

CRITICAL REALIST PHILOSOPHY FOR SCIENCE

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### Abstract

This paper considers the philosophy of science that is central to Critical Realism (CR). Following a discussion of the foundational transcendental arguments of the movement, it reviews stratification, emergence, and the possibility of a unity between the natural and the social sciences. CR is broadly contrasted with other versions of scientific realism, empiricism, and idealism. The philosophy of psychology is considered as a test case in which critical realism can be demonstrated to be a workable theory of science. Specifically, a theory is proposed of social psychology that would be scientific in virtue of meeting the standards of a critical realist view of science.

## Table of Contents

1. Introduction	1
2. Transcendental Realism	5
3. Central Tenets of Transcendental Realism	35
4. A Critical Realist Version of Social Psychology	61
5. Conclusion	84
6. References	86

## Critical Realist Philosophy for Science

### Introduction

As a graduate student in London studying the philosophy of the social sciences, I was introduced to the ideas of Roy Bhaskar by way of the question of the degree to which the social sciences are or can be unified with and studied in the same manner as the natural sciences. In The Possibility of Naturalism (1975, 1998) Bhaskar posits a unity of the natural and social sciences by virtue of a realist theory of science in which the aim of study is the same in both cases. Rather than looking for laws (i.e. Humean constant conjunctions), which seem not to apply to the social sciences, Bhaskar holds that science (be it natural or social) seeks to understand the structures, mechanisms, and processes that are efficacious in causing the phenomena under study. A number of my fellow graduate students and I became interested in Bhaskar and the movement that grew out of his ideas.

Roy Bhaskar studied philosophy, economics and politics at Oxford. He has taught at and/or been a Research Fellow at the University of Edinburgh, Pembroke and Linacre Colleges, Oxford, University of Sussex, and City University London.

*Critical Realism* began as a theory of science and then a theory of the social sciences, *transcendental realism* and *critical naturalism*, respectively. The term *critical realism* (CR) is an elision of these two terms. Implicit in Bhaskar's theory of social science is a theory of *explanatory critique* (a critique that is so in virtue of its explanatory power) that is central to CR ideas regarding ethics and politics. The fourth central aspect of Bhaskar's philosophy is his development of a theory of *dialectic* that is largely a refinement of Hegel and Marx— a "process of conceptual or social ... conflict,

interconnection and change" (1993, p. 3), a process which results in the removal of causally efficacious obstacles to human flourishing. Dialectic also speaks to ethical and political theory.

In London Bhaskar is a well known philosopher. His work has been described as "a *Copernican* revolution in the philosophy of science" (Harré, 1970, p. 15).<sup>1</sup>

Considering the widespread awareness of Bhaskar's work in the UK, I was rather surprised to discover, upon returning to Canada, that virtually nobody has heard either of the movement or of Bhaskar.

The purpose of this thesis is to contrast and compare Critical Realism with other broad themes in philosophy of science, and to endeavor to show that the understanding of scientific and social scientific phenomena offered by CR is more coherent and plausible than rival philosophies of science. I try to present a readable summary of the CR theory of science (*readable* because Bhaskar's own writings, though rich in original ideas, became increasingly impenetrable—due to obscure grammar, grammatical and syntactical error, and a penchant for neologisms — with each passing work), resting primarily on Bhaskar's central argument for realism.

The focus of this paper will be the CR theory of science, so it will center primarily upon transcendental realism, and to a lesser extent critical naturalism. The theories of explanatory critique and dialectic are briefly touched upon, but only in the capacity of contributing to a better understanding of the CR view of science.

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<sup>1</sup> In a 1998 conversation with Nancy Cartwright I mentioned something that I had recently read from Bhaskar. Her response was that she had been working on an almost identical idea but Bhaskar had beaten her to the punch, so to speak, and that "that [expletive] Bhaskar has done it *again!*"

The entire critical realist movement began with a modest argument that initially aimed at solving some problems in the philosophy of science; the movement could be said to stand or fall with the five pages that comprise the third section of the first chapter of A Realist Theory of Science (which is described in the next section of the paper).

Bhaskar observed that the deductive nomological vision of science was being attacked piecemeal, but that “the ensuing accounts of science have been unable to sustain the coherence of precisely those features of science which they have placed to the fore of the philosophical agenda, namely scientific change and the non-deductive aspects of theory” (Bhaskar, 1986, p.1).

For example, he considered philosophers of science like Kuhn and Feyerabend to be developing a view that the world is created and changed by us (a view he calls “subjective super-idealism”), but argued that such views could account for neither how there can be conflicting descriptions, nor what (if anything) is described by those descriptions. He considers Popper to be in a similar predicament in that he has not shown a refutation to be rational unless one can assume nature to be uniform, yet Popper has not shown that he has a solution to skeptical arguments (Bhaskar, 1997, pp.217-218).<sup>2</sup>

Bhaskar offers a transcendental argument to explain how it is possible that there can be change in scientific theories and in experience. Bhaskar’s usage of the term “Transcendental” is in line with Kantian thinking (Bhaskar, 1998, p. 5). He premisses his argument on a certain type of experience, then tries to infer what the world *must* be like in order for that experience to be possible. His premisses are not specific scientific theories; rather they rest upon the observation that changing experience necessitates that

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<sup>2</sup> For, example, there is no “conclusive practical experimental proof of falsity” since, in any scientific “anything like conclusive proof to settle an empirical question does not exist”(Popper, 1982: xxii).

scientific theories are replaced and refined apparently continually. His conclusion is transcendental realism.

### Transcendental Realism<sup>3</sup>

It could be contended that any philosophy is realist in some sense. Bhaskar casually suggests that this is the case (1986, pp. 5,6) though he does not make a case for the point. One could, for example, consider Plato and Berkeley to be realists regarding forms and sensations, respectively. “Scientific realism” according to Bhaskar “...is the theory that the objects of scientific inquiry exist and act, for the most part, quite independently of scientists and their activity” (1986. p. 5).

Collier (1994, pp. 6-7) suggests that there can be degrees of realism that can be claimed by scientific realism based on the following desiderata:

1. Would the facts that make the claim true be so independently of someone knowing of their truth? Are the claims **objective**? Whether or not something is real is independent of whether it is an object of knowledge. An object or power may continue to be without appearing to be.
2. As a realist theory makes claims that are not about empirical data, but rather that which lies behind empirical data, realist claims can always be refined or refuted. Realist claims are always **fallible**.
3. Realist claims refer to something deeper than appearances (except in the special cases—in introspective psychology for instance—where the representation *is* the object of interest. There is a small discussion of this toward the end of this chapter). The conditions and causal mechanisms that bring about the appearances are liable to

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<sup>3</sup> Note: in the following sections CR (Critical Realism) and TR (Transcendental Realism) are used somewhat interchangeably. CR refers to the entire Critical Realist movement, while TR refers specifically to the transcendental argument that underpins CR.



last longer than, or be more stable than the appearances. Realist claims are **transphenomenal**.

4. In addition to transphenomenality, the realist theory also claims to be **counter-phenomenal**, in that it is possible that in addition to explaining empirical data, the theory could *contradict* appearances. Collier suggests (after Marx) that it is “precisely the capacity of science for counter-phenomenality which made it necessary: without the contradiction between appearance and reality, science would be redundant, and we could go by appearances” (Collier, 1994, p. 7).

The CR theory of scientific realism, *transcendental realism*, can be described as a shift from the primacy of epistemology to the primacy of ontology. In the classical Empiricist approaches to science that serve as a foil against which Bhaskar develops his position, ontology follows from epistemology, in that what the empiricist will allow to be *real* or *existent* tends to follow from what can be known. Transcendental realism is the assertion that reality exists independently of, and has ontological priority over, representations of it. Scientific realism is “not a theory of truth..., but of *being*...” consisting “first and foremost, of a theory about the nature of the being, rather than the knowledge, of the objects investigated by the sciences” (Bhaskar, 1986, p. 6). External Realism (Searle’s realist presumption, similar to the conclusions of CR (Searle, 1985, Chapters 7 & 8)) is an ontological assertion that there is a way that things are, independent of how they seem. Realism is not a theory of truth (i.e. not the correspondence theory of truth), not an epistemic theory entailing a “god’s eye view” as suggested by Putnam, and not a theory of language necessitating a single correct descriptive vocabulary.

Bhaskar's aim (1997/1975, chapter 1; 1994, chapter 5; 1998b) is not to simply posit that there is an external reality (or that there is a reality underlying the appearances observed in science) and then discuss what follows from this presumption. Rather, he attempts to ground his position in a transcendental argument, the premisses of which involve the very possibility of the existence of science. The argument is not an argument from the success of science (*an argument from optimistic induction?*); in fact it is quite the opposite. A primary consideration is the *transitory* nature of scientific knowledge: in order for the continual change in our perceptions and in science to be intelligible, he argues that the world *must*<sup>2</sup> be a particular way.

What many would consider to be the classic argument for realism, more commonly referred to as the “No Miracles Argument” than the *argument from optimistic induction*, comes in various forms, but is given an especially clear formulation by Putnam (1975, p. 73):

The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle. That terms in mature scientific theories typically refer..., that the theories accepted in a mature science are typically approximately true, that the same terms can refer to the same even when they occur in different theories—these statements are viewed not as necessary truths but as part of the only scientific explanation of success of science, and hence as part of any adequate description of science and its relation to its objects.

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<sup>2</sup> This is not the last time that I will use or quote the term *must*. For now, this refers to a force somewhat greater than intuitive plausibility and weaker than deductive certainty. I recognize that this is certainly not an adequate explication of the usage, so near the end of this section on TR I shall endeavor to explicate more precisely the force behind the term.

Smart (1963, p. 39) takes a similar tack:

Is it not odd that the phenomena of the world should be such as to make a purely instrumentalist theory true? On the other hand, if we interpret a theory in the realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there really are electrons, etc., this is just what we should expect.

Smart's (1963, pp. 8-12) approach here can be read as a *plausibility* argument in that the domain of the philosopher lies less in issues that are open to empirical scrutiny, and more in the clarification of conceptual issues. I think that the most charitable reading of CR is to likewise read it as argument that seeks to establish the plausibility of the realist position.

A primary difference between *no miracles* or *cosmic coincidences* arguments and that of CR is that the former are arguments from the success of science, while the latter is an argument from the existence of science. By not depending upon the success of scientific theories to the same degree as do the premisses of the *no miracles* argument, TR buttresses realist positions against arguments against arguments from pessimistic induction in a way not open to the former. Pessimistic Induction is the view that, when one considers the vastness of the graveyard of abandoned theories, believing that the theoretical postulates of current theories will not also go the way of the Ether and Phlogiston amounts to unwarranted optimism. Even if arguments from pessimistic induction succeed, TR still holds water.

The entire argument for transcendental scientific realism (“A genuinely original argument...strong, elaborate and well integrated, elegant and powerful...” Harré, 1976) revolves around the observation of a central paradox in science. The starting point of the argument, the state of the world that stands in need of accounting for, is that on one hand scientific knowledge is a *social product* (this is the *transitive* dimension of science), but at the same time scientific knowledge is (or intuitively ought to be) of *something that exists independent of our knowledge* of its existence (this is the *intransitive* dimension of science). More specifically

- (i) The *transitive* dimension of science is an epistemological notion— our knowledge of the world, and the tools by which scientists obtain and construct that knowledge. The transitive objects of knowledge are the materials out of which a science is constructed, theories, paradigms, models, methods. Here, *transitive* is not used in the relational sense (e.g. If I am taller than  $x$  and  $x$  is taller than  $y$ , it follows that I am taller than  $y$ ), rather, it is a recognition that, being counter-phenomenal or transphenomenal, scientific claims are fleeting or transitional. Though it is a moot point, *transitional* or *transitory* might have been the better choice of words here.
- (ii) The *intransitive* dimension is an ontological concept— it is the world itself, the objects of the knowledge. The intransitive objects of knowledge include the structures, mechanisms and processes that are referred to in the theories, paradigms hypotheses, models, etc.

The view is articulated by considering what the world *must* be like in order for commonly accepted scientific practices to be possible. In fact the entire structure of the CR theory of science is rooted in a single transcendental argument (the key foundational pages of A Realist Theory of Science, mentioned above (pp. 30-36)).

To attribute scientific significance to an event requires both experimental activity and perception of the event. For sense perception of an event to be intelligible, one must assume that the object of perception is intransitive, or else scientific change would be impossible, and observation would be moot. "If changing experience of objects is to be possible, objects must have a distinct being in space and time from the experiences of which they are the objects." (Bhaskar, 1998b, p. 23-25). Or as Harré (1986, p. 12) puts the point: "For there to be public reliability [in science] something must exist independently of whomsoever found it first."

For Kepler to see the rim of the earth drop away, while Tycho Brahe watches the sun rise, we must suppose that there is something that they both see (in different ways). Similarly when modern sailors refer to what ancient mariners called a sea serpent as a school of porpoises, we must suppose that there is something which they are describing in different ways. (Bhaskar, 1997, p. 31).

The intelligibility of experimental activity likewise presupposes the existence of an intransitive dimension. The purpose of experimentation is to allow a specific pattern of events to come to fruition that would not have occurred otherwise. The role of the experimenter is to bring about that pattern of events, whilst not being the mechanism or causal law (underlying the pattern) that is the object of interest. While this may appear to

be an uninteresting truism, Bhaskar notes two significant consequences that follow from it. (i) There must be an ontological distinction between the causal law or mechanism and the observed sequence of events, as the experimenter is a causal agent in the sequence of events, but not the causal law. (ii) Unless the mechanism/causal law identified can be said to hold outside of the experimentally closed circumstances, experimental activity can have no rationale. And realistically, other than in the case of astronomy, there is no such thing as a causally closed system outside of those artificially established in experimental environments (see also Cartwright, 1994).

