

# Realism without Reductionism: Toward an Ecologically Embedded Sociology

Michael S. Carolan

Department of Sociology  
Colorado State University  
B236 Clark  
Fort Collins, CO 80523-1784  
USA<sup>1</sup>

## Abstract

*Building off of the path-breaking works of Roy Bhaskar—and in particular his philosophical position of critical realism—this paper works toward a realignment of sociology with the life and ecological sciences. Sociology has been cautious of looking too far into the realm of the biophysical for causal potentials out of fear that such analyses might mark the beginning of a slippery slope toward biological reductionism. Yet, as this paper argues, such fears of reductionism are conceptually misguided. Critical realism argues that reality is stratified, rooted, and emergent. Consequently, to bracket social life from those levels “beneath” it—or, in some cases, to write out nature entirely (e.g., discursive theory and “strong” social constructionism)—is to approach the study of those phenomena with a degree of institutionalized blindness. Instead, this paper argues that sociology must open its doors to all causal potentials, regardless of where this search may lead.*

**Keywords:** *critical realism, evolution, ecology, determinism, sociology, environment*

## Introduction

Sociology’s position toward socio-biophysical analyses has often been one of resistance and at times hostility and abhorrence. Of course, this unease is understandable, even perhaps justified; particularly when such pronouncements are driven by ideologues seeking to validate, and in the end reify, the status quo (and any inequalities therein contained).<sup>2</sup> A fear of determinism—of losing the agenic capacity—is what spurs much of this disciplinary resistance of looking too far into the biophysical realm for descriptive and explanatory variables of social phenomenon. Granted, there are those who take this to an extreme, as in reducing all of reality—social and otherwise—to, say, genes (e.g., Dawkins 1989 [1976]).<sup>3</sup> But, as I argue, building off of the conceptual pos-

ulates of critical realism, calls for reductionism—whether they be atomistic (as with Dawkins’ selfish gene) or holistic (as in systems theory)—are short-sighted and ill-conceived given the emergent, rooted, and stratified properties of reality. To speak of the role of the biophysical in social theory is not to place the “booming and buzzing” reality of society in the straightjacket of biological determinism. Rather, it is to open sociological analysis to *all* the complexities of social life.

The question then becomes, in the words of Benton and Redclift (1994, 4), “how do we open up to investigation the relationship between humans and the rest of nature, without letting in the ‘Trojan horse’ of biological determinism?” An answer to Benton and Redclift’s question is forthcoming. A response is necessary if we are to move toward a sociology that is receptive to the dynamic, open, and interpenetrating relationship between the natural and social worlds. As Benton (1991) noted well over a decade ago, social phenomena, no matter how much sociologists may wish otherwise, cannot escape the complex web of biological, chemical, and physical interactions. The politics of health and well-being, of gender and sexuality, of the environment, of animal rights and welfare, and of the body represent just a few of the sites of socio-political contention that raise major questions as to the place of the biophysical in the social sciences.

Toward this end, this paper proceeds as follows. I begin with an overview of critical realism, particularly that as detailed by Roy Bhaskar. From here, I briefly overview some examples of reductionistic theorizing found in the social sciences that critical realism so poignantly rejects. The stage is now set for the final section, where a pragmatic case is made for ecologically embedding sociology. Here, guided by recent advances in the life and ecological sciences, discussion centers on how an ecologically embedded sociology can remain critical and non-foundationalistic (in terms of its knowledge claims) while leaving space for a causally efficacious biophysical realm.

## Introducing Critical Realism

The philosophical position popularly known today as “critical realism” emerged largely out of the United Kingdom in the 1970s (e.g., Benton 1977, 1981; Harré 1970, 1972; Harré and Madden 1975; Keat 1971; Keat and Urry 1975). Yet while many contributed early to this tradition, it is Roy Bhaskar’s (1980, 1993, 1994, 1997 [1975], 1998 [1979], 2002) treatment of critical realism that is considered by many to be the most systematic, complete, and influential today.<sup>4</sup>

Specifically, critical realism provides a non-foundationalist epistemology. In doing this, it acknowledges that knowledge-producing actions only make sense when the assumption of the existence of an independent material reality is granted—hence, its claim to *realism* (Benton 2001a). Critical realism does not, however, assume a one-to-one correlation between knowledge claims and reality. Consequently, it importantly denies the correspondence theory of truth (contrary to other realist positions [see, e.g., Bricmont 2001]). Toward this end, critical realism makes an important distinction between the way things are (intransitive dimension) and our knowledge claims about those objects of knowledge (transitive dimension). To conflate the two—by way of confusing statements of what we *think* is (epistemology) for what *is* (ontology)—is to succumb to what Bhaskar (1997 [1975]) calls an “epistemic fallacy.” This allows, and here is the crux of critical realism (and thus what makes critical realism *critical*), for the fallibility of knowledge claims. Thus, while critical realism allows for the concept of verisimilitude (that is, statements and theories vary in terms of their similarities to the real system they are meant to represent), it does not view the development of knowledge as something “progressive” (e.g., as linearly moving closer to universal Truths). Rather, knowledge claims should be continually critiqued, challenged, and revised as both culture and practice shape the lenses through which we view the world.<sup>5</sup>

Bhaskar develops critical realism as a sustained critique of Humean empiricism in general and his theory of causation in particular (for detailed discussions of this debate, see, e.g., Archer et al. [1998] and Collier [1989, 1994]).<sup>6</sup> Bhaskar begins with the question: “What must the structure of reality be like for scientific knowledge to be possible?” (Harvey 2002).<sup>7</sup> While it is not necessary that science occurs, since it does exist, “it is necessary that the world is a certain way. It is contingent that the world is such that science is possible... [T]hat the world is structured and differentiated can be established by philosophical argument; though the particular structures it contains and the ways in which it is differentiated are matters for substantive scientific investigation” (Bhaskar 1997 [1975], 29).<sup>8</sup> By way of this reasoning, Bhaskar arrives at the following stratified account of reality: 1) the “empiri-

cal,” consisting of experiences/observed events; 2) the “actual,” involving the flow of events produced either under controlled conditions of experimentation or as uncontrolled “conjunctures;” and 3) the “real” world of causal powers/tendencies and deep structures.

In Bhaskar’s own words:

*Explanatory science...seeks to account for some phenomenon of interest—typically an experimentally produced event pattern—in terms of a (set) of mechanism(s) most directly responsible. Producing this explanation will involve drawing upon existing cognitive material, and operating under the control of something like a logic of analogy and metaphor, to construct a theory of a mechanism that, if it were to work in the postulated way, could account for the phenomenon in question. The reality of the mechanism so reproduced is subsequently subjected to empirical scrutiny, and the empirical adequacy of the hypothesis maintained compared to that of competing explanations. Following this any explanation that is (tentatively) accepted must itself be explained, and so forth, a move which, in itself, presupposes a certain stratification of reality. (Bhaskar and Lawson 1998, 5) (my emphasis).*

Bhaskar gives further shape to this account of reality through his discussion of *rootedness* and *emergence*. As the terms suggest, “higher” level phenomena are rooted in, and emergent from, more “basic” phenomena.<sup>9</sup> This allows for the co-existence of both *being* and *becoming*. Rootedness and emergence represent a conceptual linchpin for Bhaskar because they close the door to reductionistic accounts of reality. Rather, causal tendencies between strata are multidirectional, going both “upward” and “downward.”

