooper

Fallibility and (non-) Realism

Both B&C and Collier argue that a central feature of critical realist is that it is "fallibilist" or that its theories assume "fallibility." (BC, 121; C, 6) I am uncertain about three ways in which this concept is expressed elsewhere in the readings.

The first of these is what I consider Collier's straw-man-ization of non-realism. (Although some of the alternative perspectives to critical realism are scheduled for later in the course – feminism/standpoint – I think a fuller view of non-realisms might be helpful this week.) Collier claims that the denial of objectivity by non-realists is also a denial of fallibility. (12) He then derides non-realism because it "licenses…uncritical dogmatism," "make[s] cognitive discourse invulnerable to cognitive assessment," and fails to establish any ground that could be emancipatory. (14-15) While I remain quite skeptical of non-realist philosophies of science, I have heard cogent arguments that seem to belie Collier's assessment.**[Strictly speaking, the anti-realists denial of objectivity does not deny fallibility as such, but rather denies the very relevance of the fallibility problem. In debates over the concept of class, for example, anti-realists will state "there is no such thing as a false definition of class, just an unhelpful one." A realist could claim that some definitions are false inspofar as they fail to identify a real generative mechanism.]**

The second of these is B&C's (from Bhaskar) discussion of Marx's critique of the wage form. Is seems to me that such reasoning inevitably relies on analysis of "open systems." As B&C state, "The epistemological limit to naturalism is the impossibility of experimental closure in the social sciences." (133) In what sense then, are conclusions such as those drawn by Marx in the example genuinely 'scientific' rather than simply 'narrative?' Moreover, how is the objective grounding of these emancipatory value judgments genuinely objective (or leads to a "realist account of ethics" if they inevitably rely on analysis of open systems? **[The open-ness of an open system simply reduces our confidence that we have properly identified mechanisms. Closed systems make it much easier to have a lot of confidence in such claims, although there still is never complete certainty. I am not sure how this bears on the idea of "objective" emancipatory value judgments or a realist account of ethics. If such accounts make sense, then they would be more difficult to defend in open systems just like an objective claim is ore difficult to defend in an open system.]**

It is unclear to me how to reconcile claims to fallibility (and how this might differ from falsification) against Pawson's argument that the "realist model would seem to allow them to bash the system into the requisite shape, however relentlessly, all in the name of 'experimental control'." (P, 143) Pawson further argues that it is a theory's location within a network, rather than its use of empirical evidence, that serves as the primary validation for a given theory.**[I am not sure that Pawson wants to <u>reduce</u> "validation" to consistency within a network of ideas. The network imposes a constraint on the potential to "bash the system into shape", since the reslting experimental has to make sense within the network of idea. But the validation of claims about specific mechanisms is still just as dependent on empirical evidence from observation/experiment as from logical evidence of consistency within a conceptual network.]** To what extent is Pawson's assessment of Bhaskar correct in his summary that: "Any regularity can be manufactured…if we make drastic enough adjustments elsewhere in the

experimental system…Conversely, by the same token no regularity is immune to experimental falsification." (149)

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11. Ana Cristina M. Collares.

My interrogation for this week puts together several questions related to the nature of mechanisms and experimentation in Baskar's critical realism, and how can this view be applied to social sciences.

First of all, I would like to start with a clarification question. Pawson claims, in page 149, that "if one allows that scientific knowledge takes on exactly the same form as ordinary discourse, then one has to confront Quine's famous thesis that any statement can be maintained true in face of any evidence".

1) Quine states that there can be no empirical derivations from theoretical statements, and that empirical evidence and theory are all implied in the same system of assumptions that can be re-arranged to accommodate any kind of evidence. Is he saying that, because of that, there is no difference between "ordinary discourse" and "scientific discourse"? **[I am afraid that I do not know Quine's work and cannot really answer this.]**
2) Pawson, or Pawson's account of critical realism, seems to suggest that "ordinary discourse" or "common sense" is different from science only because the experimental system is build apart from the system of theoretical models, and they can be confronted with each other in order to find consistency. How can this inconsistency be detected, i.e. the mechanisms exist in the real world without the concurrence of measurement, but how can scientists build the bridge between their measurement instruments, or their sensations, and the system of empirical regularities? **[I think the pivotal issue here is that scientists are always working, in effect, with theories about two quite distinct kinds of mechanisms – theories about how their measurement devices record observations and theories about how mechanisms in the world generate the events recorded by the measurement devices. In general there is no reason to suppose that the theory of measurement is such that it will tend to lend support to any particular theory of mechanisms in the world. Where there is a reason to believe this – where the theory of measurement devices appear to be "rigged" to support the theory of mechanisms – then we become naturally very skeptical about the resulting evidence. This happens all the time in social science, for example, in the debates over IQ tests.]** How can we know that a system is really closed? **[In social science the system is almost never even partially closed. The best we can do is simulate closure through various devices, such as statistical controls.]**