Consider the purpose of the scientific experiment. If one wants to clearly understand the effect of the causal mechanism under consideration, it is necessary to isolate the cause and effect from any superfluous or extraneous variables that could interfere with an observation of the phenomena. Without this experimental control, one can never be certain that the results observed are in fact due to the postulated causal mechanism. In the absence of experimental control the best that the researcher can claim is that the results are due to the causal mechanism in conjunction with other factors. Ideally, experimental control allows the mechanism being tested to be tested in isolation by allowing the observation of a single mechanism of nature isolated from external causal mechanisms, so that the experimenter can see what the mechanism under question will do on its own. Bhaskar (1986, p. 35) sums up the purpose of the experiment as

an attempt to trigger or unleash a single kind of mechanism or process in relative isolation, free from the interfering flux of the open world, so as to observe its detailed workings or record its characteristic mode of effect and/or test some hypothesis about them.

This introduces a notion that is central to the CR philosophy of science: open vs. closed systems. Causal closure seems to be used slightly ambiguously, both generally and in the works of the Critical Realists. The term *causally closed system* could refer to a system in which it is possible to observe the effects of a single causal mechanism, or it could refer to one in which there is no influence of any causal mechanism external to the system. For the purposes of this paper, unless otherwise stated any reference to causal closure, or to closed systems, refers to the first sense of the term. Certainly no system is ever perfectly closed, with the possible exception of the universe as a whole if the latter meaning of closure is implied. However, the purpose of experimental control is to establish causal closure (in the first sense) to the greatest degree possible, or at the very least justify the application of a *ceteris paribus* clause. It might be said that the only instances in which Humean causality is actually obtained is when a causally closed system is established; outside of an artificially closed system superfluous variables make constant conjunctions impossible.

Bhaskar can be read as arguing against a rather narrow (though common and probably incorrect) construal of Hume. A more charitable reading of Bhaskar's take on Hume might be to interpret him as explicating how Hume could have stated his position in a more suitable probabilistic manner. Hume's argument is not meant to establish that constant conjunctions are sufficient grounds upon which we can infer a causal law, but that they are the only grounds upon which we can make attributions of causal laws. Surely Hume would have accepted that because of intervening variables conjunctions are going to be, at best, frequent. However, the position that he uses as a foil against which to draw out his position is that of constant conjunctions.

If science draws its conclusions from artificially established environments, would this not cast doubt on the validity of experimentally produced knowledge regarding its applicability or generalizability to a non-experimentally controlled environment (i.e. the real world)? Do experimental results tell us simply what happens in artificially established experimental conditions? A common question that I was asked (and that I in turn asked) when doing research in Social Psychology was the following: how can the results obtained in the laboratory be generalized to conditions external to the experiment? How, for example, could the answers to a questionnaire, given by undergraduate volunteers, sitting at a comfortable desk, with no aggressive stimuli, be generalized to how subjects would in fact react at a sports riot (Russell, Arms, & Mustonen, 1999)? While it is granted that the laboratory setting in this example is certainly artificial, it must also be asserted that artificiality is not what makes the circumstance experimental. The defining characteristic of this and other experiments is the attempt to obtain—through experimental control—causal closure. If the artificial situation fails to obtain an approximately closed system, it likewise fails in its claim to experimental significance. Compare the sports psychology example to the case of ethological data based on zoo studies. While both are artificial, the latter fails to obtain experimental closure in that, instead of eliminating extraneous influences, it introduces new factors such as isolation, captivity, and unnaturally close proximity to human observers.

Two of the measures of realism, mentioned above, are transphenomenality and counterphenomenality. These notions require that there be *depth* in the realist theory, in that there ought to be something that goes beyond the sense data. Bhaskar and the Critical Realists provide this depth in a number of ways.



First, in the first chapter of A Realist Theory of Science, Bhaskar posits three domains: the domain of the *empirical*, the domain of the *actual*, and the domain of the *real*.

To make the distinction between these domains clear, one must consider the distinction between the causal powers of a given thing and the exercise of those powers. Where Berkeley relegates the *things* experienced to the realm of the explanatorily unnecessary (leaving only the experiences themselves), there seems an immediate inability to account for such commonplace occurrences as finding a lost object, inferring what happened while asleep, or questioning one's memory regarding a perceptual event. Because events can occur unexperienced or inferred even empiricists will (even if implicitly) allow a level of reality that accounts for experience. These are the events caused by the powers of things.

My vehicle (on its better days) can drive in excess of 100 kilometers per hour. While it is parked I can still confidently boast that it has the power to do so, even if at the time the power is unexercised.

Given that events are caused by the powers of things, and given that such powers continue to exist even when unexercised, then after experience and events, there needs to be a third level of reality. How is it that, even at rest, my vehicle can be known to have the capacity to surpass 100 km/h? Because it is possible to predict powers from *structures*. My vehicle (my computer, my heart...) has the power to do what it does in virtue of its structure.

Then what we mean when we refer to a causal *mechanism* is the power, in virtue of the *structure* (particular configuration of particles, fields, forces) of an object, to bring

about a possible outcome. Causal mechanisms are known via causal inference. This should generate little ontological controversy as in daily life we are wont to do this without second thought: charred remains imply a fire and a burgled house implies a burglar. In experimentally controlled causally closed instances, when any extraneous causal influences have been eliminated, the researcher concludes the existence of a causal mechanism. It may be an open question as to what the causal mechanism is, but the observation of the effect insinuates that there is *some* causal mechanism in effect.

The domain of the empirical contains only that which is experienced. The domain of the actual includes that which is experienced, but goes further: in order to account for experiences, CR argues that events are the best explanation (there is further discussion of actualism below). The domain of the real allows for and recognizes the existence of the mechanisms that cause the events. “We now have a perfectly acceptable ontological basis for causal laws. For if it is wrong to reify causal laws, and it is wrong to reify causal mechanisms, it cannot be wrong to reify things!” (Bhaskar, 1997, p. 50). The structure of an entity is the set of properties by which it holds causal sway over the world. It would likely be a mistake to think of such structures as necessarily being similar to our everyday experience at the macro level (e.g. the above automobile example). “It is wrong to think of them as necessarily like material objects—they may be powers, forces, fields, or just complex structures or sets of relationships.”

As discussed above, realistically, causal closure only occurs when it is artificially established. Consequently, other than in experimentally controlled conditions, no causal mechanism can be said to operate unimpeded. Nothing ever really behaves precisely as the laws of nature predict that they should. *If* scientific laws refer to *constant*

conjunctions, they must refer to the relation between causal mechanism and effect not in natural conditions, but only when there is no interference from external influence. And the explanatory value of a scientific law lies in its *contribution* to an explanation of events in an open system in which the mechanism in question is but one amongst many.

A second way in which CR allows depth in reality is that, not only are multiple causal mechanisms posited, it is further contended that these mechanisms exist in *multiple strata* (Bhaskar, 1997, pp. 18-19).

The logic of scientific discovery can be summarized with breathtaking brevity: "...[1] a regularity is identified, [2] a plausible explanation for it is invented, and [3] the reality of the entities and processes postulated in the explanation is then checked." (Bhaskar, 1998b; p. 49).

The move from the first step to the second involves *creative model building*, in which plausible causal mechanisms are hypothesized.

And for transcendental realism, the move from (2) to (3) involves experimental production and control, in which the reality of the mechanisms postulated in the model are subjected to empirical scrutiny. For transcendental realism [,] that some real things and generative mechanisms *must* exist can be established by philosophical argument... [b]ut it is contingent and the job of substantive science to discover which...hypothetical or imagined mechanisms are not imaginary but real... (1998b; p. 50).

Given, as is argued above, that it is a mistake to hold that a constant (or even frequent) conjunction is sufficient grounds to claim a causal law, then can there be any

objective grounds for distinguishing between accidental generalizations and law-like necessary connections? The difference is that in the case of the law-like statement, but not the non law-like statement, there is a generative mechanism that can be cited as producing the correlations and connections in question.

Once explanatory mechanisms and structures are established as real, the next step involves explaining the actions of *those* mechanisms and structures. Plausible models and mechanisms are postulated, and empirically tested. Bhaskar (1998b, p. 66) offers the following example:

<i>Stratum I</i>	$2\text{Na}+2\text{HCl}=2\text{NaCl}+\text{H}_2$	
	explained by	
<i>Stratum II</i>	theory of atomic number and valency	Mechanism 1
	explained by	
<i>Stratum III</i>	theory of electrons and atomic structure	Mechanism 2
	explained by	
<i>Stratum IV</i>	{competing theories of subatomic structure}	Mechanism 3

and summarizes the process thus:

A general pattern of scientific activity emerges from this. When a stratum of reality has been adequately described the next step consists in the discovery of the mechanisms responsible for behavior at that level. The key move in this involves the postulation of hypothetical entities and mechanisms, whose reality can then be ascertained...Now the stratification of the world must be assumed by the scientist, working in any field, to be in principle unbounded. (1998b; pp. 66-67).

Again, the central role of the notion of a power becomes apparent. In the place of the causal laws posited by the positivist the transcendental realist postulates the tendencies of the generative mechanisms to be “powers which may be exercised without being fulfilled or actualized (as well as being fulfilled or actualized unperceived by men).” (1998b; p. 37). The ascription of a power is the assertion that a “thing will do (or suffer) something, under the appropriate conditions, in virtue of its nature.” (p. 70).

The ascription of a power is not the end point; it is followed by the construction of possible explanations. The qualitative description of the generative mechanism depends upon (i) practical work (experimental, technical) and (ii) theoretical work (the production of a concept of the object). If the entity cannot be perceived, does this mean that it cannot be known to exist? This is a conflation of two senses of theoretical—not directly perceivable vs. dubious existence; entities that are not directly perceivable can be known to exist indirectly by observation of their causal capacities (e.g. “the detection of radioactive materials by a Geiger counter, of electricity by an electroscope, of a magnetic field by a compass needle.” (1998b; p. 73)).

The idealist position (e.g. Berkeley) is that the very idea of unperceived entities is unintelligible. But the causal realist (including the transcendental realist) is going to allow reality or existence to anything that can have causal effect on the world. Hume may be right in that it is impossible to see the causal powers of an object—we do not see what is passed on from one billiard ball to the next. But we do see that there are powers in objects via observation of the effects. We can observe that certain structures are going to have certain capacities. We see that the billiard ball has powers that a cube lacks. At the

macro-level we see structures (mini-vans) with certain powers, and posit a continuity with objects at the unobservable level.

The above considerations suggest the existence of a philosophic ontology, that being the existence of an intransitive dimension—a reality independent of experience. Once committed to the intransitive dimension, it follows that ontology is prior (in being, though not necessarily in knowledge) to epistemology. Failure to recognize this leads to the *epistemic fallacy* (Bhaskar 1998, pp. 27-33; Searle, 1997, pp. 112-115, 122-123, holds a similar position). This is the view that we can reduce statements about being to statements about knowledge and that ontology is dependent upon epistemology (as in Kant's Transcendental Idealism). Hence the positivist can assert that a proposition not empirically verifiable or falsifiable is essentially meaningless. The epistemic fallacy is a conflation of the proposition with the grounds for making the proposition. As such it denies the possibility of a philosophic ontology—the *intransitive dimension* established by a simple transcendental argument that has the practices of science as its premiss.

Consequently, philosophies of science containing forms of the epistemic fallacy (e.g. logical positivism, verificationism) are inconsistent with the possibility of the practice of science. "Without a concept of a reality unknown, but at least in part knowable, philosophy could not display the creative and critical activity of science, and cease to be of any practical relevance for it. This was the price paid for the dissolution of ontology. A philosophy of science depends upon its reconstitution." (1998b; p. 33).

Empiricism and verification are made superfluous by the philosophic ontology of the intransitive dimension, and ascription of reality to a theoretical entity can be solely on the basis of its capacity to have a causal effect on the material world (1998, p. 12). The

Newtonian or Laplacean conception of the universe is replaced by a stratified one in which causal laws are better described as tendencies that may or may not be actualized or perceived (due to the lack of causal closure in the environment in which they operate).

An appreciation of the depth of transcendental realism could be gained by comparing it to a weaker variety of realism, that which Bhaskar (1997, chapter 2, p. 64) has labeled *actualism*. Actualism, though consistent with empiricism, is realist in the sense that it accepts that things exist and events happen, but fails to accept the existence of that which underlies the appearances of the phenomena in question. Following a narrow interpretation of Hume, the actualist finds cause and effect only at the level of events, relegating *theoretical entities* to the realm of metaphor and simile by describing them as useful fictions or explanatory constructs (e.g. Stace, 1967; van Fraassen, 1980 suggests that a less than literal construal of scientific discourse is desirable). A deeper realism, such as that prescribed by CR holds that things have the causal capacities that they do in virtue of their *structures*. TR is transphenomenal because it ascribes such causal powers regardless of whether the powers are being exercised. Under the right conditions, an aircraft has the power to fly, and it retains that power by virtue of its structure even when not in flight. "...[I]f it is wrong to reify causal laws, and it is wrong to reify generative mechanisms, it cannot be wrong to reify things." (Bhaskar, 1997, p. 50). The structure of an entity is the properties by which it holds causal sway over the world.

Collier (1994, pp. 8-11) quotes the following passage from Nietzsche (1969, p. 45) in order to illustrate how actualism tends, if applied consistently, to negate the possibility of unexercised powers.

A quantum of force is equivalent to a quantum of drive, will, effect — more, it is nothing other than precisely this very driving, willing, effecting, and only owing to the seduction of language (and of the fundamental errors of reason that are petrified in it) which conceives and misconceives all effects as conditioned by something that causes effects, by a ‘subject’, can it appear otherwise. For just as the popular mind separates the lightning from its flash and takes the latter for an *action*, for the operation of a subject called lightning, so popular morality also separates strength from expressions of strength, as if there were a neutral substratum behind the strong man, which was *free* to express strength or not to do so. But there is no such substratum; there is no ‘being’ behind doing, effecting becoming; ‘the doer’ is merely a fiction added to the deed — the deed is everything. The popular mind in fact doubles the deed; when it sees the lightning flash, it is the deed of a deed: it posits the same event first as cause and then a second time as its effect. Scientists do no better when they say ‘force moves’, ‘force causes’, and the like — its coolness, its freedom from emotion notwithstanding, our entire science still lies under the misleading influence of the little changeling, the ‘subject’ (the atom, for example, is such a changeling...).