Accordingly, once “higher” levels emerge, they can exert influence on “lower” levels—what is referred to in the philosophy of biology literature as “downward causation” (e.g., Campbell 1990, 1994, 1997; Emmeche et al. 2000; Kim 2000; Peter et al. 2000). Stress from one’s job or marriage, for example, can affect one’s nervous and endocrine systems, thus altering chemical reactions, hormonal secretions, blood flow, and protein synthesis—in short, one’s very physiology. Even something as distinctly biophysical as the brain, it turns out, is a product of both rootedness and emergence. In the words of neurologist Antonio Damasio (2000, 108-109):

*There are not enough genes available to determine the precise structure and place of everything in our organisms, least of all the brain, where billions of neurons form their synaptic contacts. The disproportion is not subtle: we carry probably about [100,000] genes, but we have more than [10 tril-*

lion] synapses in our brains. Moreover, the genetically induced formation of tissues is assisted by interactions among cells, in which cell adhesion molecules and substrate adhesion molecules play an important role. What happens among cells, as development unfolds, actually controls, in part, the expression of the genes that regulate development in the first place. As far as one can tell, then, many structural specifics are determined by genes, but another large number can be determined only by the activity of the living organism itself, as it develops and continuously changes throughout its life span” (my emphasis).

Critical realism thus provides us with this crucial reminder, which social and natural scientists would do well to remember: causality is not unidirectional, but rather has the potential to flow in both directions—both upward and downward (and, as is often the case, synergistically). Psychobiologist Henery Plotkin (2003, 100) readily admits that culture cannot be located within genes alone, but rather is “an adaptive supertrait built upon—emerging from—cognitive mechanisms, some of which themselves might be emergent supertraits...on a scale [of complexity] unlike any other encountered by biologists.” Thus the need for the “higher” level social sciences; for ultimately, the “higher” level phenomena that they study cannot be explained away with references to particle physics or genetic sequencing alone.

Bhaskarian critical realism is thus not to be confused with other realist positions. According to critical realism, anything belonging to an extended—or “higher”—level of emergent strata can still be influenced by “lower” level strata. Some realists, such as Dawkins (1989 [1976], 1986)—through his so called “selfish gene” theory—take this to mean that “lower” mechanisms, once they are established, can be held to explain all relationships that occur at “higher” levels (and it is here where realism has gotten a particularly bad rap among social scientists). Such a position, however, ultimately leads to the (incorrect) conclusion that, in time, and with enough knowledge, all of “the sciences” can be reduced to some basic structure or law(s) of the universe—be it (“selfish”) genes, quarks, or quantum mechanics.

While these reductionistic positions typify the classic argument for upward causation in nature, they run counter to the position laid out by Bhaskar. At best, “lower” level sciences only explain the constitution and boundary conditions of “higher” level potentials (this point is revisited later when discussing the concept “norm of reaction”). They cannot explain, *in toto*, how or why those potentials are actualized or exercised. Chomsky’s (1971, 1975, 2000) hypothesis of a universal grammar, for example, posits a structure with onto-

logical “depth” that allows for language to develop in an “underdetermined” fashion. Yet having the neurological capacity to acquire language says nothing to whether one will actually acquire language, nor does it indicate which language(s) they will acquire. Or take the heated debate surrounding genes and sexual differentiation. While genotype undeniably plays at least some role in shaping sexual differentiation, it is by no means the Aristotelian final cause of secondary sex characteristics.<sup>10</sup> Rather, just as we must be careful not to remove the body from its materiality, we must likewise take care not to separate the corporeal body from its socio-cultural milieu (e.g., the political economy, gender relations, class dynamics, the cultural economy) when attempting to understand the “how’s,” “what’s,” and “why’s” of *doing* gender (Birke 2000, 2003).

It should now be clear why Bhaskar is opposed to Humean empiricism: causation cannot be reduced to models of succession events, regularities, and repeat occurrences. Rather, explanation depends upon identifying causal mechanisms, understanding how they work, and under what conditions they do so. Explanation thus depends upon identifying “powerful particulars.” As Rom Harré (2001, 22) recently argued: “Causality is not Humean concomitance but agential. The fact that the power is not observable in itself, but only in its effects, is, as Thomas Reid pointed out two hundred and fifty years ago, not an adequate ground for throwing it out as a sound ontological concept.”

In “closed systems,” conditions are such that the object possessing the causal power in question is stable and the external conditions situating the object are constant. Such conditions are themselves quite rare throughout much of the natural world, however (although the natural sciences can often artificially mimic such “closures” in experiments): for instance, the temperature at which water boils varies by altitude; the speed at which sound travels varies according to the medium through which the wave is traveling; even the so-called “fine structure constant” of the universe (or “alpha”) has shown to have been changing since the dawn of space-time (Choi 2002). In the laboratory of the social world, however, such “closed” conditions are non-existent (nor can they be achieved through acts of human engineering). Within such “open systems” we find reciprocally interactive entities and causal mechanisms, which oftentimes can only be seen in their effects. We must therefore be careful not to artificially seek (causal) “closure” in a system that is anything but closed. Rather, descriptive and explanatory accounts of social phenomena must remain “open” to the ecologically embedded reality that is part and parcel of the world in which we reside (Bhaskar 1993).<sup>11</sup>

## Moving Beyond Discursive Reductionism

Over the course of the last few decades, we have seen in sociology a strong push to write out nature entirely by viewing it as an extra-social or extra-discursive force (e.g., Butler 1989, 1994; Foucault 1980, 1981, 2000; Lorber 1994; Risman 2001). While most social scientists are well aware of the perils of biophysical reductionism, and know what such reductionism looks like, rarely do we stop to reflect upon the types of reductionism that the social sciences pronounce and perpetuate. Before moving on to the final section, where a pragmatic case is made for the need of an open articulation between the “social” and “natural” sciences, allow me to highlight what socio-cultural reductionism can (and does) look like. In this section, I briefly sketch two positions that can be viewed as presenting a radically discursive (or socially constructed) ontology: the first, Foucaultian poststructuralism, followed by Judith Butler’s discursive reduction of gender.

### Foucaultian Poststructuralism

Foucault (1997, 224) sought to “sketch out a history of the different ways in our culture that humans develop knowledge about themselves: economy, biology, psychiatry, medicine and penology.” He does this through an analysis of discourse. When speaking of “discourse,” it is important to keep in mind that Foucault (1972, 48) was not speaking about “a mere intersection of things and words: an obscure web of things, and a manifest, visible, coloured chain of words.” In addition, he was also referring to those “practices that systematically form the objects of which they speak” (Foucault 1972, 49).

According to Foucault, then, the discursive relations of power/knowledge represent the fundamental tapestry of social life; a conception of reality where discourse rests upon more discourse “all the way down.” Toward this end, some have gone as far as to argue that Foucault lapses into a form of postmodern functionalism, where all is the product of discourse, but where discourse itself is no product (Carolan and Bell 2003). Such a framework presents a picture of social life where discourse is analytically prior to, and a causal force upon, everything else, including the biophysical realm (a point that sociologists of the body have likewise repeatedly emphasized in their critiques of Foucault [e.g., Bartky 1997; Shilling 1993; Turner 1991, 1996]).