This leads to my second issue: I understand from the readings that, according to critical realism, scientific research implies in finding the underlying mechanisms that structure the causal relations **[I am not sure this is quite the precise way to put it. Mechanisms don't "structure causal relations"; mechanisms are causal relations. One could say that mechanisms structure empirical observations, or perhaps mechanisms structure empirical relations. But mechanisms are a way of talking about causation, at least if causation is understood as the processes through which effects are generated]**, and scientific experimentation aims only to find consistencies in the causal relations suggested by these mechanisms. So, where can I situate

what I call "basic research", i.e., research that is not searching for mechanisms, but aiming to select or detect regularities (for example, when a biologist find a new kind of animal and is trying to classify it)? **[Research is important for figuring out what needs explaining as well as figuring out explanations. What you are calling basic research is part of figuring out what needs explaining.]**

I am asking this question because I was thinking about the role of experimental research in sociology. It is suggested that social sciences cannot be treated as natural sciences in the view of critical realism, but "it is still possible to have a science of society in the same *sense* as the sciences of nature, but not necessarily of the same form as them and to employing the same methods" (Ian and Craib, p.131). Is experimentation, and the use of control groups, the alternative for social sciences to try to confront the theoretical and the experimental system? This wasn't clear for me from the texts. **[Of course, lots of natural sciences don't really use much experimentation: meterology, astronomy, cosmology, etc. Small group experimentation is an example of experimentation in social science, but there are also randomized treatments in natural settings, and certainly lots of quasi-experiments, although those are always vulnerable to misidentification of mechanisms. In natural quasi-experiments the problem of self-selection in the cases being compared always threatens the credibility of the experimental claims.]**

Finally, another thing that was said from social sciences from the point of view of critical realism is that it cannot have closed systems. Max Weber (in many places, but specifically on "Methodology of Social Sciences", 1949) presents an account of the sociological method according to which the reality has multiple aspects and the scientist has to "cut" pieces of the real in order to find causal connections. Can these "pieces" of reality be considered "closed theoretical systems"? **[A partial system is not exactly a closed system. Formal models try to create a closed system in thought, but these models only reveal the logic of mechanisms under the radical assumptions of the models, and thus are not really like experimental closed systems.]**

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12. Matt Nichter

'Transcendental' seems like a misnomer. Kant's arguments were intended to prove what MUST be the case in order for X to be possible. Bhaskar's arguments, which Collier insists are fallible, appear to be of the standard 'inference to the best explanation' type. **[I think the goal of the explanation is still transcendental even if because of the empirical and observational complexity one can never be certain that one has established necessity. The idea is still figuring out, as best as one can, what must be true about the world for X to be possible. There may just be more than one possible state of the world, given what we know, that would make X possible.]**

I don't see how Bhaskar's arguments (or any arguments, for that matter), can refute evil-demon/brain-in-a-vat forms of idealism, which are consistent with any possible sense experience. We have no better grounds for believing that a structured, hierarchical external reality accounts for the character of scientific experiments than we do for belieiving that an evil demon accounts for them. (I am not much exercised by such arguments. I only mention them because the authors seem to suggest that

Bhaskar has somehow refuted idealism as such.) **[At the level of vagueness just stated, you are right. But can the evil demon enable us to make predictions about things we have not yet observed on the basis of the theory of the effect-generating process? Isn't the evil-demon only usable post-hoc to explain what we have already observed, not some as yet not observed event?]**

More importantly, I don't see how Bhaskar's arguments can rule out the most currently popular forms of anti-realism, which deny that we have grounds to say our understanding of theoretical/unobservable entities is anything more than 'empirically adequate' (consistent with phenomenal appearances) given the underdetermination of theory by data and the inifinity of distinct theoretical entities consistent with any amount of data. **[This is where Pawson's consistency criterion around the relationship between the measurement-mechanisms and the observation-social-mechanisms comes into play. It may be that data is underdetermined by theory, but it is tougher to imagine strong consistency conditions being met automatically in which there is consistency simultaneously a) within a network of ideas within the theory, b) within the network observational mechanisms, and c) between the theory-ideas network and the observational-network. Of course there is enough slippage within each of these that there is lots of room to manoeuvre, but still, all of this makes it possible to adjudicate between contending claims about the mechanisms generating observations.]**

Between these two extremes - radical absolute idealism, and more modest anti-realism about the nature of theoretical entities - lies crude, traditional empiricism. I think this must be Bhaskar's main target. And I agree that positing unexercised powers and mechanisms better explains the character of everyday scientific practice than does crude, traditional empiricism.