This “changeling” (the subject) that Nietzsche tries to make sound so implausible, is any individual that is causally efficacious by virtue of its internal structure. Were he correct objects would not bring about effects because of the way that they are. If correct,



the appearances of the event is as deep as reality goes, and there are no causal powers in the objects of study to account for the events; there can be no unexercised powers.

It is perhaps unfair to attribute to the “popular mind” the belief that an action exists before it happens. If one were asked “what caused that sudden flash of light?” an immediate answer might be “the lightning”, but if a follow-up question such as “are you saying that the lightning and the flash of light are two separate things?” were asked, the answer would likely be in the negative. The answer to the first query is, as Nietzsche noted, an accident of language, not an accurate reflection of meaning. An accident does not exist, then occur; a debate does not first exist, then happen. But an icy road and bald tires exist prior to an accident, and debaters and their ideas exist prior to a debate. The notion of the unexercised power is difficult to maintain only if one cannot differentiate between *can* and *does*.

Bhaskar (1989, p. 4), using the example of social structures, counters the actualist position thus:

All social structures — for instance the economy, the state, the family, language — depend upon or presuppose social relations — which may include the social relations between capital and labour, ministers and civil servants, parents and children. The relations into which people enter pre-exist the individuals who enter into them, and whose activity reproduces or transforms them; so they are themselves structures. And it is to these structures of social relations that realism directs our attention — both as the explanatory key to understanding social events and trends and as the

focus of social activity aimed at the self-emancipation of the exploited and the oppressed.

Within social systems at least, the shallow realism of actualism denies any difference between an actual change in the structures that dictate the range of possible events and a simple change in events. But certain sorts of events or changes will always be precluded by certain sorts of structures. Returning to Bhaskar's example of social structures, it could be argued that the structure of a free market economy precludes the possibility of full employment and environmental responsibility.

An actualist theory of science is shallower, and hence less realist than TR in that it denies that the phenomena of interest are transphenomenal. And it follows that if transphenomenality is denied, counter-phenomenality is denied with it.

A potential criticism of the realist theories of science is that their claims to truth are dogmatic, and in the face of the observation that all theories are apparently abandoned or modified, claims of objective truth are simply unwarranted. "Surely", the anti-realist would argue, "considering the appalling track record of every scientific theory that has gone before, why would one be willing to consider current theories true?" CR does indeed accept a correspondence version of truth, in that it accepts that the facts that make a statement true or false are independent of that statement. But would anybody, realist or otherwise, hold to the position that a claim of the status of "scientific theory" is a claim to infallibility? The history of science has demonstrated conclusively that science is inescapably, inherently, fallibilistic.

Surely this criticism of dogmatism is pointed in the wrong direction. To claim that one's theory is objectively true is to open it up to the possibility of being refuted. If the

theorist allows that the statements of the theory refer to something external to the theory, it is possible (at least in principle) for researchers to examine that something and determine if it is in fact as the statements of the theory indicate. Would not the attack of dogmatic claims of infallibility be as valid, if not more so, against the anti-realist? If a theory does not admit to referring to anything external to the discourse (as exemplified in Feyerabend, 1970, 1975), would it not be invulnerable to criticisms that the facts are different than are claimed by the theory?

Before ending this summary of the CR theory of science, it is worthwhile taking another look at the idea of causal closure and what follows from it.

It could be said that the actual work of experimentation (artificial causal closure) is to bring about an artificial alignment between the domains of the empirical, the actual, and the real. The work of science is an active intervention into the real world, an attempt to actualize and observe the workings of a causal mechanism.

Having made domains congruent in order to empiricize the real, i.e. having observed the intransitive dimension, the role of the theorist is to transform the transitive dimension. The theorist takes the existing theories, paradigms, models, etc., and using data derived from the experimental actualization of the mechanism of interest, corrects, expands, or otherwise develops existing knowledge into a more adequate accounting of the domain of the real.

Popper's (e.g. 1963) criterion of falsifiability can be described (following Bacon) as a method for putting questions to nature and allowing nature to supply the answer. The CR theory of science shares this aim with Popper. Popper, believing that scientific

discourse indeed refers to an external reality, allows that the way reality *is, must* be the arbiter of the correctness of scientific knowledge.

There are elements of the Critical Realist theory for science that seem, at least *prima facie*, incompatible with the existing state of knowledge in some areas of science.

The first concern is the idea that realism is somehow dependent upon the independence of objects of study from their perceivers. This seems to break down in (i) psychology and (ii) quantum mechanics in which the boundaries between the transitive and intransitive dimensions blurs.

- (i) Psychology. Is it possible to undertake a scientific study of one's own mental states and events? CR apparently does not allow for this possibility.

Introspectionism was initially rejected on the grounds that there seems to be no possibility of a distinction between the perceiver and the perceived. If so, then the very act of introspecting is changing the mind so that the only thing studied by introspection is the act of introspection itself. Certainly introspection can offer knowledge regarding one's own mental states and events, but I suspect that if a scientific study of the mind is possible (and it ought to be because minds exist), introspectionism isn't the way it ought to go. The problem could be along the lines of asking how the one thing that we know better than anything else cannot be subject to scientific scrutiny.

Goodman (1951, pp.97-98) is, I think, on the right track by suggesting that we consider judgements of our own mental states and events to be decrees rather than knowledge claims. This sounds like an interesting approach to resolving the paradox, but it will not be pursued here.

- (ii) Quantum Mechanics. According to one (influential) interpretation of quantum phenomena, the very act of observing a subatomic particle influences the nature of the particle; the nature of the inquiry determines the characteristics of what is observed. Now, most physicists would not argue that physical reality would not exist if there were no minds to perceive it. Therefore there does seem to be a mind-independent object of study. The fact that in quantum events the act of perception or measurement determines the characteristics of the perceived could be considered one of the phenomena that should be accounted for in any quantum theory; it is part of the world, not caused by our representations (beliefs, theories, statements) of it. As one can study the human eye by use of the eye, so too can one use the mind or use perception to understand quantum phenomena involving human perception.

The second concern is that TR requires that objects have a distinct being in time and space; in quantum mechanics it is plausible that particles do not have a position until some localizing interaction occurs. However, if these particles do exist independently of observers, then there is *some* way that they are. Either that way is describable in terms of time and space, or there are other categories akin to time and space. It might be the case that humankind is limited in our epistemic capabilities in such a way that makes it impossible to adequately conceptualize how they are. Still, there is *a way that they are*—and this could be read in at least two ways. First, we might be interested in the way that the particles are prior to or independent of observation or measurement. Second, we might be interested in the observed properties (what does it mean to say that characteristics do not exist or do not exist fully until observed?). If observation and

measurement cause an object to be a certain way, then there is still a mind-independent phenomenon to observe—whether it is the newly reified characteristics of the particle, or the interaction of measurement and particle.

#### Epistemological Concerns

Before moving on to the next chapter, there is a serious and potentially debilitating argument that can be and is used to undermine arguments for realism. It can be contended that arguments for realism, including the CR version of realism, are circular. One way of clarifying exactly what is intended by this criticism, and perhaps how one might answer it, is to consider the frequent use of the word *must* in the preceding paragraphs, and to decide just what force lies behind the term. For example, I have stated or quoted above...

“in order for the continual change in our perceptions and in science to be intelligible, the world *must* be a particular way.”

“The view is articulated by considering what the world *must* be like in order for commonly accepted scientific practices to be possible.”

(Harré, italics added) “For there to be public reliability [in science] something *must* exist independently of whomsoever found it first.”

(Bhaskar, italics added) “we *must* suppose that there is something which they are describing in different ways.”

(Bhaskar, italics in original) “...that some real things and generative mechanisms *must* exist can be established by philosophical argument...”

(Following Popper) “...the way reality *is* must be the arbiter of the correctness of scientific knowledge.”

Now, is not the insertion of the word *must* into the above passages merely question begging? Surely (the idealist is going to counter) the inclusion of the *must* is little more than a plea to *common sense*, or for the reader to examine the things one believes he knows to be true.

I recall sitting in an undergraduate class on either epistemology or philosophy of the 17<sup>th</sup> and 18<sup>th</sup> centuries, as the instructor explained why he felt idealism to be unconvincing. Professor Brown offered two reasons that struck me as intuitively compelling, though hardly logically compelling. First he suggested an argument similar to one mentioned in passing in this essay—that idealism had difficulty in accounting for certain common occurrences such as finding a lost object. He then suggested that the reason that he was not convinced by idealism was the complexity of the phenomena experienced—i.e. that one could experience phenomena from virtually every conceivable perspective.

In the same course we considered G. E. Moore's (1959) "A Defense of Common Sense" and his famous "proof" of objects external to the mind. Moore's proof amounted to acknowledging that we *know* of things of the kind that can be met in space, and from this it follows that there exists objects that are external to our minds.

My reaction to each of these arguments for external realism was twofold. First, they accurately captured my intuitions. *Of course* I *know* that when I hold my hand up in front of my face it is external to my mind. On the other hand, the arguments are not in the least logically compelling. To put it simply, *if* idealism is correct, then this is exactly how one should expect the *world* to be.

The only way to be convinced by Moore's "proof" is to have already accepted the truth of the proposition that there exists the sort of things that can be met in space—and *things* and *space* require a prior acceptance of external reality. The acceptance of Moore's realism depends upon the prior acceptance of realism. If we are to read Bhaskar's TR argument as a *proof* of external realism it too is open to accusations of question begging.

But an approach like that of Moore would not be considered unreasonable in some approaches to epistemology. Chisholm (1982, italics are mine), for example, suggests that "[w]hat few philosophers have had the courage to recognize is this: we can deal with the problem [of what we know] *only by begging the question*...it is unseemly for us to try to pretend that it isn't so...And in favor of our approach is the fact that we *do* know many things, after all." In a chapter that aims to establish external realism as a rational position, Searle bluntly states "I do not believe there could be a non-question-begging argument for ER." (Searle, 1995, p. 184).

However, such views seem to rest upon a rather restrictive version of what will count as knowledge. Traditionally, if one wants to hold a belief claim to be a knowledge claim, one must be able to account for why a given belief is in fact knowledge. Epistemologies such as those given above fail on this count. Similar views might contend that that in order for a belief to be knowledge it must be formed according to reliable belief forming procedures, though it is not necessary to identify what those procedures are (e.g. Goldman, 1979). The implication is, that if one wishes to avoid begging the question, adjudication between beliefs formed by reliable processes and those not formed



by reliable processes is not possible. Perhaps this is why only the “courageous” philosophers can accept such an epistemology.

It appears that if the aim is a non-circular certainty in the realist-idealist debate, the debate will consistently run into the same impasse. When a data set can support conflicting conclusions, one seeks a crucial experiment. To what sort of crucial experiment could we turn in order to resolve the idealist-realist question? In this case there is no final arbiter that will allow a satisfactory adjudication because both philosophies will be able to account for all conceivable observations on their own terms.

Malcolm (1952) distinguishes between two types of knowledge. The first is what most (at least most not familiar with epistemology) presume to be knowledge—those beliefs, the truth of which cannot be doubted; it is our certainty of things such that (we think) no evidence could overturn the belief. A second, less restrictive, description of knowledge concerns those things that we feel fairly certain to be true, though perhaps not with a deductive certitude; beliefs that one can revise in the light of new information or observations counts as knowledge in this sense. Those unschooled in epistemology are likely to consider their knowledge claims to be of the former type, while those who have attempted to examine the foundations of their beliefs discover the former to be extremely rare.

In the case of the idealist-realist debate, the impossibility of an external meta-language or crucial experiment makes knowledge in Malcom’s strong sense impossible. Here we must face facts. If Bhaskar’s Transcendental argument is intended as a proof of external realism, he has attempted to prove something that cannot be proven with

absolute logical certainty. He is trying to establish something that can only be established by the use of circular reasoning.

But as mentioned above, regardless of how much stock Bhaskar and his disciples put into transcendental arguments, I think that the most charitable reading of TR is as an explication of our realist intuitions. In such a reading we are trying to buttress the realist positions against competing veins of thought by arguing for its plausibility.

So perhaps the *must* in question ought to be understood in terms of the weaker sense of what constitutes knowledge. If a practicing scientist were to run into a predicament of the same form as the idealism-realism debate—and by the same form I mean one in which no crucial experiment is forthcoming—how would he adjudicate between hypotheses? Would the scientist not refer to less empirical criteria—for example simplicity, comprehensiveness, and number of ad hoc addenda? Now if the idealists will allow that such a process of reasoning is rational in the case of choosing between equally confirmed hypotheses, can we turn the tables on the non-realist and argue that applying a similar train of thought allows one to weigh the respective virtues of idealism and realism, and come down on the side of realism?

Such is the view of Maxwell (1970, p. 12): “[A]s our theoretical knowledge increases in scope and power, the competitors of realism become more and more convoluted and ad hoc and explain less than realism.”

Given that there is no absolute adjudication between idealism and realism (in that both are equally *confirmed* by the evidence) one might apply a Bayesian approach akin to that of the scientist. The probability of idealism may be the same as that of realism, but surely few will disagree that realism has considerably greater *prior* probability. Given

that rival theories are equally confirmed, each is weighted according to beliefs as held prior to or independent of these theories. It is hardly worth pointing out that in the absence of fairly sophisticated philosophical arguments, few are likely to believe that all experience (including other minds) exists only in the mind of the perceiver.