This is not to suggest that Foucault says nothing of materiality, particularly in his writings on the body. But the body for Foucault is only accessible through an investigation of how the discourse of “the body” emerges out of the (medical) sciences, thereby reducing it to effects of discourse and its corollary components of power/knowledge (Carolan

2005b). In the words of one of the foremost scholars of the body, Bryan Turner (2003, 275): “Foucault’s analysis of the human body was an attempt to show that the ‘body’ was a contingent effect of power rather than a fact of nature.” This is not to suggest that Foucault (e.g., 1979) was unaware of the role of material technologies of discursive inscription in modern societies—as in, for instance, his discussion of the Panopticon. But such materiality was not given independent *a priori* causal force to the emergent regimes of power/knowledge. Instead, such material instruments were viewed by Foucault as the practical effects of power/knowledge that have become, in the words of Deleuze and Guattari (1987), “mineralized” at a specific point of space, place, and time (Carolan forthcoming). In short, the realism that Foucault posits is a thoroughly discursive one, which leaves little space for extra-discursive powers.

### Butler’s Discursive Reduction of Gender

Butler argues that gender is not an objective thing. Rather, it is performed: “gender reality is performative which means, quite simply, that it is real only to the extent that it is performed” (Butler 1990, 278). But she takes this discursive turn even further by then questioning the distinction between gender and sex: where the latter refers to the corporeal “facts” of our existence, while the former speaks to the social conventions that shape how we “do” gender. Accordingly, Butler (1993, 2-3) argues that sex is not “a bodily given on which the construct of gender is artificial but [...] a cultural norm which governs that materialization of bodies.”

For Butler, our very conceptions of reality—of what *is*—is forever shaped by language: “there is no reference to a pure body which is not at the same time a further formation of that body” (Butler 1993, 10). Consequently, “‘sex’ becomes something like a fiction, perhaps a fantasy, retroactively installed at a prelinguistic site to which there is no direct access” (Butler 1993, 5). Sexed bodies, in other words, are social constructions, materialized through discourse. Gender thus becomes something we iteratively perform, and the construction of sex is merely an episode of that performance (New 1998). Yet by highlighting this so-called “corporeal fiction,” which we have come to know as the material body, Butler strips it of all *a priori* (extra-discursive) causal powers.

In making this argument, however, Butler (like Foucault before her) conflates ontology with epistemology and thus commits an “epistemic fallacy.” Yes, the practical, conceptual, and linguistic means we use to grasp the world are historically relative. Our knowledge of the world is (and will always be) mediated and culturally impregnated (to various degrees). But the characteristics and forces that such knowledge references (regardless of verisimilitude) are independent of our means of knowing—that is, they are real. Mak-

ing this important distinction (which critical realism, for instance, does) between the transitive (epistemology) and intransitive (ontology) dimensions allows for judgments between knowledge claims to be made. While it is well established that there are no self-evident, universal criteria for evaluating knowledge claims (Gieryn 1983, 1995; Jasanoff 1987; Kuhn 1970 [1962]; Mulkay 1976), epistemic judgments can still be made according to the internal coherences of the accounts (Bhaskar 1980, 1997 [1975]; Lawson 1997; New 1998) and their concomitant practical effects (Evans 2005; Jasanoff and Wynne 1998; Wynne 1992).

In the end, those who deny the independent existence of a causally efficacious biophysical realm lose all critical purchase to oppressive exercises of power, especially those involving anything *physical* in nature—from torture, to genital mutilation, to contaminated water and polluted air (Soper 1995a). That is, if there is no biological substratum lending experiential force to, say, pain, then “pain” becomes a mere discursive construction, which can thus be reduced through either discursive denial or discursive reconstruction (Soper 1995b). Such projects of “strong” social constructionism are consequently, upon closer inspection, radically uncritical in character due to their inability to “say *what* oppression is bad *for*, or what it does damage *to*” (Sayer 2000, 98).

Sociology has been correct to take care so as to not legitimate ideologically-driven biological determinism and the socio-political ramifications that accompany such proclamations. My concern, however, is that this apprehension toward determinism has become too one-sided. With all of our hand-ringing about the dangers of “bringing nature back in” to sociological analyses, due to its deterministic undertones, we remain incorrigibly blind to the other side of the coin: to the appalling prospects of an equally dangerous cage—cultural determinism. In our rush to condemn the fixed-biological, we often (mistakenly) overly prescribe mutability and fluidity to the socio-cultural. We must thus stay vigilant to all prescriptions of determinism—biological and otherwise—while concomitantly remaining open to the multidirectional causal potentials that constitute a stratified, rooted, and emergent reality.

### **Making a Case for an Ecologically Embedded Sociology**

To close off society from its substratum—or in some cases to write out nature entirely—has led to, borrowing from Nietzsche (1983 [1874], 74), a social scientific orientation that suffers from “epistemological myopia”: for “most of what exists it does not see at all, and the little it does see it sees much too close up and isolated; it cannot relate what it sees to anything else and it therefore accords everything it sees equal importance and therefore to each individual thing

too great importance.” The world is indeed a highly symbolic and social one, this cannot be denied. But social phenomena possess a rootedness that sociologists would do best not to forget (see, e.g., Freedland and Isaacs 2005; Freese et al. 2003; Massey 2002; van den Bergh and Stagl 2003). Sociology is the study and explanation of social phenomena; one need only look toward the nearest introductory text for support of this claim. But somewhere along the way this boundary condition has become convoluted; misinterpreted to the point where sociology is now taken in many circles to be the study of social phenomena via *only* the explanatory power of other—biophysically *dis*-embedded—social variables.

In this section, the argument is made that sociology’s long term viability depends upon “opening” our sociological imaginations to the efficacious forces of the biophysical realm. This argument is informed by recent advances and exchanges between the life and ecological sciences. In what follows, I lay out for discussion empirical sites which problematize the so-called “nature-society divide.” This discussion is organized into four sections. The first speaks to the non-linear exchange between ecology and society. This is followed with a brief discussion of the relationship between environmental toxins, behavior, and patterns of social organization. Attention then turns toward the concept of “environmental degradation.” Here, the case is made that environmentally-inclined social sciences (e.g., environmental sociology) could improve their critical authority if only they were not so quick to limit their investigations to the upper stratum of reality, but instead remained open to the casual potential of biology and ecology. Finally, an example of just such an ecologically embedded sociology is presented in the form of Peter Dickens’ recent articulation between Marxism and the biological sciences.

#### **Ecology and Society**

Society is more than mere interrelationships (or “networks” [e.g., Castell 1996, 1997, 1998, 2000] or “flows” [e.g., Urry 2003]) between people or social roles. It is also those interrelationships and natural stocks and sinks, land, agricultural ecosystems, tools, buildings, and non-human animals. In other words, society is constituted of “lateral” and “horizontal” integrations between non-human beings, ecosystems, and the co-structuring relations between individuals (Benton 1994, 2001a, 2001b). Mounting research, for example, has sought to highlight the relationship between genes and culture (e.g., Dawkins 1986, 1989 [1976]; Pinker 1994, 1997, 2002; Pinker and Bloom 1990; Plotkin 2003; Rose 1992, 1997; Wilson 1975, 1978, 1998). What I shall focus on in this section, however, is the growing literature documenting the “downward” tendencies of culture *on* genes (this relationship is revisited in the conclusion).