I would, however, like to discuss further what is meant by a 'mechanism.' The best I could glean from the readings was that a mechanism is an underlying structure in virtue of which a thing gains its causal powers. **[that seems right]**

Finally, a quick aside about 'stratification ' (of reality, not society). Benton and Craib argue that reductionism is mistaken because 'entities at higher levels have powers and properties not predictable in advance on the basis of properties of lower-level entities.' But prediction is beside the point; the reductionist need maintain only that all of the causal properties of the higher level phenomena can be explained (though not necessarily predicted) in terms of those of the lower level phenomena. Also, Benton and Craib speak of causality 'flowing down the heirarchy' as if this were an obvious fact. But their examples (emotional trauma affecting the central nervous system) beg the question against reductionism. (I'm not defending reductionism; I just don't think Benton and Craib's arguments are valid.) **[I think you are right about the downward causation point. This seems, at best, something quite specific to the linkage between mental and biological mechanisms. It is hard to see chemistry have down effects on physics, or even what that would mean. We will talk about reductionism in a couple of weeks.]**

13. Matías D. Scaglione

1. Is "**realism**" a new word for "materialism"? Since the "realists" believe in the existence of an *external* (material, cf. Collier, 3) *world* "independent of, and often, defying, our desires of it and attempts to change it (Benton and Craib, 120): can we say that they are "materialists" in the conventional, not-necessarily Marxist sense of the term? Does the external independent world have for the realist any impact on the true or false beliefs? Is the external world so independent for the realists so that *false beliefs are just the result of ignorance*? **[Realism and materialism are consistent, but critical realism involves a number of specific formulations that would necessarily hold for all materialists, such as the claims about open vs closed systems and the problem of the transitive and intransitive objects of science. One might see realism as an elaboration of a number of philosophical implications of a materialist perspective. On the issue of true and false beliefs, critical realism is a methodological stance. It does not inherently take a stance on the issue of the social production of false beliefs. One view is that they are just ignorance, but you could have a critical realist theory of belief systems in which there was an active mechanism of belief distortion operating in a social setting.]**

2. It seems to me that the "realist" meaning of the adjective "**critical**" is at loggerheads with the definition of "realism". As long as the critical realists "tend to share the social realists' commitment to *changing* unsatisfactory or oppressive realities" (ibid.): aren't they including *ideal elements*, thus undermining their original "realists" tenets? How does a critical realist know that their beliefs regarding the evils of the world are true, how does he know, for instance, that the profits arises from unpaid labor and not from the work of the capitalist, if not *as a result* of the *critical study* (for instance the Hegelian/Marxist immanent alternative) of capitalist categories, etc. In other words, is critical realism ultimately subject to the *ideal moral commitments* of the social researcher? **[I have never had a very clear understanding of the claim that there is something in the philosophical postulates of critical realism that imparts to it an emancipatory project. I can see why, if a person is committed to emancipatory goals, that critical realism is a good methodological stance, since understanding the mechanisms that generate harms seems important for eliminating those harms. But I don't see how in and of itself critical realism provides a moral judgment on what counts as a harm or what counts as social justice. (I suppose one could develop some notion of human flourishing rooted in a theory of "objective needs" which might move towards a realist view of harms, but I don't know if one can really build a full emancipatory theory strictly on realist premises.]**