Let us now return the discussion to the usage of the term *must*. We have to cede that there is no crucial experiment that can decide once and for all what *must* be the case, what *must* be the way the world is in order to account for the possibility of science, but this does not entail ceding the impossibility of a rational choice between idealism and realism. I think that like Moore's (1959) defense of common sense and Smart's (1963) cosmic coincidences argument, the TR theory of science is best read not as a knock down proof of realism, but as a an argument that tries to make explicit the plausibility of the realist position.

As such, are the standards not internal coherence, intuitive plausibility, prior probability, lack of ad hoc hypotheses, and simplicity? By this standard the possibility of finding a lost object or, more generally, of counterphenomenality intuitively necessitates that there *must* be a mind independent reality. "Without a concept of a reality unknown, but at least in part knowable, philosophy could not display the creative and critical activity of science, and cease to be of any practical relevance for it." (Bhaskar, 1998b; p. 33). The requirement that reality can be different from appearances, to some minds this is *proof* enough that there must be a mind independent reality.

If a colony of scientists and philosophers with a realist bent were to be visited by an alien anthropologist from a planet where idealism is considered the common sense view and realism has not been conceived of, what difficulty would the anthropologist

have in comprehending the realist view? Perhaps, no matter how internally coherent, comprehensive, and rational the realist position is, the anthropologist could always counter “yes, but all that you say can be equally accounted for by idealism.” The human western anthropologist who seeks to understand non-western culture cannot do so until he understands (i) from the perspective of those involved in that particular way of life (ii) by understanding the entire web of concepts, meanings, and practices, and the relations between these. Likewise, one whose web of meanings and concepts is shaped by an idealistic perspective may never be convinced by the elegance, simplicity, coherence, etc. of realism. I don’t think that it counts as a strike against realism to admit that it can only be accepted “from the inside.”

Scientists operate within a set of assumptions, often unspoken. Bhaskar’s transcendental argument is an attempt to make those assumptions explicit. Searle (1995, pp. 182-183) offers a “Wittgenstinian” example:

You cannot infer from

$$\sim(\exists x) (\text{money } x \ \& \ \text{in my wallet } x)$$

*(It is not the case that there is some such x that x is money and x is in my wallet.)*

to

$$(\exists x) (\text{money } x)$$

*(There is some such x such that x is money.)*

But all the same the original utterance only makes the kind of sense it does, we only understand it the way we do, against the presupposition of the existence of money. It has its sense against a space of possibilities of

having money. External Realism articulates a space of possibilities for a very large number of statements

In conclusion, my view is that Bhaskar has overstated his case. The “proof” is only such upon the condition of the prior acceptance of realism. In one sense I think Bhaskar succeeds. In the sense that Searle suggests that “ $\sim(\exists x)$  (money  $x$  & in my wallet  $x$ )--(*It is not the case that there is some such  $x$  that  $x$  is money and  $x$  is in my wallet.*)” only makes sense against a particular set of assumptions, TR suggests that our experience of science is plausible only against a set of realist presumptions.

## Central Tenets of Transcendental Realism.

### Stratification and Emergence

As discussed above, the CR theory of science contends that nature is stratified. CR further holds that the study of nature, science, ought therefore be likewise stratified in a manner that reflects nature. Although science is a social artifact, there are aspects of nature that necessitate the sciences be divided, albeit roughly, along particular lines. Though it *might* be the case that in some respect every phenomenon can be given a description under physics, only a subset of those phenomena can be studied under chemistry, a subset of those under biology, and a subset of those under psychology.

The relationship between strata is for the most part one way. Higher order strata are governed by the laws that define activity in that realm, and are constrained by the laws of lower order strata. A human is constrained by the laws of psychology, but is also subject to the laws of biology, chemistry and physics. But the reverse does not hold. Not all chemical substances are part of living organisms (so are not subject to laws of biology), and we do not attribute a psyche to all living matter (so all life is not subject to the laws of psychology).

This does not mean that members of higher order strata are able to act contrary to the laws that belong to the strata that underpin their own. An organism is not able to break the laws of chemistry, “[a]s an animal it has got *active* powers to do what, as a conglomeration of chemicals, it has only got a *passive* power to ‘do’”(Collier, 1994, p. 108, italics added). The member of the higher order stratum will therefore be constrained by more kinds of causal mechanisms than the member of the lower order stratum.

The importance of experimental activity in natural science,..., allows us to stress that the predicates 'natural', 'social', 'human', 'physical', 'chemical', 'aerodynamical', 'biological', 'economic', etc. ought not to be regarded as differentiating distinct kinds of *events*, but as differentiating distinct kinds of *mechanisms*. For in the generation of an open-systemic event several of these predicates may be simultaneously applicable.

(Bhaskar, 1997, p. 119)

The reason for the term "constrained" rather than "caused" or "influenced" in the above description of the causal interaction between strata is that, when the description goes no further than the domain of the actual, there is causal interaction in the direction from higher order to lower order strata. Although, for example, laws of human nature do not constrain the chemistry of the individual, one's chemistry is continually influenced by the activity (eating, drinking, exercise) of the individual. The biology of a tree will determine its height and the density of its leaves, and this will influence any number of factors in its environment.

Now as Bhaskar and the Critical Realists do not draw their conclusions from premisses drawn from specific sciences or scientific practices (rather, from the existence of science, scientific practice, and perception), the *mechanisms* that CR refers to are not specific objects or events but abstractions. Here I think there is an ambiguity in Bhaskar's writings. *Mechanism* can be read as inhabiting the transitive or intransitive dimension. The most consistent reading comes when we read it as an intransitive construct. A *mechanism* is a tendency belonging to a particular type of thing, and operates where any member of that class of thing is (Lawson, 1997, pp.199-237; Sayer, 1981). So when a

realist claims to offer an explanation, she means this in two senses. In a theoretical sense, the realist claims that (as in the passage quoted above, from Bhaskar, 1998b, p.66) when one has a knowledge of a theory describing one stratum, then knowledge of a theory describing the strata/stratum that underpin it will enrich understanding of the first. But the realist is contending more than just a relation between the theories. The realist theorist is suggesting that the theory refers to *existing* causal mechanisms, and that the relation between the theories describes a relation between causal mechanisms. When one claims that a theory regarding one (lower) stratum *explains* theoretical statements regarding a higher stratum, one is not making a claim to the sort of explanation that connotes an efficient cause; the mechanisms are in different strata, and are simultaneous, so the explanation posited is a *vertical* explanation.

So when a higher order mechanism is explained in the terms of mechanisms belonging to a more basic stratum, does the higher order mechanism cease to be of scientific interest? Is it, so to speak, *explained away*? Bhaskar argues (1997, pp. 105-118, 163-185) that this is not the case. Historically theorists have never been able to predict higher stratum mechanisms from knowledge of the more basic stratum. Mental states and events could be considered the paradigmatic case here. Once the higher stratum phenomena are discovered and elaborated upon, they become the explanans, and the lower stratum phenomena the explananda. If the theorist were to only consider a mechanism fully explained when the mechanisms that underpin it are fully explained, this would set in place an unreasonable regress, and the theorist would never consider a phenomenon fully explained. Even if a phenomenon could be fully explicated in terms of a *rock bottom* stratum, would not there always be a lingering fear that the phenomena of



that stratum could be redescribed in terms of some as yet undiscovered more basic sphere?

Perhaps explicability in terms of more basic stratum should be considered a virtue? A mechanism that cannot be explained in terms of a more basic stratum seems *prima facie* less scientific than one that is conducive to such an explanation.

Bhaskar (1997, p. 113) differentiates between *emergence theories* and *reductionism* by describing the relationship of higher strata mechanisms to the more basic stratum as the first being rooted in the latter. If a theory is *rooted* in, as opposed to being reducible to, a more basic theoretical framework, it does not deny the existence of the theoretical entities postulated by it; it recognizes that there are complex aspects of the world (again, mind could be considered the paradigmatic case) that are irreducible in that they cannot be adequately described solely in terms of the mechanisms of the less complex stratum.

It follows from this that the operations of the higher level cannot be accounted for solely by the laws governing the lower-order level in which in which we might say the higher order level is 'rooted' and from which we might say it was 'emergent'. Now an historical explanation of how a new level came to be formed would not, it is important to see, undermine this principle. Let us suppose that we could explain the emergence of organic life in terms of the physical and chemical elements out of which organic things were formed and perhaps even reproduce this process in the laboratory. Now would biologists lose their object of inquiry? Would living things cease to be real?...No, for in as much as living things were

capable of acting back on the materials out of which they were formed, biology would not be otiose. For a knowledge of biological structures and principles would still be necessary to account for any determinate state of the physical world. *Whatever is capable of producing a physical effect is real and a proper object of scientific study.* It would be the task of biologists to investigate the causal powers of living things in virtue of the exercise of which inter alia they brought about various determinate states of the physical world...Sentience determines the conditions of applicability of physical laws, but is also subject to them. If the elements of the lower-order are real then so must be the causes that determine the conditions of their operation, i.e. the comprehensive entities formed out of them. (1997, p. 113, italics added).

The difference between a world in which there are living things and one in which there are none could not be described in the absence of a vocabulary including the postulates of biological theory.

Bhaskar suggests that there are at least three senses of *reduction* (1997, pp. 114-116).

- (i) There might be a “lower order or microscopic” stratum that is the foundation for a higher stratum property or power.
- (ii) It might be possible to “*explain* principles of the higher-order science in terms of the lower one.” In order for this sense of reduction to work there must be a possibility of (at least a partial) translation of theoretical terms between the strata.

- (iii) It might be possible to make a prediction, based upon knowledge of a more basic stratum, regarding characteristics of objects of study of the higher order science

A strong case can be made against (iii) the actualist account (in this sense it is the view that there can be an account of the world in which all phenomena can be reduced to a single basic stratum; an account in which actualism would hold) of a scientific world view. As mentioned before, the empirical case is that there are apparently no cases in which such a prediction has been made. But of course this ought not negate that such predictions might in principle be possible.

However, there is what I think to be a better argument (Bhaskar, 1997, p. 116): science, a *social* activity, is itself a member of a higher order stratum:

The only way of reconciling experimental activity with the empiricist notion of law is to regard it [science qua social activity] as an illusion; that is, to regard actions performed in it as subsumable in principle under a complete atomistic state description. In principle this applies not only to experimental activity but to all scientific activity (including theory construction) in as much as it involves physical effects. Now this has the absurd consequence that the apparent discovery of natural laws depends upon the prior reduction of social to natural science. Or to put it another way, in an actualist world there would be no way of discovering laws which did not already presuppose a knowledge of them. (1997, p. 116).

How might the reductionist reply to the CR arguments against eliminative reductionism? She might claim that if theorist (I) claims that in stratum (i) mechanism *A*

causes effect *B*, and theorist (II) claims that in a more basic stratum (ii) mechanism *A'* causes effect *B'*, that the theory postulated by (I) is just a loose way of saying what is said by (II). When a psychologist claims that a theoretical postulate such as *attitude* causes a particular social behavior, can the neuro-behaviorist not just as accurately redescribe the relationship along the lines that some particular groups of neurons (perhaps some combination of frontal cortex—for planning and the limbic system—for motivation), relays instructions to the motor cortex, causing a particular pattern of muscle contractions?

Rorty (1980, p. 354) suggests that “Physicalism is probably right in saying that we shall someday be able ‘in principle’ to predict every movement in a persons body (including those of his larynx and his writing hand) by reference to microstructures within his body.” Bhaskar (1991, p. 48 (cited in Collier, 1994, pp. 114-115) offers a counter example (the *Guardian* and the *Independent* are UK newspapers): “Suppose A goes into a newsagent’s and says to proprietor B, ‘the *Guardian* please’, and B hands him a copy of it.” Rorty’s optimistic physicalism requires B handing A the *Guardian*... “even if A had performed some quite different action, such as asking for the *Independent* or for a packet of chewing gum or B to marry him or dancing a jig, and even if A had not been present at all.” The inference Bhaskar draws from considering this absurdity is that one’s neurophysiology is not a closed system. Not only is it causally influenced by interaction with others, “the social and semantic nature of the interaction between A and B is irreducibly what causes the action. No physicalistic description of sound waves proceeding from A’s vocal cords to B’s eardrums would explain how A got the *Guardian*.” (Collier, 1994, p. 115).

While this is certainly an interesting and illuminating example, it is by no means clear that it is a strong argument for rootedness. It is, *I think*, intuitively compelling, but granted, my intuitions already lean this way. Somebody whose intuitions are in the direction of explanatory pluralism could cite the example as evidence that multiple explanations for a single event do not necessarily have to map perfectly on to one another (e.g. Pollock, 1988; Kim, 1990; Cartwright, 1999).

Now could chemistry be reduced to physics in the absence of a well articulated and established body of chemical knowledge? “[E]very historically successful reduction of one science to another has depended upon the *prior* existence of an established corpus of scientific principles and laws in the domain of the reduced science.” (Bhaskar, 1997, p. 116). No translation can be coherent if the original formulation is not coherent. Collier (p. 115) sums up the idea pithily: “For we are not fluent in neurophysiologese, and can only explain social interaction in its terms by translating it into social-interactionese.”

Description of a mechanism at any stratum will presuppose the existence of any number of more basic strata. But it will not be rooted in all strata that it presupposes. Economics, for example, is rooted in society, and possibly biology (if we are to take seriously the more extreme arguments of sociobiologists, e.g. Lumsden and Wilson, 1981). A mechanism will almost certainly be rooted in no more than two levels, and usually no more than one.

This rootedness-emergence relation offers a realistic alternative to two equally plausible and influential trains of thought. Atomism holds that in order to be properly understood, reality must be reduced to its most basic components; holism is the polar opposite, holding that the parts of reality can only be understood in terms that which it is

a whole. In the sense argued against by transcendental realism, both of these methodologies are reductive in that they both fail to recognize the autonomy of, or the scientific validity of the study of, any stratum except the most basic or the most whole, potentially leading to an unbounded regress. On the other hand, when reduction and holism are replaced with rootedness and emergence, science allows for significant irreducible wholes such as molecular, biological, societal, economic or political entities.