For instance, it is generally acknowledged in the ecological sciences that culture can alter selection pressures on sickle-cell anemia (Bodmer and Cavalli-Sforza 1976; Diamond and Rotter 2000; Stephens et al. 1998). This can occur, for example, as society shifts—over the course of multiple generations—its farming or building practices, which can change a group's exposure rate to malaria-carrying mosquitoes (Diamond 1989). Another well-known example involves how changing cultural selection pressures on genes can affect lactose tolerance. Among humans there are both “absorbers” (lactose tolerant) and “malabsorbers” (lactose intolerant). The proportions of malabsorbers and absorbers in human populations are related to the prevalence of dairy farming within a society (Durham 1991; Feldman and Cavalli-Sforza 1976). As noted by Ehrlich (2000, 64), “as selection led to more individuals in dairying cultures being absorbers...the idea of continuing to drink the milk of nonhuman animals after weaning would itself have spread, as it evidently did.”

The increasing prevalence of childhood asthma (and allergic reactions more generally) over the twentieth century, particularly in the West, offers another example of exchange between social and biophysical strata. Research points to a complex relationship between patterns of social organization and the evolution of certain genes involved in the body's immune response to various environmental phenomena (Davey 2003; Strachan 2000). It is postulated that the immune system in infants becomes “educated” by early and repeat exposure to respiratory bacteria and micro-organisms, which leads to the development of lifelong immune-response mechanisms. Yet the shift toward post-World War II affluence (among other things), may have short-circuited this development by cutting off the body's contact with various micro-organisms. By reducing exposure to the numerous microbes our ancestors regularly came into contact with as children, our immune systems, it is argued, fail to learn how to keep from overreacting when exposed to even innocuous phenomena (like, say, peanuts) (which is not the same as the “hygiene hypothesis” of the late-1980s and 90s, which is beginning to be rethought) (Hamilton 2005; Holt et al. 1997). The asthma epidemic may thus be the body's protective response to respiratory bacteria that are reaching ever higher levels due to broader social changes in both public and private realms.

Or take the so-called “Flynn effect” in cognitive testing: the well-documented increase in the global mean IQ (intelligent quotient) at the rate of approximately three points per decade (although recent research suggests such a trend may be leveling off [Brown 2002]).<sup>12</sup> Of course, there are any number of explanations for this—from better diet and nutrition, to the development of a social milieu which has radically shifted our engagement with the world from one of concrete relations to one of heightened abstraction, to issues po-

tentially related to the instrument of measurement itself. In the end, considerable debate remains as to its cause. Given the rapidity at which this process is occurring, though, it is highly unlikely that it could be explained away with mere reference to some inherent, fixed biophysical constitution. What we are likely seeing in the “Flynn effect,” then, is the effect of a complex dynamic between socio-cultural and biophysical strata—an effect involving both “upward” and “downward” causal forces.

Finally, the etiology of disease has also changed considerably as the globalization of air travel, food systems, and economic markets further compress time and space. As human population grows, as urban areas continue to push further into the hinterlands, and as we become increasingly mobile more people are expected to be exposed to large “animal reservoirs of disease organisms” (Ehrlich 2000, 282). When attempting to understand the etiology of HIV, for example, one must not forget such *social* processes as the proliferation of international air travel, urban sprawl, social inequality, and the pressures placed on chimpanzee populations due to excessive hunting.<sup>13</sup>

Even the foot and mouth (FM) epidemic that visited the UK in 2001 can ultimately be understood as a socio-biological effect. The variant that caused this epidemic seems to have originated in South India in the early 1990s, most likely in a mutation arising from the domestication of animals (House of Commons Committee on Environment, Food and Rural Affairs 2002). From here, it spread as a result of both legal and illegal international trade of meat and meat products (Lowe et al. 2001; Nerlich 2004). But also, its dispersal has been linked to a WTO policy that distinguished between three types of countries: those countries with FM, those countries free of FM with vaccination, and those free of FM without vaccination (Law and Singleton 2004). According to this policy, countries free of FM and not practicing vaccination could achieve disease free status after a twelve month period being free of outbreak. In the case of an outbreak, if it can be controlled and immediately stamped out, then disease free status can be regained after three months of the last slaughter of diseased stock, followed by serological surveillance. As others have noted (Campbell and Lee 2002; Law and Singleton 2004), the Code thus offers considerable market advantage to those countries that are FM free and who likewise choose not to vaccinate. The UK was thus slow to vaccinate in order to have such an advantage, which contributed to the extent of the epidemic.

When seen in this light, it is important for us to think of illnesses, diseases, and epidemics as being more than purely biophysical artifacts. Rather, we need to expand our understanding of both the *being* and *becoming* of these phenomena to include social and cultural variables. In other words, to see

them as socio-material effects, which affect individuals who are organically embodied but who likewise possess psychologically and socially embedded attributes that are causally linked in both the etiology and prognosis of disease (Benton 1991, 2000, 2003).

### **Environmental Toxins, Behavior, and Social Organization**

While we are aware of the potential health threats of environmental pollutants to the human body, we are only beginning to understand the impact that toxins have on human behavior and patterns of social organization. There is growing interest, for example, in lead and manganese neurotoxicity. Recent epidemiological research has linked some cases of Attention Deficit Disorder (ADD) and Attention Deficit Hyperactivity Disorder (ADHD) to heavy metal exposure—namely, lead and manganese (Hammond 1998; Needleman and Gatsonis 1991; Needleman et al. 1990; Walker 1998). This is due to lead's ability to downregulate dopamine and glutamate whereas manganese downregulates serotonin (Masters 2001). While this does not suggest that all cases of ADD or ADHD can ultimately be traced to heavy metal exposure, there are examples in which lead removal has been used to effectively replace drug treatment for certain ADHD/ADD cases (Juberg 1997; Masters 2001; Walker 1998).

Research has also linked lead and manganese exposure to aggressive behavior (e.g., Denno 1994; Fulton et al. 1987; Land et al. 1990; Needleman 1996; Needleman et al. 2002; Nevin 2000; Pihl and Ervin 1990; Stretesky and Lynch 2004). For example, Masters and colleagues reviewed six different studies comparing levels of heavy metals absorbed by both violent offenders and nonviolent offenders in the same prison (Masters 2001; Masters and Coplan 1999; Masters et al. 1998). In every case, they found that lead or manganese was significantly higher in violent offenders than in the non-violent offenders.

Clearly, the affects of neurotoxicity on behavior are tenuous; I do not intend to suggest otherwise. But they are tenuous, in part, because the links and relationships have yet to be thoroughly examined. The potential affects of toxins on behavior are, however, intriguing, and present a whole new realm for sociological investigation and imagination if explored carefully.

This brings us to endocrine disrupters: It is well-documented that endocrines—testosterone and estrogen—can influence behavior and primary sex characteristics (Masters 1983; Udry 1995). Endocrine disruptors (such as DDT, kepone, lindane, some PCB congeners, several dioxins, cadmium, lead, mercury, alkyl phenols, and diethylstilbestrol) are those industrial externalities that have the ability to mimic and/or disrupt “natural” endocrine processes, causing, for in-

stance, the lowering of testosterone in males and estrogen in females. In endocrine disrupters, then, we have an artifact from a distinct socio-organizational configuration (namely, [late] industrial society), the affects of which have the ability to “reach down” and affect such “lower” level phenomena as physiology (Colborn et al. 1997). As noted by one scholar, these phenomena are changing “the very characteristics that make us human” (Cortese 1996, 214).