14. Eva Williams

In an otherwise straightforward essay on the central perspectives within the critical realism framework, Benton and Craib (2001), provide further clarification of the distinctive differences between critical realists and positives utilizing the classic discussion of structure/agency. They

conclude this section by stating that the various ways that critical realists have engaged with the issue of structure/agency have been "…very effective against both methodological individualist and empiricist tendencies to dismiss the reality of (unobservable) social structures. Yet in the very next breath readers are informed that Bhaskar's perspective, that the "…treatment of society as a continuous transaction between intentionally acting human agents and the social structures they reproduce or transform"(133), misses something important. According to Benton and Craib, Bhaskar's perspective, "…neglect(s) both human embodiment, and the significance of non-human materials, processes, living beings and so on as participants in human social life"(133). What are the author's trying to convey here which Bhaskar's framework lacks? What do they mean by the "human embodiment" or by "living beings" that is not contained in the above: "intentionally acting human agents"? **[I found this passage a bit mysterious as well. I think they are saying that social structures actually become durably internalized in actors, perhaps as mental structures, in ways that no longer depend upon "continuous transactions". "Embodied" then means quite literally that social relations have become part of a person's body, and thus is no longer simply enacted in continuous social transactions. This would suggest that the agent becomes a site of mechanisms that are more substantively autonomous from the structures within which the agent acts (or something like that). Frankly I don't see why this is such a big deal, why this matters for anything important in the critical realist stance.]**

Ray Pawson's 1989 work *A Measure for Measures,* raises some interesting philosophical questions related to the way that repeated exposure to a set of constructs and the instruments used to measure them results in a level of familiarity which permeates into our collective understanding of the world around us. Is this similar to the way a product name will become synonymous with the product itself? His example is that of the measurement of length. "Our familiarity with the family of rulers," he explains, "…has lead to a situation in which our notion of the property becomes almost synonymous with the instrument used to measure it" (145). At the root of this discussion is the problem of the "operational definition" which according to Pawson (staying with the above example), "…covers only a small part of our understanding of matters of length and distance…"(145). He concludes, "…it is illusory to imagine that it is these simple instruments which give the concept such a ready and universal meaning"(145). While certainly compelling to wrestle with, how does Pawson resolve this? It would seem that on page 148 he is advocating for a "network of scientific discourse" but he fails to elucidate for readers what precisely what he means by this. Thoughts??? **[I think the network idea of scientific discourse is meant to solve a potential problem with viewing theories as simply a serial collectives of ideas. In the non-network view it is easier to see how all observations could always be rendered compatible with theories, since there is nothing to constrain the reconfiguration of the theoretical elements. They are just an ad hoc menu of elements which can be bashed around until the theory and data fit each other. The network of ideas view makes it harder to manipulate the theoretical structure. I am not sure what this has to do with the problem of the familiarity of operational measurements, however.]**

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15. Mara Eisch-Schweitzer

In Latour's "anthropological" approach to reconstruct laboratory life, Bennet and Craib describe what was constructed as a seamless form of life, incorporating all its many aspects, just as the ethnographer of other cultures claims to be reconstituting the centre of those cultures: their belief-system, their technologies, their ethnosciences, their power plays, their economies "in short, the totality of their existence" (Latour 1993: 100). Could this be understood to be the culture of science; in turn the "culture of empiricism"; and how would that differ from the "ideology of empiricism"? **[I don't quite understand the question here. I don't know Latour's work very well, so it is a bit hard for me to react to this.]**

I am struggling with the concept of "meaning" throughout our readings. If experimentation is when, for example, data are created and evaluated through the process of attempting to build an experimental system which duplicates the blueprint set down in the mathematical model (Pawson, p. 141), don't we at the end of the experiment really only have more theoretical and observational terms? In this sense, doesn't science just continue to create knowledge (information) without real meaning? **[I am not completely sure what you mean by "meaning" in this context. The theories in question have content: these are theories of specific generative mechanisms that produce effects in the world. Isn't the "meaning" bound up with the effort an identifying such effect-producing processes? Experimentation is a way of subjecting our claims about such mechanisms to critical tests, however imperfect those may be, by trying to isolate the operation of specific mechanisms.]**

I also have a question from last week's discussion regarding Kuhn. I understood, that according Kuhn, a paradigm to govern a group of practitioners not the subject matter. In this view a discipline could have one or more paradigms.**[In Kuhn's view a paradigm also constitute a way of distinguishing scientific from unscientific work, of demarcating knowledge-producing practices from obfuscation. A discipline can certainly contain multiple paradigms, but – in Kuhn's work – this would either indicate that the discipline was internally in a revolutionary transition (a new paradigm replacing an old one), or in a state of perpetual civil war, or perhaps in a prescientific stage where no paradigm had reached a fully scientific level of problem-definition. Kuhn did not believe, as far as I know, that you could have a happy pluralism of distinct paradigms within a given field.]**