Strata can be considered in a hierarchy in terms of causal closure; the higher one goes on the rootedness-emergence scale, the more difficult it will be to obtain the experimental control necessary to establish causal closure. What would causal closure even mean when the object of study is an economy; or even worse, what of evolutionary psychology? So the higher the strata, the more improbable the possible approximations to a causally closed system. So here's the question: How is science even possible, and what would it look like when describing members of higher order strata?

Bhaskar offers a picture of what explanation would like in an open system (1997, pp. 118-126). In an open system, an event is going to be caused by a number of mechanisms, so an explanation of the *transitive verb* variety is going to be lacking, as is displayed in the following example (on p. 123, from Pelling, 1960, p. 77. The italicization of the transitive verbs is Bhaskar's)

This *pressure* from the Labour Party, with its great *influence* on the industrial workers, combined with the attitude of President Wilson himself, slowly *propelled* Lloyd George in the *direction* of the formulation of war aims. *Hindered* as he was by the obligation of earlier agreements with the European allies, he *ensured* that his declaration, *made* on the 5<sup>th</sup>

January 1918, was only in the vaguest terms. It was, however, not incompatible the more specific Fourteen Points enunciated independently by the American President a few days later, and appealed to by the German Government as a basis for peace negotiations at the time of the armistice in November.

Bhaskar recommends explanation in an open system be modified into a four stage process:

- (i) Resolution: the event or process is resolved into its component causal factors.
- (ii) Redescription: causal factors can be redescribed with reference to the mechanisms operating in that open system. This presupposes a pure (as opposed to applied) scientific/theoretical description of these mechanisms.
- (iii) Retrodict: Having redescribed the causal components in the appropriate theoretical terms, one must look at the possible the causes of these components.
- (iv) Eliminate: finally eliminate as many of these possible antecedent events as can be eliminate by means of independent evidence about these antecedent events.

#### Contra Empiricism

It is easy to see the attraction of empiricism, and see how it could have played the liberating role that it did. Starting with a clean slate and allowing nature to write on it allows one to *think for oneself* as opposed to taking the past authorities at face value.

Thinking for oneself, though a noble idea, is not likely to be a productive pursuit until one has become accustomed to the skills of critical thinking and aware of the issues to be thought about by “thinking other people’s thoughts after them...” (Collier, 1994, p. 71); the empiricists did not recognize that in order to learn from nature, one must learn from others how to learn from nature. Collier contends (p. 72) that empiricism consequently is too radical *and* too conservative.

Too radical in that it may undervalue old authorities (as Aristotle was undervalued in the Enlightenment). But too conservative in a much more fundamental way: that it makes one uncritical towards one’s own experience, and towards one’s own concept of experience... It does not recognize that what we experience is determined not just by what is there, but by what we have already learned.

Berkeley’s account of perception takes the example of vision as an adequate paradigm upon which to build his model for the acquisition of knowledge. He makes the presumption that observation is passive, not looking *at* or looking *for* (for example). If knowledge is obtained by the passive observation of colored shapes, this leaves a large body of knowledge unaccounted for, knowledge, for example, of a long practiced and well learned skill.

Berkeley’s philosophy is emblematic of the general empiricist case. Collier (p. 73) characterizes the empiricist concept of experience as follows:

- (i) it comes through the senses;
- (ii) it is passively imprinted by nature;
- (iii) that on which imprinting occurs is otherwise a blank page;



- (iv) one's experience can be entirely understood without reference to the experience of others (it is *individualistic*);
- (v) each experience is related only externally to each other experience, so individual experiences can be understood without reference to one another (it is *atomistic*).

The empiricist account of the acquisition of knowledge does not suffice as an account of how scientific knowledge accumulates as it would consider scientific progress to be the amassing of sense impressions. But understanding scientific knowledge as amassed sense impressions does not explain (i) why some data are scientifically significant while some are not, or (ii) why there could be a need for experimentation rather than simply observation (similarly, why would scientists ever need scientific training?).

Hume's account of causation is perfectly coherent if one assumes the empiricist account of knowledge. If one presumes that *all* ideas are derived from impressions, then in order to understand causal necessity "we must find some impression that gives rise to the idea of necessity, if we assert that we really have such an idea." (*A Treatise on Human Nature*, XXXX). The idea cannot come from a simple succession of impressions, as all experience is a succession of impressions (*An Enquiry Concerning Human Understanding*, Section IV, Part I). Hume finds the source of "several instances in which the same objects are always conjoined together," when "we immediately conceive a connection between them, and begin to draw an inference from one to another."

Aside from the transcendental realist argument against this notion, intuitively, Hume seems to have failed to account for a wide range of causal judgements. Often a

single observation of a conjunction is enough for one to draw a causal conclusion (an *on* button, for example), and conversely, there are times when a very large number of observations of a particular conjunction will not lead one to draw the Humean inference (few believe the crowing of the cock to cause the sun to rise, for example).

Now, assuming that Bhaskar's transcendental arguments are correct, Collier (1994, p. 75-79) summarizes the ways in which CR negates the possibility of a Humean foundation for scientific knowledge (i.e. Empiricism).

- (i) That experimentation is necessary in order to isolate the objects of study shows that scientific knowledge cannot be accounted for solely in terms of nature imprinting knowledge on a blank slate.
- (ii) Empiricism cannot distinguish epistemically significant experience from non-significant experience. Without this distinction scientific knowledge is little more than a function of accumulated experience.
- (iii) The observation of conjunctive events is an insufficient ground for a theory of causation as the world of our experience is not a causally closed system, so constant conjunctions are not likely to occur outside of experimentally controlled situations. Causation therefore cannot consist solely in the relations between successive events.
- (iv) That science is a social process (knowledge is obtained by persons, often working in institutions, passing on knowledge and skills to successive generations of scientists) counters the idea that knowledge is an individual possession and pursuit.

- (v) The only entities so far observed that are capable of obtaining scientific knowledge are human minds. As the human brain shares the characteristic of being composed of matter with all things composed of matter, the fact that minds can obtain scientific knowledge suggests that there is something about them that makes this acquisition possible. The acquisition of knowledge implies that the mind is not a blank slate.

### The Epistemic Fallacy

An enduring legacy of empiricist thinking is what Bhaskar describes and names “the Epistemic Fallacy” (1997, pp.16, 36-38).

This consists in the view that statements about being can be reduced to or analyzed in terms of statements about knowledge; i.e. that ontological questions can always be transposed into epistemological terms. The idea that being can always be analyzed in terms of our knowledge of being, that it is sufficient for philosophy to ‘treat only the network, and not what the network describes’ [Wittgenstein, 1961, 6.35], results in the systematic dissolution of the idea of a world (which I shall here metaphorically characterize as an ontological realm) independent of but investigated by science. (pp. 36-37).

The classic case of the epistemic fallacy is the central logical positivist dogma of verificationism: any statement that is not empirically verifiable is essentially meaningless (an example of this fallacy, committed by Dennett (1991) where the existence of

qualitative subjective mental states and events is denied because there is epistemic uncertainty regarding them, is discussed in a latter section of this paper).

The empiricist might argue, for example, that when we discuss causation, we are not really discussing causation, only our individual conceptions of what causation is. Herein lies a reason that the epistemic fallacy is seen to be so innocuous and is so commonly committed. Is it not the case that when we really talk about causation (or any other concept for that matter) it is impossible to discuss anything but our current conception of it? The question of what causation *itself* might be is apparently meaningless, as the question of what one can know about causation is exhausted by what we can *know* about causation.

The realist will grant that it might be obvious that when one discusses a concept like causation one is necessarily *applying* her own conception. But that does not translate into talking about one's own conception. If it was not possible to distinguish between conceptions of causation and causation itself, would discourse comparing conceptions be possible? The question is addressed below in the section regarding idealism in a discussion of incommensurability.

The empiricist may agree that there might be a distinction between one's conception of a thing and the thing in itself, but disagree about the significance of the distinction; as it is apparently impossible (the empiricist would have us believe) to compare one's conception to the thing in itself, such a distinction holds no relevance to epistemic theory.

Any conception of causation (or any other concept) will necessarily be one's own, and if it is replaced or modified, the new conception will still necessarily be one's own.

True, but that does not disallow a distinction between discourse regarding causation and discourse regarding a particular conception of causation. In discourse regarding causation itself one focuses on interaction with the world, potentially discovering adequate reasons for revising a concept of causation. In (meta-)discourse regarding concepts of causation the focus shifts away from experience or experimentation to study of the history of ideas. Such a pursuit could lead a theorist to revise a concept of causation, but in a less scientifically significant manner than in the above sense.

Now what sorts of grounds are there for revising or replacing conceptions? The grounds are that a given conception fails to account for observed phenomena. The *transitive* dimension (where conceptions reside) is not an end in itself, rather it is produced with the purpose of describing, explaining, and predicting the intransitive dimension. The question of whether one is applying a conception correctly is only peripherally a question about the concepts; it is really about what can be known about the world, and is answered by means of putting the question to the intransitive dimension.

The premiss upon which the transcendental arguments of Bhaskar and the Critical Realists rest is the possibility of changing perception, particularly scientific progress. In the absence of a referent external to perceiver or theorist, one cannot account for (explain or justify) changing experience. "If changing experience of objects is to be possible, objects must have a distinct being in space and time from the experience of which they are the objects...we must suppose that there is something which they are describing in different ways." (Bhaskar, 1997, p. 31).

It is not by accident that I placed a discussion of the Epistemic Fallacy between discussions of Empiricism and Idealism. Both rest on forms of the fallacy. Empiricism

offered an introduction to the fallacy which in turn offers a similar introduction to Idealism.

### Contra Idealism

Modern Idealism seems to be the result of trying to cling to the Epistemic Fallacy, while at the same time trying to account for change.

Berkeley considered the universe to be comprised of minds and ideas, and observed that (at least finite) minds do not decide for themselves what the objects of their perception will be. Different minds see the same things, according to Berkeley, according to the goodness of an infinite mind (God).

Kant's transcendental idealism considers knowledge to be a product of the human mind. He concluded that the mind imposes form (space and time), but the content depends upon objects independent of the mind, *things in themselves*. How the mind acts upon the objects is not knowable, as all that is available to the mind is the formed and categorized mental states and events that we do in fact experience.

Bhaskar considers 20<sup>th</sup> century idealism (citing, in particular, Feyerabend, 1975, and Rorty, 1980) to be even more idealist than Kant, Berkeley, or Leibniz, in that the latter could at least make a claim to objectivity as all who perceive the same according to the will of God or because of the *things in themselves*. Bhaskar (1986, pp. 1-2) sees recent developments in idealism as *super-idealist* in that it sees the world as dependent upon the mind. The *super-idealists* go one better than Kant and relegate the things-in-

themselves to realm of the hypothetical (in a pejorative *non-existent* sort of sense), and consider objects of knowledge to be the knowledge that they produce. In Bhaskar's terms, the super-idealist conflates the transitive and intransitive dimension by denying that the latter is independent from the former.

For example:

A fish is only a fish if it is socially classified as one, and that classification is only concerned with fish to the extent that scaly living things in the sea help society define itself. After all, the very word 'fish' is a product of the imposition of socially produced categories on nature. (Tester, 1991, p.46; cited in Collier, 1994, p. 87).

If discourse does not refer to anything external to it, does it amount to anything more than poetry or fiction? If it does not deepen our knowledge of anything other than itself, surely it cannot be said to a scientific discourse.

An idea that is central to idealist philosophies of science (one that Bhaskar (1997, p. 248) attributes to Kuhn (1970), though Kuhn does not draw idealist ontological conclusions from it (see Kuhn, 1970, pp. 111-135)) is *incommensurability* (Bhaskar, 1997, pp. 248-250). When theories are incommensurable there is no relation between theoretical postulates or between the data of the two theories. Bhaskar argues against this (1997, pp. 248-249, 1986, pp. 70-93) by showing that if one accepts the incommensurability of theories, this leads to absurdities.

Although Bhaskar accepts that beliefs are produced by fallible social procedures, he does not believe that that acceptance should commit one to the idea that all beliefs are equally valid in that there are no rational criteria for choosing one belief as more correct

than another. *Epistemic* relativism ought not necessarily entail *judgmental* relativism (1986, p. 72).

If incommensurability is claimed, there is still a common referent, the entity or mechanism in the intransitive dimension to which the theories both refer. There is therefore a rational means by which to adjudicate between theories: of the two theories, which has more explanatory and predictive success under its own description.

We can then allow, for example, that theory  $T_a$  is preferable to theory  $T_b$ , even if in the terminology of Kuhn and Feyerabend it is ‘incommensurable’ with it, if theory  $T_a$  almost all of the phenomena  $p_1 \dots p_n$  that  $T_b$  can explain under its descriptions  $B_{p_1} \dots B_{p_n}$  plus some significant phenomena that  $T_b$  cannot explain. (1997, p. 248).

But, the modern idealist is likely to claim (as is anticipated, 1986, pp. 73-75) that incommensurable theories are so because there is no object that is external to the theory (i.e. no common referent), so the means of adjudicating between theories as suggested by Bhaskar is meaningless.

Although the modern idealist is likely to make the above claim, she is also likely to proceed as if the claim is false. They will allow, “the difference between say Newtonian and Einsteinian dynamics and that the latter is an advance on the former...” (1997, p. 249). But how can such a judgement be at all possible if there is no possibility of communication between theories? If incommensurability is to have any meaning, it has to refer to a conflict between theories. *Theories can only be in conflict if they conflict about something; if they are not about the same something, they do not conflict.* One could never have a rational reason for preferring one theory to another unless one could



recognize what it is that the theories clashed over. Even the weaker Kuhnian case of incommensurability (i.e. inability to communicate between theoretical schemas, but without ontological repercussions) cannot account for a rational choice between theories; if it really is impossible to have knowledge of an object under two descriptions, an understanding of one would preclude the possibility of ever understanding the other.