Finally, many of us are already quite familiar with the dangers of mutation and other health risks that accompany radiation exposure. Perhaps less known, however, is the degree to which such mutations may become inherited and passed on to future generations. While the findings are still tentative, research has recently suggested that radiation exposure could lead to the development of unstable genomes, which have the potential of reaching as far into the future as one's fourth generation of offspring (or one's great-great-grandchildren) (Dubrova et al. 2000). Indeed, in one epidemiological study, this was cited as a possible explanation for a leukemia cluster recorded around Britain's Sellafield nuclear plant (Muir 2002).

The links between environmental toxins and social organizational patterns cannot be ignored *prima facie*. These relationships are empirical in nature and must be assessed accordingly (e.g., Freedland and Isaacs 2005; Stein et al. 2002). Indeed, the study of these relationships become all the more pressing in light of the fact that poor and minority groups are more likely to be exposed to harmful pollutants (Bullard 1995; Daniels and Friedman 1999; Lanphear 1998; Lynch et al. 2004a, 2004b; Massey 1994; Stapleton 1994; Stretesky 2003; Szasz 1994).

### **Environmental Degradation**

Often we think of environmental problems, such as “erosion,” “overpopulation,” “deforestation,” “urban sprawl,” “the heat-island effect,” and “global warming” as unproblematic objects that are readily quantifiable and measurable. Rarely, however, do we view them for the “black boxed” concepts that they are—as concepts that have been imposed on reality for purposes of explanation and understanding rather than being one-to-one reflections of the reality they are said to represent.

Such “black boxing,” for instance, can be readily seen in statistical techniques of ecological prediction. Granted, environmental scientists, the public, policymakers, and the like rely heavily upon statistical predictions of ecological degradation when seeking to establish the current state of the environment. Such predictions have resulted in numerous widely read (and cited) books over the years—from *Limits to Growth* (Meadows et al. 1972) to *The Skeptical Environmentalist* (Lomborg 2001). Yet, while widely read and debated,

rarely do such discussions take a step back to problematize the taken-for-granted concepts that are widely seen as capturing instances of ecological degradation—such as “deforestation,” “urban sprawl,” “diversity” (or lack thereof), “overpopulation,” and the like. If we were to do this, we would find that they are often un-reflexively couched within the existing terms of environmental discourse and scientific concepts in ways that seek to impose structure and order onto complex realities (Jasanoff 2004). Admittedly, we need these terms so as to be able to talk about particular states of reality—namely, those related to (anthropogenic driven) changes in the natural environment. Yet, something inevitably gets lost in translation when we transform a complex socio-biophysical effect into an unproblematic object that can be readily measure and quantified (Carolan 2004, 2005a; Ravetz forthcoming). This, of course, is not to deny the underlying reality (or referent) of these scientific claims—forests, for instance, are being lost at a rapid rate, cities are expanding ever outward, and the population of this planet is increasing. The problem, however, occurs when we naturalize, and thus “black box,” these concepts; a move that also often adds to the socio-political conflicts that surround them (Carolan 2005a; Forsyth 2003).

Let us look briefly at the phenomenon of “erosion” (see also Forsyth [2003]). In biophysical terms, erosion refers simply to the movement of soil from one place (e.g., Midwest farmland) to another (e.g., the Mississippi Delta). Clearly, there are negative consequences associated with this phenomenon: from the removal of nutrient rich organic matter from productive farmland (although such may likewise increase the fertility of soils “downstream”), to the role it plays in increasing the intensity (and frequency) of dust storms, to the unwanted depositing of soil (Troeh 2004). Yet the question remains: given the ease in which we speak, quantify, and record this “thing” we call erosion, is it really as universal and un-problematically quantifiable as we make it out to be? Perhaps not.

Research conducted in the Himalayas, for example, points to a number of biophysical factors that may contribute to soil erosion: from the role of tectonic uplift and monsoonal rainfall to the impact of naturally occurring gullies on steep slopes (Hofer 1993; Ives and Messerlie 1989). Studies conducted in northern Thailand likewise problematized the assumption that population growth and poor farm management practices were the main driving forces behind soil erosion in mountainous zones (Forsyth 1996, 1998). This research revealed that local geomorphological processes had, over time, led to the formation of deep gullies across the landscape in the Chiang Rai province. These gullies, which predate agriculture, were likely more effective conduits for lowland sedimentation from the highlands than were highland agricultur-

al fields. This led the researcher to conclude the following: “It is therefore likely that much lowland sedimentation from the highlands was the result of naturally occurring rather than agricultural practices” (Forsyth 2001, 151).

By better understanding the ecology of environmental problems, social scientists can prevent themselves from falling into the very trap that they seek to prevent others from falling into—that is, the trap whereby aspects of nature become naturalized, essentialized, and fixed. In viewing soil erosion in this light, we must acknowledge the multitude of biophysical—and not just social—causes behind it (Schjønning et al. 2004). Thus, instead of viewing “soil erosion” as a uniform, universal, and largely (if not purely) anthropocentric (social) phenomenon, we must work to understand the complex local geophysical processes that may also lay behind it.

To do otherwise may in fact result in the opposite outcome than that which is socially and locally desirable. For example, attempts to combat erosion through reforestation (a common strategy employed around the world to reduce sedimentation) have in some cases actually *increased* lowland sedimentation because these strategies overlooked the relationship between sheet and gully erosion and the role of farmers in reducing runoff (Calder 1999; Driver 1999). There are, in other words, no universal “fixes” to the problem of erosion, because there is no universal, one-size-fits-all way to understand erosion. Rather, it is a complex phenomenon that is embedded, not only socially and biophysically but also locally.

### **Peter Dickens: Critical Realism and Marx’s Theory of Subsumption**

In recent years, sociologist Peter Dickens (e.g., 2000, 2001a, 2001b) has looked toward Marx’s theory of subsumption to inform potential linkages between the biological and social sciences. According to Marx (1970), capitalism inevitably leads to what he describes as “real” and “formal” subsumption, in which the latter details the direct subordination of labor to capital and the former expresses the loss of individual autonomy. Dickens (2001a, 106) seeks to push this idea even further, suggesting a more radical interpretation: namely, that “capitalism, in conjunction with the various forms of biological predispositions...may over the long term have been shaping human biology in its own image.” In forwarding this argument, Dickens is careful, however, not to subscribe to a type of neo-Lamarckianism. Rather, he looks to how biological tendencies can be shaped, and ultimately changed, by broader social and environmental processes.

One such area detailed by Dickens is human development. For example, Dickens points to epidemiological work that suggests that the effects of the mother’s environment are



transmitted to her yet unborn child—what he likens to a type of “weather-forecast.” According to Dickens (2001a, 102), these “weather forecasts” have been shown to be transmittable from one generation to the next: “the capacity of a woman to nourish her fetus being in part determined by that woman’s own intrauterine experience.” The implication being that inadequate pre-natal care—which can alter “normal” (in developmental terms) rates of metabolism, hormonal excretion, blood flow, and the like within the uterus—can lead to a poorly “adapted” (in evolutionary terms) individual, and perhaps with time, if conditions are not improved, an entire class. The Marxian interpretation of this is that this points to yet another “contradiction” of capitalism (in terms of it being a contradictory internal logic)—which, in this case, is the rise and perpetuation of a working-class of ill-health.

Dickens also speaks of post-natal development. Drawing from an earlier piece by Buck-Morss (1982), who writes of the Western bias in the work of Jean Piaget, Dickens applies a critical realist interpretation to Piagetian psychology. According to Dickens, a key ingredient of capitalism is the need to separate abstract thought from more experiential forms of thinking (with the former now being valued over the latter), so that these activities can be allocated to different groups of individuals. And indeed, in modern capitalist states, children are quickest to grasp abstract ideas. Thus, rather than as some concrete universal, detached from “higher” level strata, the suggestion is that new light could be shed on Piaget’s work on ontogeny when informed by a critical realist framework through Marx’s theory of subsumption. Dickens argues that the modern capitalist state plays an equally important role to biology in a child’s development of abstract thought. The deep structure/tendency for abstract thought is thereby present, but how it is actualized and exercised is shaped by such “higher” level phenomena as the political economy, culture, as well as other dynamic intersubjective processes. In the words of Dickens (2000, 104):

*The emerging power of abstract knowledge can be partially attributed to the growth and extent of ‘commodity abstraction.’ Much of life in capitalist society is organized around abstraction. In particular, the buying and selling of goods means that people are constantly obliged to think in terms of a subtract substance known as ‘value.’*

### Other Potential “Flavors” of Critical Realism?

In all, the above research lends support to those calls from within the discipline of sociology to “bring nature back in” (Catton 1992). But how does critical realism move us in

this direction any more convincingly than if we were to simply say that “the social” and “the natural” mutually influence each other—a position that can be found in such heuristics as “conjoined materiality” (Demeritt 1998), “conjoint constitution” (Freudenburg et al. 1995), “coevolution” (Norgaard 1994), and “ecological dialogue” (Bell 1998)? And secondly, should we speak as if there is only a single critical realism or can we talk of different critical realisms—as perhaps there being different “flavors” of critical realism? Allow me to briefly address the former question first, regarding the multitude of heuristics that are already out there in the literature, which will then lead to the answering of the latter question.

To begin, one problem with the above heuristics is that they present positions of realism, *prima facie*. Like critical realism, they do not deny the causal significance of materiality. Unlike critical realism, however, they merely presuppose their realist assumptions to be true, *a priori*, without philosophically and logically supporting such a position. Such heuristics also leave open other salient questions. For example, when speaking of “the social” and “the natural,” are we referencing analytic, conceptual, or ontological categories? Where does the phenomenon of emergence fit into such conceptual heuristics? And if emergence is not addressed, how then would one explain the asymmetrical ontological relationship between the two “realms”—e.g., the biophysical can exist without the social but the social cannot exist without the biophysical? What we find in these heuristics, then, is an implicit “epistemic fallacy,” for upon closer inspection they mistake epistemological claims for ontological ones.

For these reasons, such heuristics are philosophically and logically problematic. Yet pragmatically speaking, these critiques need not take away from their explanatory and descriptive potential. What Bhaskar has done for us is, if you will, the philosophical heavy lifting. He gives us a logically coherent argument for the independent existence of a material reality that is constituted of non-linear causal forces that are directed both upwards and downwards. In doing this, he shows us that we can have an ecologically embedded sociology without falling into the thralls of biophysical reductionism.

This brings us to the second question: can we say, then, that there are different “flavors” of critical realism? Answer: Yes, we can. Allow me to now elaborate on this by incorporating into the discussion another (critical?) realist position that is frequently used in the social sciences to understand human and non-human interaction. The position that I am referring to is the “realism” of Bruno Latour (1987, 1988, 1993, 1999, 2004a, 2004b) (recognizing, however, that he has been called everything from a “direct,” “naïve,” and “neo-” realist [e.g., Collins and Yearley 1992; Elam 1999], to neither a realist nor idealist [e.g., Bloor 1999]).

First, as with the previously mentioned heuristics, the work of Latour falls to similar criticisms. Latour (1999, 30) wants “to know how the sciences can be at the same time realist and constructivist,” much like Bhaskar. Yet such a query leads Latour to commit an “epistemic fallacy” of his own, for he speaks of ontology only through the door of epistemology. Of course, Latour would respond that his position is not to deny “reality” but to recognize “it” (whatever it may be or lend itself to be) as mediated, inevitably, by culture and practice. And in doing this, he provides space to allow for a merging with Bhaskar’s position, who concomitantly presents a realist philosophy of science to explain why science itself is possible in the first place.

In this light, the works of Latour and Bhaskar are quite complimentary. For Latour, Bhaskar provides a much “firmer” realist philosophy of science, which could greatly help to undermine those “anti-realist” chats that have haunted Latour since he and Woolgar published *Laboratory Life* in 1979 (e.g., Niiniluoto 1991). As others have noted, while Latour’s writings have been instrumental in highlighting how historical actors and societies have “worked” to define the boundaries between nature and society, they can have a disempowering effect on realist scientific explanation, measurement, and prediction due to an under-theorized realist foundation (Forsyth 2001). Toward this end, Bhaskar’s realism injects greater critical force into those Green technoscience critiques—that have accompanied the “Latourian turn” in STS (Science and Technology Studies)—by allowing those positions to still turn to science for political and intellectual ends (e.g., to still be able to “scientifically” demonstrate that ecological degradation is occurring) (Benton 2001b).

On the other hand, in Latour we find a much more detailed and (arguably) more sophisticated discussion of how power, capital, political persuasion, practice, and discourse distort our “access” to that independent reality. Through Latour, we are able to speak more forcefully about how the boundaries between the “social” and “natural” sciences came about and the practices that continue to “purify” the various disciplines. Latour thus provides a detailed sociology of knowledge which need not contradict the fundamental conceptual underpinnings of Bhaskar’s equally sophisticated realist philosophy of science.<sup>14</sup> In all, discursive space can be found in critical realism to speak of the Latourian “hybrid”—as well as Latour’s “sociology of translation”—without any contradiction to its stratified, emergent realist claims (for examples of such work, see, e.g., Carolan 2004, 2005a).

In the end, Bhaskarian critical realism presents the most thorough and sophisticated justification for a realist philosophy of science, without the reductionism of more orthodox realist accounts. Having provided this needed philosophical groundwork, there then becomes many ways to “do” critical

realism (see, e.g., Lopez and Potter 2001). The commitment to this type of framework lies in viewing reality as a complexly stratified, open system which, while real—that is, it exists independent of our knowledge of it—can only be known to us through the ever-distorting lenses of culture, history, and practice.<sup>15</sup>

To conclude this section, let us now briefly turn attention to a well known sociological work that, at one point, illustrates the degree to which the descriptive and explanatory powers of sociology can be undermined when its practitioners are not open to the possibility of a causally efficacious biophysical realm. The work of which I speak: Kai Erikson’s (1995) highly acclaimed book, *A New Species of Trouble*. In this work, Erikson writes of a small group of native peoples—the Ojibwa—living in northwest Ontario. Grassy Narrows, as the community was called, was deeply troubled. In 1970, it was discovered that twenty thousand tons of methylmercury had made its way down a local river to the community of Grassy Narrows (the same river the Ojibwa drank and fished from). For those unfamiliar with methylmercury, it is a particularly toxic beast. Its presence cannot be detected by human sense alone, and it hides deep within the organs of the human body, where it can inflict the most damage. Indeed, the outward effects of mercury poisoning are often mistaken for something else entirely, allowing its destructive affects to go unimpeded, often until it is too late. And here, as they say, was “the rub” for the troubled community of Grassy Narrows.