Feyerabend is a case study of the super-idealist position. His position is ostensibly about freedom of thought. But without any grounds for believing in the *truth* of our beliefs, that freedom is rather empty.

For Hume, 'reason is and only ought to be the slave of the passions'. But in Feyerabend the passions lack their necessary complement: an efficient slave. Knowledge may not be the most important social activity, but it is the one upon which the achievement of any human objective depends. Freedom, in the sense Feyerabend attaches to it, depends upon knowledge...; we can only be as free as our knowledge is reliable and complete. We are not free to choose what we believe if we are to attain the kinds of objectives Feyerabend mentions. Only if belief-in-itself was the sole end of human action would Feyerabend be warranted in such an assumption. (Bhaskar, 1975b, pp. 42-43)

### Naturalism

The question of Naturalism is the question of the degree to which the social sciences are or can be unified with and studied in the same manner as the natural sciences.

As the CR theory of science puts ontology before epistemology, it is natural that, in a realist explication of how it can be possible for there to be a methodological unity between the natural and social sciences, Bhaskar (1998, p. 25) should “concentrate first on the ontological question of the properties that societies possess before shifting to the epistemological question of how these properties make them possible objects of knowledge for us.” His approach to the study of the social sciences consequently begins by asking what properties of the objects of social inquiry make them possible objects of knowledge. The model proposed by Bhaskar begins by noting shared implicit knowledge of social mechanisms and structures, making them explicit, and using these as the basis for building social scientific theories.

As the CR view of science involves stratification, rootedness, and allows complex wholes, a realist story of the social sciences rejects the intuition of the methodological individualist/humanist that complexity is ultimately an illusion, and understanding really only follows from reduction of complex wholes into less complex constituent phenomena. On one hand, methodological individualism seems quite absurd: “Seldom does it occur to subscribers to this view that an identical train of thought logically entails their own reducibility, via the laws and principles of neurophysiology, to the status of inanimate things.” (1998, p. 26). On the other hand methodological individualism/humanism seems obviously true. It is all but beyond question that society is made up out of people; “the material presence of society = persons and the (material) results of their actions.” (1998, p. 30).

Bhaskar's vision of the social sciences involves suggesting that there can be a middle ground between methodological individualism/humanism and methodological holism/structuralism:

Sociology is not concerned, as such, with large-scale, mass or group behaviour (conceived as the behaviour of large numbers, masses or groups of individuals). Rather it is concerned, at least paradigmatically, with the persistent *relations* between individuals (and Groups), and with the relations between these relations (and between such relations and nature and the products of such relations). (1998, 28-29).

Social being is constituted by one's social relations. There is an ontological distinction between the individuals and the relations that constitute their social being. Consider the social relation of being an employee of a corporation; the relation persists after a particular worker retires or is replaced. The relation may have existed prior to and following the term of employment, as did the employee.

The strength of a relational approach is apparent in its ability to cope with the counter-examples that are offered by those who would criticize methodological individualism or humanism. "A tribesman implies a tribe, the cashing of a cheque a banking system." (p. 28). It appears that that if one were to try to hold to an atomistic view of the social being of individuals, the difficulty would be "how one could ever give a non-social (i.e. strictly individualistic) explanation of individual, at least characteristically human, behaviour!" (p. 28).

An atomistic view of relations would also be found to be lacking. One relation could not be fully explicated in the absence of a presupposition of other relations. Society is the sum of relations and relations between relations.

The two opposing views of social sciences described above each seem to offer the only possible theoretical structure when one looks at the specific “facts” that they are trying to incorporate.

- (i) Humanism. Social reality is constructed out of nothing but human actions and the effects of those actions. All social actions are carried out by people either separately or in groups. Actions taken by entities such as corporations or governments do not occur unless they are acted out by actual individual persons. As John Lennon would like to have us believe, if there are no soldiers, there is no war. Methodological Individualism/Humanism seems obviously true.
- (ii) Structuralism. The position that one holds in society doesn't just influence social actions; the structure of society enables, constrains, even determines what members of a society do. One cannot vote without an electoral system, cannot be a buyer or seller without a market, and must work to provide for self and family in a market economy. Social structure also determines the way in which a society develops. One might look at a market economy and see that there must be periods of both growth and recession, that there must be inflation and technological advance. One might also look at the structure of society and predict particular courses of development. The Russian and French Revolutions can be explained by

recourse to the social structures that existed prior to the events.

Methodological Holism/Structuralism seems obviously true.

From the point of view (i) it would appear that in order to explain social action, the concepts needed are those that apply to the individuals, things like motivation or attitudes. More complex social phenomena can be interpreted as nothing but the cumulative effects of the actions of individuals.

From the point of view of (ii), the theorist could eliminate individual psychological concepts entirely in favor of structural concepts. Motivation, attitudes, agency, and so forth could be interpreted as illusory or epiphenomenal, merely by-products of or the result of structure.

Despite their intuitive “obviousness” humanism and structuralism cannot both be true. Bhaskar offers a third perspective, the Transformational Model of Social Activity (TMSA, which he attributes to Berger & Pullberg, 1966) that tries to account for both sets of considerations by suggesting that both sorts of causality are real: individual agency and structure both have real causal effects. Society produces and transforms people from biological raw materials; people create and modify society through their actions. “Society is both the ever-present *condition* (material cause) and the continually reproduced outcome of *human* agency. And praxis is both work, that is, conscious *production*, and (normally unconscious) *reproduction* of the conditions of production, that is society.” (Bhaskar, 1998, pp. 34-35).

An illustration of this transformational process can be found in the use of language. Language exists prior to its being learned by any given person; rules of grammar, syntax, and usage exist independently of any particular user (condition).

Language is used for conscious personal means (conscious production). The use of language allows it to continue to exist and to evolve (unconscious reproduction).

The importance of distinguishing categorically between people and societies, and correspondingly between human actions and changes in the social structure, should now be clear. For the properties possessed by social forms may be very different from those possessed by the individuals upon whose activity they depend. Thus one can allow...that purposiveness, intentionality and sometimes self-consciousness characterize human actions but not transformations in the social structure. The conception I am proposing is that people, in their conscious activity, for the most part unconsciously reproduce (and occasionally transform) the structures governing their substantive activities of production. Thus people do not marry to reproduce the family or work to sustain the capitalist economy. Yet it is nevertheless the unintended consequence (and inexorable result) of, as it is also a necessary condition for, their activity. Moreover, when social forms change, the explanation will not normally lie in the desires of agents to change them that way, though as a very important theoretical and political limit, it *may* do so. (1998, p. 35).

Central to the TMSA is a means by which the causal mechanisms of the psychological and sociological strata can interact. If, as is argued above, there is an ontological distinction between people and the structures of society, then

we need a system of mediating concepts,...slots, as it were, in the social structure into which active subjects must slip in order to reproduce it...[a]

'point of contact' between human agency and social structures. Such a point...must *both* endure and be immediately occupied by individuals. It is clear that the mediating system we need is that of *positions* (places, functions, rules, tasks, duties, rights, etc.) occupied (filled, assumed, enacted, etc.) by individuals, and the *practices* (activities, etc.) in which, in virtue of their occupancy of these positions (and vice versa), they engage. (1998, pp. 40-41).

So for Bhaskar and the TMSA, the subject matter of the social sciences lies in the relations between "positioned-practices" rather than between specific individuals, as it is the former that endure, though they may be occupied by different individuals at different times.

### A Critical Realist Version of Social Psychology

In this section the persistent questioning of the scientific status of social psychology is examined. It is argued that a significant reason for the rejection of the scientific veracity of social psychology by many practitioners of science is that they hold an inadequate (empiricist/verificationist) view of science. A further reason for the conception is a lack of adequate demarcation between social psychology and related fields. This could be remedied via a redescription of social psychological endeavors into first moment social psychology (social psychology proper, narrowly defined) and second moment social psychology (related disciplines that contribute to social psychology proper). Furthermore, social psychology proper, thus demarcated, can meet the standards of realist versions of natural science.

Wyer and Srull (1984) speculate that our understanding of social behavior can be “enhanced” by paying attention to cognitive processes that underlie them. The central claim of this chapter is that that speculation is understated. The very *essence of social psychology* is that it is (or at least ought to be) the discipline that seeks to explain social behavior via reference to mental states and events, particularly intentional consciousness.

It could be argued that social psychology fails to meet the minimum requirements that are needed in order to qualify as a science along the following lines. Davidson (1974), for example, held that psychological phenomena are not subject to nomological interpretation in that it seems *prima facie* impossible to provide the bridge principles that will allow translation from the psychological to the physiological. Fay (1983), following Popper, suggests that social phenomena cannot be studied in the same manner as the natural sciences, as human systems seem intrinsically open. Scriven (1956) and Hayek



(1967) make a similar points by emphasizing the complexity of human activity—to describe human behavior at such a complex level apparently makes prediction impossible.

There are at least two potential lines of reasoning that might lead one to conclude that social psychology is unscientific: The first is the model of science commonly held by practitioners of science; the second is nature of the objects of study.

Let us first consider the commonly held view of science that precludes social psychology.

Consider the possible consequences of practitioners of science predicting what the limits of the scientific endeavor might be. Surely this would erect artificial barriers in what science could in fact accomplish. Hence, the assumption that everything is in principle discoverable to science seems a necessary heuristic device. Having said that, it appears that this attitude has led to a few grievous errors, particularly with respect to the nature of the mind. These errors have had repercussions for social psychology; specifically, many deny that social psychology can have any legitimate claim to the appellation “science.”

One of these errors is related to an incomplete rejection of Cartesian Dualism. A common presupposition of science (explicitly or implicitly) is physicalism, which in its simplest form states that the *real* world is comprised entirely of the physical world. Cartesian thought divides the world into the physical and the mental. The hamartia of physicalism is that it fails to reject the vocabulary (and the attendant dichotomous categorization) of Cartesian Dualism; it retains the (Cartesian) physical and dismisses the (Cartesian) mental. The scientist whose ontology is shaped by the physicalist qua

Cartesian categories and terminology might have difficulty with a statement like “brains cause mental phenomena” because if the mental is not included in the physical then there can be no such thing as the mental.

Another error lies in the assumption that scientific knowledge ought to be in the form of scientific laws. Such laws are commonly the result of observing a constant conjunction, and inferring that the conjunction would be as constant outside of an experimental situation. The validity of this Humean inference requires that the systems in which the laws operate are closed systems (closed systems are those in which constant conjunctions obtain, as in the controlled experimental situation). Critical Realism holds that closed systems don't in fact occur unless artificially established, and the lack of predictability in the human sciences strongly intimates that the human mind (as well as other objects of social inquiry such as institutions, societies, cultures) is not a closed system.

A third error is alluded to in the opening paragraphs, and is closely related to the first two mentioned. The principle of scientific verificationism follows from the thesis of physicalism. Because everything that is real is physical, everything in the universe, at least in principle, should be the sort of thing that is discoverable to science; if something cannot be discovered by the objective physical sciences, then it cannot be part of the natural world and cannot exist.

Mental events and states run into difficulty with physicalism and verificationism because their ontology has an element of irreducible subjectivity. Consider the standard case of reduction. To explain warmth, one dismisses the qualitative, subjective, experiential feel of warmth, and, focusing entirely on physical processes, reduces heat to

mean molecular motion. Application of this pattern to mental states and events leads to a serious difficulty: if we dismiss the qualitative subjective property of the phenomena, we eliminate the very thing that we wish to explain.

How do those who put their faith in physicalist verificationist science perceive such phenomena? Dennett (considered by many to be a/the leading thinker in matters of the mind) says that **“there are no qualia [qualitative, subjective mental states or events] over and above the sum total of dispositions to react...”** (Dennett, 1991, p.398).

The second source of bias against social psychology lies in the nature of the subject matter. Phenomena like aggression, self-efficacy, and attitudes seem to have an essential qualitative subjective element that, according to the above considerations casts doubt on their very existence, and *prima facie* disqualifies them from scientific scrutiny.

Hence social psychologists appear to be faced with a paradox. They wish to pursue knowledge based on scientific methods, but the nature of the subject matter seems to make this impossible. The solution to this paradox taught in undergraduate psychology and research methodology courses can be interpreted as deeply unsatisfactory. When the phenomenon in question is not strictly observable, an operationally definable and measurable correlate may stand in. And though the behaviorism of Watson and Skinner (I was taught) is now largely considered anachronistic, the operational definition that is employed is little more than a correlate of the target phenomena described in behavioristic terms. But an attitude, for example, is intuitively an attitude in virtue of its mental content, and its subjective *attitudish* quality, independent of any “dispositions to react” that might accompany it.

A potential unsavory consequence of making the above move is the further paradox that the intended subject matter of social psychology cannot be reduced to what social psychology, in practice, studies.

In the Laplacean universe that is an uncritical presupposition of much empiricist or positivist thought, reasons/mental states can claim no causal purchase on the world. A perspective shaped by the theses of physicalism and verificationism traditionally cannot conceive of an ontology with room for the mental. Rather than derive the counter-intuitive conclusion that thoughts do not exist or that behavior is independent of thought, perhaps the wiser move would be to examine the assumptions that would necessitate such an inference.

The argument of this chapter is as follows:

1. A realist perspective of science allows for the existence of phenomena based on the observation of the causal powers of the phenomenon in question. As such, realism can accommodate the practice of science in a way that empiricist models cannot.
2. Mental states and events have observable causal effects on the world, and are therefore a legitimate subject of scientific scrutiny. Mental States and events can be used fruitfully to explain social behavior.
3. A social psychology demarcated from other disciplines (particularly sociology) by virtue of defining its subject matter as mental states and events would therefore qualify as a natural science.
4. Finally a redefinition of some endeavors of social psychology will be presented. I propose that social psychology can be divided into two moments.