For various socio-historical reasons, alcohol abuse also ran rampant among the Ojibwa (in fact, between 1974 and 1978, 80% of all deaths in Grass Narrows could be attributed in various degrees to alcohol abuse). The situation, however, becomes even more disturbing when one realizes what the clinical symptoms of mercury poisoning are: impaired vision and hearing, slurred speech, loss of memory, loss of fine motor skills (e.g., impaired walking and hand coordination), tremors, and mood swings. Yet, do not those symptoms also parallel remarkably well with the actions of someone who is intoxicated? The question thus implied by Erikson is this (although not explicitly framed as such): how does one tell the difference between an individual who is inebriated from one who is displaying clinical signs of mercury poisoning? And of all those supposed deaths linked to alcohol abuse—could they have perhaps been the effect of something much more toxicologically sinister?

While Erikson’s (1995) thoughtful analysis of the Ojibwa peoples is not informed by critical realism, it does highlight how social reality may not always be as it seems, even if all the social causal variables appear to be in place. At first glance, it appears as though one could quickly and unproblematically link such aforementioned individual behaviors to

the rampant levels of alcohol abuse—which itself could be a proxy for various social stressors—occurring among the male population of the Ojibwa peoples. This would certainly provide a nicely packaged explanation for what Erikson witnessed—namely, the high rates of divorce, suicide, and domestic abuse. Incidentally, Erikson notes that such were the “findings” of certain earlier studies. Yet Erikson, rightfully, questions these conclusions. While he speaks not of critical realism, nor of realism of any sort, he acknowledges that, at least in this particular case, something else might be of causal consequence.

Of course, he goes no further than to suggest a causal potential within methylmercury to understanding the social problems that plagued the people of Grassy Narrows. But it does beg the question: what if such variables were at play? This then begs further questions, particularly salient to the argument at hand: such as, how many sociological analyses have unintentionally let such potentially efficacious biophysical variables fly below their methodological radar screens; and to what effect has been this epistemic rigidity—from, for example, ill-informed policy to questionable theory construction and testing?

### Conclusion: Bridging the Divide

Admittedly, while the examples used in the preceding section were to illustrate cases of reciprocal causation between social and biophysical strata, they tended to favor downward causation more so than upward—such as by highlighting social selection pressures on genes rather than the affect of genes on, say, social behavior. This move was a strategic one. It was done in aim of those individuals whom, while sympathetic to arguments for letting “nature back in,” remain hesitant of granting nature too much causal efficacy—such as, perhaps, is found in those arguments couched in an evolutionary framework. Importantly, however, while critical realism rejects arguments of both biological and cultural reductionism, it embraces theories of non-deterministic coevolution as being consistent with its stratified and emergent view of reality. And this includes some of those positions grounded in an evolutionary framework. Allow me to now remedy this deficiently by briefly turning attention to some of these coevolutionary arguments and concepts.

Coevolution can take the two following forms. The first acknowledges the role of biophysical strata in establishing boundary conditions. We can think of this as representing a “weaker” program of coevolution. For instance, the potential is not there for us to breathe underwater without the aid of technology, no matter how much we try to socially construct otherwise. The same can be said for our inability to digest grass or to live much beyond 120 years. And most social sci-

entists are willing to grant this much when speaking of the biophysical as playing a real role in shaping social life. The second form of coevolution, however, is more contentious: namely, those socio-biophysical accounts that seek framing within a coevolutionary framework (e.g., Catton 1998, 2002; Crippen 1988; Crippen and Machalek 1989; Dickens 1998, 2000, 2001a, 2001b; Dietz et al. 1990; Dietz and Burns 1992; Freese 1997a, 1997b; Lopreato and Crippen 1999; Machalek 1999; Maryanski 1987, 1992, 1997, 1998; Maryanski and Turner 1992; Turner 1996, 1997, 1999, 2002). This represents a “stronger” program of coevolution.

Granted, while we should approach all models of human behavior and socio-cultural change with a critical eye, this does not justify the dismissing of “strong” programs of coevolution simply because they look toward biology for descriptive and explanatory variables. Toward this end, it is important to first understand the difference between the meaning of “evolution,” as it has been historically used in the social sciences (e.g., Marx, Sumner, Spencer, etc.), and Darwinian evolution (e.g., variation, selection, and retention). In doing this, we find that Social Darwinism is not Darwinism (Levins and Lewontin 1985). The distinction is that the former represents an essentialist framework while the latter represents a non-essentialist framework. Or, if we were to place this discussion in terms of theory construction, the former is “frame-invariant” while the latter is “frame-relative” (McLaughlin 1998, 2001).<sup>16</sup>

Perhaps the most prominent evolutionary approach used today to understand genetic and cultural evolution through their interactions is the “dual inheritance model”—which represents a non-deterministic hybrid between memetics and evolutionary psychology (Boyd and Richerson 1985; Feldman and Cavalli-Sforza 1976; Read 1984; Read and Behrens 1989; Sperber 1996; Sterelny 2001). In the words of Laland and Brown (2002, 242):

*Like memeticists, gene-cultural coevolution enthusiasts treat culture as an evolving pool of ideas, beliefs, values, and knowledge that is learned and socially transmitted between individuals. Like evolutionary psychologists, these researchers believe that the cultural knowledge an individual adopts may sometimes, although certainly not always, depend on his or her genetic constitution (my emphasis).*

The dual-inheritance model is non-reductionistic because it views the forces that unite culture and genes as “pulling” in both directions, both upwards and downwards. In addition, while placing equal analytic emphasis on the causal powers of *both* genes and culture—which arguably further distinguishes this approach from memetics and evolutionary psychology—it also takes an explicitly non-adapta-

tionist stance (Laland and Brown 2002; Laland et al. 1995; Laland et al. 2001; Rogers 1988). Thus, while taking care to examine the processes related to the natural selection of genes, it also remains open to culture-gene interactions that are non-adaptive or even maladaptive to this evolutionary process.

The unwillingness of some to engage intellectually with such approaches to socio-biophysical interaction may in part be due to reasons of bio- and eco-illiteracy (Ellis 1996; Udry 1995). For in understanding how genes and culture interact is to realize that such frameworks do not automatically make humans “prisoners to their genes” (Henslin 1997, 50-51). For example, one of the foundational concepts of culture-gene co-evolution is what is called the “norm of reaction” (Hall 2001; Sarkar 1999). The norm of reaction underlines the premise that environmental factors give shape to the development of a phenotype. Consequently, “a trait that features a narrow norm of reaction is one that produces only a limited range of phenotypes when subjected to a wide range of environmental variation...[while]...a trait with a wide norm of reaction manifests a much greater array of phenotypes in response to variable environmental input[s]” (Machalek and Martin 2004, 461). The norm of reaction thus allows for the possibility of extraordinary variation where no known genetic differences exist—in other words, no genetic straitjacket here.