(i) First Moment Social Psychology, or Social Psychology Proper is the explanation of social behavior by recourse to mental states and events. (ii) Second Moment Psychology would stand roughly in relation to social psychology proper as ethnography stands to anthropology; it would be the research programs that contribute to First Moment Social Psychology (such as areas of sociology, cognitive psychology, sociobiology/evolutionary psychology, neurobiology, etc.)

#### Transcendental Realist View of Science

As was argued in prior sections, empiricist philosophic positions that include the theses of physicalism and verificationism can be replaced by a causal realism established by transcendent argument. Verification, per se, is made superfluous by the philosophic ontology of the intransitive dimension, and ascription of reality to a theoretical entity can be solely on the basis of its capacity to have a causal effect on the material world (Bhaskar, 1998, p. 12). The Newtonian or Laplacean universe is replaced by a stratified one in which causal laws are better described as tendencies that may or may not be actualized or perceived (due to the lack of causal closure in the environment in which they operate).

This paper accepts the causal realist perspective of (for example) Harré and Bhaskar, and the stratified world of Bhaskar and the Critical Realists. However, this should not be taken as implying that a theory of social psychology that attempts to fit into that scientific structure ought to adopt the peripherally related realist theories of social science. Hence, Two Moment Social Psychology makes no specific claim to fit with Bhaskar's Transformational Model of Social Activity (e.g. Bhaskar. 1998, pp. 31-56).

The aim is not to fit social psychology into Bhaskar's view of the human sciences, nor into a hermeneutic framework. The aim is to argue that social behaviors are best explained by recourse to the mental mechanisms that generate them. The *social* aspect of this model is not derived from a shared methodology with other human or social sciences, it is social in virtue of that which it seeks to explain (*social* behavior).

#### Causal Efficacy of Mental States and Events

What are these mental states and events that are postulated as the causal mechanisms that generate social behavior? One is hesitant to use the term *qualia* (qualitative, subjective, phenomenal mental states and events) as it is vague, problematic, and controversial. Furthermore, *qualia* does not quite capture the sense of mental states and events as is used in this paper in that (i) unconscious mental activity is not precluded from playing a role in psychological explanations of behavior, but fail to reach the criteria of the quale, and (ii) (narrowly construed) leave out an important element of mental activity, that being acts of volition.

Goldman (1993) proposes that "we should be prepared to use the terms 'phenomenal' and 'qualitative' for states with any sorts of sensory feel, not just sensory qualities..." Searle (1997, pp. 8-9, 29; 1992, p. 20), suggests "[t]hat there is a special qualitative feel to each type of conscious state...", but is "...hesitant to use the word 'qualia' and its singular, 'quale,' because they give the impression that there are two separate phenomena, consciousness and qualia." One does want not to give such an impression as "the mind consists of qualia, so to speak, right down to the ground." The position of this paper is that these authors are correct, and that qualia and acts of volition are two sides of the same coin. However because of the controversy and lack of

specificity surrounding the use of *quale* and *qualia*, for present purposes the term “intentional consciousness” will be employed to include both the phenomenological properties of our conscious experiences (*qualia*) and acts of will (*volitions*).

Polger and Flanagan (1997?; also Flanagan, 1993, pp.129-152) argue “...that for any intelligent activity *i*..., even if we do *i* with conscious accompaniments, *i* can in principle be done without these conscious accompaniments.” The presence of consciousness is thus allegedly contingent and difficult to account for in terms of survival value. This is the thesis of *Conscious Inessentialism*. Consciousness might have no survival value, they argue, but still might have evolved as a byproduct of brain states that offer survival value. The coat of the polar bear is so heavy that it slows the animal, but its evolution can be accounted for by the warmth that the added weight offers. An analogous (“Just So”) story might be available in the case of consciousness.

But the idea that consciousness offers no survival value is less plausible when one considers *blindsight*, and Penfield’s research on epileptic subjects.

Persons who suffer blindsight have damage to visual pathways sufficiently high (striate cortex/area V1) that although the brain has *informational* sensitivity (it is demonstrably registered in the brain) to visual stimuli, the damage makes it impossible for the patient to have *experiential* sensitivity (that register is below the conscious threshold) to a portion of the visual field. The patients protest that they are blind in a portion of their visual field, yet when asked to guess at what objects might be in the “blind” spot, can have a remarkably high success rate (Velmans, 1992). When asked to reach for an object in the blind field, the patient, though protesting that (s)he doesn’t know what is there, will move his/her arm in the right direction, and position and shape

the hands in the correct manner in which to pick up the object (Marcel, 1988).

Furthermore, although it could be demonstrated that a patient has informational awareness (i.e. the brain is aware, as demonstrated by accurate guessing, but the subject is not consciously aware) of a glass of water in the blind field, a thirsty blindsight patient will not reach for a glass of water in his/her blind field (Marcel, 1986). The brain of a subject/patient might be aware of particular aspects of her environment, but the person with a conscious awareness of those same aspects will have a demonstrable advantage over those without it.

Penfield (1975, p. 39) discusses case studies of patients suffering petit mal epileptic seizures. There are cases of patients who suffer a seizure while performing a well learned (meaning that the activity can be performed well without conscious effort, or close conscious attention) goal directed activity such as walking, driving, or playing a musical instrument. In cases where the activity is sufficiently well learned, goal directed activity persists in the absence of consciousness. Here, support should *not* be inferred for the thesis of conscious inessentialism. What should be noted is that there was a pronounced lack of flexibility on the part of the actor: the pianist was not aware of the piece that he was playing and as a result it sounded mechanical, and the driver drove through red lights.

The above can be cited as counters to the thesis of conscious inessentialism. Intentional consciousness provides survival value, and makes a difference in the conditions of the material world by virtue of the fact that it adds creativity, flexibility, and finer powers of discrimination (Marcel, 1986; Searle, 1992, pp. 107-109; Van Gulick, 1989).



Denying the causal powers of consciousness can lead to absurdities. Assume that your beliefs, attitudes, and alleged reasons for acting are not causes of your behavior (e.g. Davidson, 1963; Lewis, 1997; Winch, 1958). The story to be told regarding one's acts of volition is in fact two parallel stories. Consider again the example of the person at the newsagents buying the Guardian. There is a physiological story (nerves firing, muscles contracting), and quite independently, there is a mental story (the attitude or belief that is perceived to be the reason for acting).

Bhaskar is not alone in arguing (1998, pp. 80-119) that psychological phenomena are not reducible to other processes.

Fodor (1980) for example, argues for *methodological solipsism*. His aim here is to argue for a flaw in the methodological comparison of minds to digital computers. If the mind is a digital computer then the brain operates on some sort of symbols (in some way analogous to the 0's and 1's), and can only distinguish between mental states and events according to differences in the syntactical structure of those symbols. So as a computer does not distinguish between strings of symbols according to what they represent, so the brain can only distinguish between syntactic properties: "...the computational theory of the mind requires that two thoughts can be distinct in content only if they can be identified with relations to formally distinct representations...it follows that content alone cannot distinguish thoughts."

So, according to Fodor, if the subject matter of psychology is the processes that go on in the mind, large portions of psychology are going to be disqualified. For example, a psychology of knowledge becomes impossible as knowledge involves the *truth* of beliefs, and questions of truth involve semantics, not syntax.

Davidson's (1974) *anomalous monism* makes a similar point regarding the nature of mind (where Fodor's point was methodological). He argues that explanations of behavior that resort to *reasons* do not presuppose any general law. Because such explanations involve taking into account a web of beliefs, and offers the possibility of a sufficient condition for given behaviors (whenever one has mental states and events (i), (ii), (iii),..., (n), and conditions 1, 2, 3, ..., n obtain, we predict behavior E) without any necessary conditions, there can be no strict psychological laws. Hence, when we describe events in mentalistic terms we cannot describe them in deterministic terms. However, Davidson does not reject mind/brain identity. If mental states and events are somehow physiological events, then we can redescribe the same events in deterministic physical terms. If there can be no strict psychological laws, but there can be strict physical laws, then there can be no strict bridging principles or correspondence rules—there can be no strict psychophysical laws.

Now, let us consider again the example of purchasing a newspaper at the newsstand. If these stories are indeed independent, how are we to make sense of the seemingly obvious *dependence* between them? There seem to be two ways to resolve the dilemma. The first is some sort of theory of pre-arranged harmony (as in Leibnizian *Monadology*), doubtless unacceptable to the majority of today's scientists. The second resolution is that phenomenal consciousness is an epiphenomenal emergent property—that consciousness is a result of brain processes, but in turn has no affect on brain states, behavior, or on any aspect of the physical world (Churchland, 1984, p. 10-11; Davidson, 1970). If we are to accept the strict philosophical definition of epiphenomena (Broad, 1925, p. 118: that epiphenomena are effects but have no affects themselves), then by

labeling qualia as epiphenomena we are forced into a corner: we must admit that (if they don't even affect subsequent brain states, and brain states cause mental states) there can be no justification for the belief that qualia exist. The fact that we do believe that we and others have subjective awareness should count as quite a strike against the assertion that qualia are epiphenomenal.

Russell (1954, p.159, 1956, pp. 145-165) submits that the causal powers of mental states and events arise because of the convergence of the transitive and intransitive dimensions in psychology (though he does not use this terminology). He proposes that "the events that make up the living brain are actually identical with those that make up the corresponding mind."<sup>4</sup>

The supposed problem of the relations of mind and matter arises only through mistakenly treating both as 'things' and not as groups of events...[A] piece of matter is a group of events connected by...the causal laws of physics. A mind is a group of events connected by...the causal laws of psychology. An event is not rendered either mental or material by any intrinsic quality, but only its causal relations. It is perfectly possible for any event to have both the causal relations characteristic of physics and those characteristic of psychology.

To say that a thought is in the brain is an abbreviation for the following: a thought is one of a group of compresent events, which group is a region of the brain.

It should be clear that (i) intentional consciousness exists, (ii) that trying to reduce them to other (computational, physiological) processes does not eliminate them, (iii) that

they physically effect the physical world, and (iv) that denying (iii) is absurd. Now, if the philosophy of science that one holds leads one to cling to absurdities, perhaps the appropriate course of action might be to stop upholding absurdities and reevaluate the philosophy of science that one holds.

### The Essence of Social Psychology

The seed of this view of social psychology was planted some years ago as I was reflecting upon the inability to give an immediate response to the query “What is the difference between social psychology and sociology?” (after having completed the requirements for a BA in social psychology!). The seed was germinated upon reading Harré’s (1993) Social Being, in which he develops a hermeneutic social psychology that seems, at least on the face of it, virtually indistinguishable from micro-sociology. In order to bring social psychology into a (social) scientific structure Harré’s hermeneutic approach avoids reference to exclusively internal mental states in explanations of social behavior, instead following a Wittgenstinian “open souls” line of thinking in which one’s mental life is constituted in one’s interaction with the world. Specifically, Harré seems to have given up on the search for the underlying causal mechanisms that might account for social behavior, and has opted for an approach of trying to understand that behavior from the perspectives of those engaged in the activity.

While this is no doubt a valuable research paradigm, it is questionable whether this approach ought to be counted as a form of social psychology, as it entirely ignores the *psych* aspect of social *psychology*, leaving only social-ology (→ sociology). Hence Harré’s version of social psychology might be more appropriately described as a form of (micro-) sociology.

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<sup>4</sup> J. J. Smart (1959) offers a classic argument in a similar vein.

The aim of the present argument is to justify the inclusion of social psychology under the banner of natural rather than social science. Whether a hermeneutic approach satisfies the conditions of a social science, hermeneutic or otherwise, is beyond the scope of this paper. What is certain is that a hermeneutic approach does not satisfy the requirements for a realist natural science in that it does not seek to model the causal mechanisms that underpin social behaviors.

One might ask if an attitude or aggression has an intransitive existence. It might be the case that the existence of mental phenomena is not intransitive to the person to/in whom it occurs. The mental states and events in question, if they exist, are intransitive relative to the external observer, researcher, or theorist. By suggesting that self efficacy, for example, has an intransitive existence, this paper is not positing a Platonic existence independent of any mind. There are mental states and events that we label as self efficacy (this will be defined below), or as a state of aggression, or as an attitude—these, it is suggested, are the most direct cause of social behavior, so are the appropriate explanans.

If one leaves the *psych* out of psychology, then *ipso facto* it ceases to be a *psychology*. A review of some central issues in social psychology will reveal that the notion of intentional consciousness is essential to the field in that whenever it is not the object of interest, the research in question could be categorized under a competing field (sociology or sociobiology, for example).

Festinger (1957) develops a theory of **cognitive dissonance** to account for changes in behavior or attitude due to inconsistencies between attitudes/behaviors and attitudes. Cognitive dissonance is the (normally unpleasant) *feeling* that arises when we

discover inconsistencies between attitudes and behaviors. Subtract the qualitative aspect of the experience from cognitive dissonance and there is no cognitive dissonance.

**Self-efficacy** is defined (Bandura, 1986, p. 391) "as people's judgments of their capabilities to organize and execute courses of action required to attain designated types of performances. It is concerned not with the skills one has but with judgments of what one can do with whatever skills one possesses." Self-efficacy cannot be merely the behavioristic relation between input and output. Self-efficacy is postulated to explain that relationship. Like cognitive dissonance, self-efficacy is a *feeling*. It is a feeling of confidence regarding performance of a specific task. Otherwise we are forced into a bit of a Nietzschean corner.

**Attitudes** shape our social perceptions and behavior, they involve associations between the objects of attitudes and evaluations of those objects (Fazio, 1989;). As Judd et al. (1991) argue, attitudes are linked together in memory, allowing activation of one attitude to lead to activation of related attitudes. Again, the possibility of a behavioral analysis of input and output is negated as the attitudes are things in themselves, not just mediational concepts, so if their qualitative *intrinsic* mental aspects are denied, the study of attitudes becomes impossible. In line with the logic of scientific discovery expounded by the realist, the study of attitudes involves the creation of schematic models of attitudes. Petty and Cacioppo (1986), for example, offer the elaboration likelihood model, and Chaiken (1987) offers a heuristic model.

**Aggression** appears to fit the same pattern in that it seems to have an irreducibly inner subjective quality. While it is true that analyses of cases of aggression are likely to include qualia, it seems that aggression itself is not a quale per se, but rather a cluster of

concepts relating behavior that arises from any number of qualia. It has been suggested that the *reasons* underlying aggression can include *frustration* (e.g. Berkowitz, 1989; Gustafson, 1989), *perceived* provocation (e.g. Ohbuchi and Ogura, 1984; Torestad, 1990), embarrassment, fear, or other forms of general *arousal* (e.g. Zillman, 1988), *sexual arousal* (e.g. Zillman, 1984), or *shame* (e.g. Tangney, 1990; Tangney et al. 1992). Again, these cannot simply be the correlation of the stimulus with output; these phenomenal mental states and events are posited as an explanation for the correlation.

**Motivation** likewise seems to be a cluster concept covering a number of qualitative subjective mental attributes that can be postulated to account for behavior. Winter (1973) has demonstrated that students who achieved in sports measured high in a *need for power*. Some (particularly males) who persevere at a particular sport or occupation measure high in *need for achievement* (e.g. Atkinson, 1958; McClelland, 1987), while others (particularly females) are high in *need to affiliate* (O'Leary, 1974). Take away these subjective affective states, and the correlation between environment and behavior is just that, a correlation (perhaps a law by Humean standards...), not a fact about the human psyche, and hence, on its own, of little interest to the *psychologist*.

A true social psychological explication of aggression seems to require the inclusion of intentional consciousness. What happens when it is left out of the explanation?

Berkowitz and LePage (1967, also Berkowitz, 1968) propose the *weapons effect*. They found that the mere presence of aggressive cues (such as weapons) enhances aggression in angered subjects. "*Guns not only permit violence, they can stimulate it as well. The finger pulls the trigger, but the trigger may also be pulling the finger.*" (1968,

p. 22). The aggressive cues are socio-cultural artifacts, and there is no reference to mental states or events, so surely this study could be just as accurately described as micro-sociology?

Gladue (1991) found that in males levels of testosterone tend to be positively correlated with aggressive tendencies, while the correlation is negative in females. These correlations are independent of social factors and gender roles. This study clearly belongs under the heading of sociobiology as it has a biological basis, relates to external social factors, and is about behavior rather than the psyche.

Attitudes can likewise be studied from a perspective other than that of their qualitative subjective element. Arvey et al. and Keller et al. (1989, 1992, respectively) speculate on the role of genetic inheritance on a wide variety of attitudes. These authors have examined differences between monozygotic and dizygotic twins. Even when separated at birth and raised in different environment, the attitudes of identical twins are still correlated more than those of unrelated persons or those of dizygotic twins. Though terribly suggestive to the social psychologist, the research would nevertheless be more appropriately described as sociobiology (or perhaps evolutionary psychology).

As is suggested in the above paragraph, aspects of behavior can be at least partially influenced by genetics (e.g. Rushton, 1989). Hence, concepts of sociobiology are based on the theory of natural selection (e.g. Archer, 1991). The process of *maximizing inclusive fitness* has been suggested as an explanation of altruistic and selfish behavior. Simply stated the hypothesis holds that individuals are more likely to act in an altruistic manner to those that are likely to be genetically similar to oneself. Such studies generally ascribe a regularity derived from the observation of correlations, bypassing any



mechanisms operating at the level of consciousness. Conceivably these mechanisms could be described as psychological if the causal mechanisms are described in terms of genetically determined neurological mechanisms and structures, and if we allow that the study of neurobiology falls under the general rubric of psychology (but would any self-respecting *neuroscientist* allow that he is doing a form of *social* psychology?). Such research is perhaps better described as evolutionary biology.

Environmental factors have been postulated to be causes of behavior. For example an astonishingly large number of people believe that a full moon directly influences behavior. Russell and Dua (1983) found that 64% of Canadian university students—people who should know better—subscribe to such beliefs. Carefully designed and controlled studies (e.g. Rotton & Kelly, 1985) have shown that there is no correlation. Rotton and Frey (1985) found a correlation between air pollution and the number of family disturbances reported to the police. Again it must be asked whether, having left out reference to mental states and events these research programs can properly be described as social psychology. The research described in this paragraph would perhaps best be described as some kind of environmental correlational sociological study.

The presence of other persons has been proposed as a cause of behavior in a number of contexts. *Social impact theory* (Latané, 1981) examines the impact that observers have on performance. It posits that social impact of observers on performance is determined by social forces (the strength or importance of the observer(s), immediacy in space and time, and the number observers), marginal impact (diminishing impact of each observer as the number of observers increase), and division of impact (the more performers there are, the less the impact of the observers). Over a century ago (as

reported by Kravitz and Martin, 1986) Ringlemann found that in a tug-of-war, as team size increased, average force exerted by team members decreased (pairs exerted 93%, threesomes 85%, eightsomes 49%). *Social loafing* (Latané, Harkins, & Williams, 1980) is the decrease in individual effort corresponding to an increase in team size. But without mediational psychological concepts (such as self-efficacy or conceptions of motivation), there is nothing to distinguish this research as specifically *psychological* research.

The critic might counter that this paper, rather than identifying what social psychologists actually do, simply redefines what research programs can be called social psychology. In this observation the critic would be correct. But the recategorization is not at all arbitrary. It is based on the following criteria. The majority of social psychology is either directly or indirectly about mental states and events (particularly intentional consciousness). (i) Social Psychology minus mental states and events ceases to be *psychology*, and can just as easily be designated as research in a competing discipline. (ii) Those who subscribe to the philosophies of science criticized above have characterized social psychology as non-scientific; but as argued above, when the discipline centers on intentional consciousness it can fit into a causal realist view of science. So if the adjective *scientific* is one that the social psychologist desires to have applied to the discipline, the redelineation seems a not unreasonable trade-off.

#### Two Moment Social Psychology

If the objects of study in social psychology are mental states and events, then pure social psychology is going to be the exception. When it occurs it is as likely to be found in books and journals of the philosophy of mind or social cognition as in works of social psychology. In the area of philosophy of mind Block (1995), for example, models forms

of consciousness; Peacocke (1983) argues for a distinction between sensations and content of experience; and Penfield (1975), Marcel (1986, 1988), Shallice (1988), and Velmans (1992) speculate about what the capacities of qualia (and therefore intentional consciousness generally) to influence behavior might be. In the area of cognitive social psychology Lassiter et al. (e.g. Lassiter, Briggs, & Bowman, 1991, Lassiter, Briggs, & Slaw, 1991) speculate on the effects on social behavior of the motivating factor *need for cognition*, Isen (1987) discusses the relationship between emotions and cognition, and various researchers have investigated the role of *schemata* (mental *scaffolding* by which we perceive and interpret stimuli) in social behavior (Bodenhausen, 1988; Pritchard, 1991).

It would be virtually impossible to conduct pure social psychological research in a vacuum. For this reason social psychology might be profitably divided into two moments.

*First moment social psychology* will consist largely in classifying or categorizing mental states and events that are the immediate cause of social behavior; it would involve the *creative construction of models of the psychological mechanisms that mediate between one's social environmental stimuli and social behavior.*

*Second moment social psychology* would consist of related fields as described above, including, but by no means restricted to, comparative psychology, neurobiology, micro-sociology, sociobiology, evolutionary psychology, environmental psychology, etc. It would be the means by which the reality (causal efficacy) and accuracy of the postulated causal mechanisms can be checked. Really, second moment social psychology *can consist in any discipline that either (i) suggests hypotheses that might fruitfully be explored by first moment social psychology, or (ii) might provide an arena in which first*

*moment social psychological hypotheses can be empirically tested.* Second moment social psychology would be the point at which the operational definitions would prove of value.

Whether second moment social psychology qualifies as being scientific is an open question. The answer will depend upon the nature of the contributing research. Hermeneutic sociology and neurobiology can surely both make tremendous contributions to knowledge regarding the cognitive structures that account for social behavior, but (at least in the scientific framework outlined in this paper) only the latter will qualify as a scientific endeavor.

Sperber (e.g. 1985, 1996, particularly chapters 3 and 6) has suggested a similar (and controversial) delineation between (cognitive) anthropology and ethnography. Sperber has suggested that the true anthropology, the true explanations of how and why societies evolve in the way that they do involves an understanding of the cognitive structures that explain how and why token mental representations (conceptually similar to Dawkins' memes) and token public productions are replicated and transformed. Ethnography supplies the observational data from which the anthropologist can draw her conclusions. Second moment social psychology would fill a role similar to that of ethnography to Sperber's anthropology. But where Sperber suggests that cognitive anthropology is a discipline distinct from ethnography, it is recognized that first moment social psychology would be virtually impossible in the absence of second moment social psychology. Therefore *research in a realist social psychology would proceed largely in the pursuit of the competing disciplines that comprise second moment social psychology.* But unless the research is explicitly or implicitly in aid of constructing or refining models

*of the mental states and event that account for social behavior, it fails to earn the designation social psychology.*

To illustrate the distinction, consider the research described above regarding social impact theory and social loafing. While failing to meet the criteria of social psychology as they stand, these projects could do so by considering the significance of the findings for self-efficacy and conceptions of motivation, respectively. Or consider the ways in which athletes explain success or failure of themselves or their teams. Lau and Russell (1980) examined 594 explanations of performance and found that following successful performances athletes (as well as coaches and sportswriters) were likely to attribute the performances to internal factors. This support for the *self serving bias* sheds light upon the way in which one makes causal attributions, maintains self efficacy, or maintains a positive self image.

#### Conclusion (of social psychology chapter)

Social psychological knowledge will not be in the form of Humean causal laws (such laws don't exist). It will be in the form of increasingly refined models of the mental causal mechanisms of social behavior. As realism argues that outside of experimentally closed systems causal closure is not a valid presupposition; the impossibility of ascription of causal closure to the human mind consequently impacts the scientific validity of the discipline no more than it does any other.

The introduction to this chapter suggests that because the objects of social psychological experimental activity are actually operationally defined behavioral correlates of mental states and events, the true subject matter of social psychology cannot be reduced to what social psychology in practice studies. The two moment social

psychology developed here overcomes this problem in two ways. First, social psychology proper (first moment social psychology), strictly speaking, would not study behavioral analogues. Instead it would examine the data of second moment social psychology (which need not be averse to studying behavioral correlates). Second, as the discipline is scientific by virtue of meeting the standards of scientific (transcendental) realism, first moment social psychology would proceed in the modification or transformation of theories by use of inference or transcendental argument. Once intentional mental phenomena are established as existent (through observation of their powers), results obtained from second moment social psychology can be used by first moment social psychology to determine how consciousness must be in order to account (in a scientific manner) for those results.

The subject matter of social psychology is likely to be mostly intentional consciousness. The denial of intentional consciousness is dependent on mistaken anachronistic philosophies of science that are in fact incompatible with the practice of science. Qualitative two moment social psychology is compatible with the transcendental realist philosophy of science.

### Conclusion

In this paper we have looked at Roy Bhaskar's Critical Realist philosophy for science. We have compared the views of Empiricism and Idealism, and while it is clearly intuitively compelling, it is lacking in some ways. Specifically, CR aims to establish the existence of an intransitive dimension—external reality, rather than just postulating it as a working hypothesis. However the *proofs* depend on a circular argument. The arguments do offer a healthy boost to our realist presumptions if read as a case for the plausibility of realism instead of as a proof. CR attempts to account for the potential paradox of science being a social and transitive practice about an objective intransitive reality. In so doing, CR buttresses the realist position against idealism by positing multiple strata of reality that roughly correspond to the various sciences. The aim of science is defined by CR as the attempt to understand phenomena in terms of the causes of the events in question rather than narrowly defined scientific laws. Scientific laws, instead of being Humean constant or frequent conjunctions, are redescribed as the tendencies of things (in virtue of their structure) to act in a particular way when unimpeded by other causal mechanisms.

The idea that science proceeds in explaining phenomena by offering explanations grounded in underlying causal mechanisms carries great intuitive weight, and is supported in a transcendental argument that justifies a non-deductivist structure of scientific reasoning.

While the CR theory of science has an advantage over some other arguments for scientific reasoning (I'm thinking specifically of *no miracles* types of argument in that it does not rely on the success of science as a premiss, so is less open to arguments from

pessimistic induction), it shares the potential weakness of being somewhat question begging.

If one allows a weaker theory of epistemic justification (internalism, particularism, or coherentism), then the claims of the scientific realist can be counted as tentative knowledge claims, and “proofs” of realism like that of Moore and Bhaskar fall within acceptable epistemic bounds, and can be retooled as explications of realist intuitions. When one makes a realist scientific claim, CR makes it clear exactly what it is that one is claiming.

CR is not without its detractors. In future works I intend to discuss some of the potential weaknesses in the theory. An area that I think particularly needs strengthening is a possible swing too far from a deductivist model. Critical Realists tend to accept Psychoanalysis and Marxism as scientifically valid theories. Perhaps a reconsideration or adoption of a modified Popperian approach could shore up its defenses against theories that many, if not most, consider illegitimate sciences.

I would also like to make explicit in a future chapter how CR would critique Van Fraassen’s (1980) theory of Constructive Empiricism, though much of the critique would already be implicit in what is above. I will probably take the line that, being *actualist*, van Fraassen’s position does not offer sufficient grounds for choosing between theories.



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