Another interesting area that seeks to understand human cognition and behavior through a coevolutionary lens is the field of evolutionary epistemology, particularly that as developed by Donald T. Campbell (e.g., 1956, 1959, 1960, 1987, 1997). At the risk of oversimplification, evolutionary epistemologists contend that scientific knowledge develops according to the same non-linear processes as found in the development of the organism: that is, to evoke Campbell’s terminology, knowledge develops by way of “blind-variation-and-selective-retention.” Granted, some evolutionary epistemologists argue that knowledge “grows” rather than “develops” (e.g., Bartley 1987). Ultimately, however, this is a matter of whether one’s position is “frame-variant” or “frame-relative.” Those of the former posit progress as a natural tendency that is independent of the environment, and thus define the “fitness” of knowledge as a matter of verisimilitude. On the other hand, evolutionary epistemologists who take a “frame-relative” position view knowledge “progression” as a product of the continuous interaction between variation and, importantly, context (thus freeing the position from critiques of essentialism).

While space constraints limit the depth at which I can describe these models, the point of this section has been to highlight that non-deterministic coevolutionary models exist. Even so, many within sociology still fail to recognize them as having anything of analytic value to say. Indeed, this point

can be empirically demonstrated by looking at recent introductory sociology textbooks, and the hostile stance often taken within them in regards to positions granting causal efficacy to the biophysical realm (Machalek and Martin 2004). Such disciplinary rigidity could be greatly softened through simply a more complete understanding of what (co)evolution means. Rather than ascribing to essentialism, determinism, and immutability, evolution is non-linear, variable, and directed by forces from both “above” (downward causation) and “below” (upward causation)—all of which are properties that are consistent with critical realism and the concept of an emergent, stratified reality.<sup>17</sup>

To conclude, while I am not suggesting that sociology is necessarily in a “crisis,” as argued by some over the last decade (e.g., Horowitz 1993; Lopreato and Crippen 1999), I would maintain that we are certainly at a crossroads. That is to say, we can either choose to focus energies on explaining socio-cultural phenomena in *all* of their wondrous complexities, or we can continue down our current path by remaining steadfast in our quest for disembodied “social” causal variables. In deciding which road to take at this crossroads we would do well to ask, “What are we ultimately students of?” Reflecting on this question, I am reminded of something Sir Karl Popper (2002 [1963], 88) once eloquently wrote:

*The belief that [disciplines]...are distinguishable by the subject matter which they investigate, appears to me to be a residue from the time when one believed that a theory had to proceed from a definition of its own subject matter. But subject matter, or kinds of things, do not, I hold, constitute a basis for distinguishing disciplines. Disciplines are distinguished partly for historical reasons and reasons of administrative conveniences...and partly because the theories which we construct to solve our problems have a tendency to grow into unified systems... [As such] we are not students of some subject matter but students of problems. And problems may cut right across the borders of any subject matter or discipline (emphasis in original).*

The purpose of this paper has been to stimulate thought, encourage debate, and unlock a few new conceptual doors to our sociological imaginations. By opening our “eye of reason” to the potentiality of *all* causal tendencies, regardless of where such a search may lead, we will become, to evoke Popper’s above-mentioned quote, better students of the problems that interest us. A sociological imagination informed by critical realism gives us a glimpse at such a potential. And it tells us something that we as sociologists have always known: that we will always need the social sciences.

## Endnotes

1. Author to whom correspondence should be directed:  
E-mail: [mcarolan@lamar.colostate.edu](mailto:mcarolan@lamar.colostate.edu)
2. See, for example, Ellis (1996), Burkett (1996, 1997), Buttel (1986, 1996), Catton (1980, 1992, 1994), Catton and Dunlap (1979, 1980), Dickens (2004), Foster (1999), Murphy (1994a, 1994b, 1995, 1997), and O' Connor (1998) for detailed discussions as to sociology's traditional resistance to biophysical causality.
3. While Dawkins does qualify, in places, his use of the term "selfish" to describe the *behavior* of genes, he often gets himself into trouble by making such sweeping statements as the following: "The gene is the basic unit of selfishness (1989 [1976], 26)." Thus, while one can find him discussing (non-deterministic) interactions between genes and human behavior, such statements become lost when he then makes arguments to the opposite effect (see also, endnote 16);  
*Now they swarm in huge colonies, safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous indirect routes, manipulating it by remote control. They are in you and me; they created us, body and mind; and their preservation is the ultimate rationale for our existence. They have come along way, those replicators. Now they go by the name of genes, and we are their survival machines* (Dawkins 1989 [1976], 19-20) (my emphasis).
4. The term "critical realism," however, was coined decades earlier (see, e.g., Drake 1920). Karl Popper (e.g., 1974, 1059) can also be cited as having used the term early on.
5. Critical realists are thereby particularly hostile to strong forms of constructionism and solipsistic postmodernism—what Bhaskar (1997 [1975]) refers to as representing "superidealism"—for they do not allow for such fallibility. Such epistemological theories posit that all knowledge claims are equally privileged social constructs (what C.W. Mills [1959] called "democratic theories of knowledge")—which, upfront, appears to be a rather admirable deontological position. Yet upon closer inspection we find such theories incapable of any broader emancipatory critique. For while they claim to reject epistemological positions of universality, they do so by paradoxically proscribing their own universal—that all knowledge claims are of equal validity. In doing this they ensnare the individual in an impenetrable hermeneutic circle. Space for constructive critique thereby vanishes as all validity claims are assigned equal epistemological footing, be they Einstein's or Hitler's. Such positions, to draw from Karl Popper (2002 [1963]), thus fail to distinguish clearly enough between "questions of origin" and "questions of validity."
6. Specifically, subsumed within "critical realism" are two philosophical projects: that of "transcendental realism" and "critical naturalism." Specifically, the former refers to Bhaskar's (1980, 1997 [1975]) realist philosophy of science while the latter speaks to the methodological argument as to whether or not a genuine "science" of social phenomena is possible (which Bhaskar [1993, 1994, 1998 (1979)] has since extended to support his "Transformational Model of Social Activity.")
7. The word "must" is used to associate it to the broader transcendental question. However, a more accurate phrasing of the question would be, in the words of Lawson (1997, 298), "what is the most explanatorily adequate, albeit fallible, account that we can come up with which renders intelligible the phenomenon of interest."
8. The central mode of inference for critical realism is thus neither deduction or induction but what Lawson (1997, 213) calls "retroduction": "The aim is not to cover a phenomena under a generalization (this metal expands when heated because all metal do) but to identify a factor responsible for it, that helped to produce, or at least facilitated, it...[; t]o posit a mechanism (typically at a different level to the phenomenon being explained) which, if it existed and acted in the postulated manner, could account for the phenomenon singled out for explanation."
9. The concept of "emergence" is hotly contested among philosophers (see, e.g., Sawyer [2001, 2002a, 2002b, 2003a, 2003b] for critiques of the concept and Bickhard and Campbell [2000] for an argument in support of the concept). Allow me to briefly present an argument in support of emergence between biophysical and social strata. According to William Wimsatt (1986, 2000), four properties must be met for phenomena to be reducible to their "lower" substratum, and if they do not a case for emergence can be made. The first property is that parts are intersubstitutable without affect to the whole. In terms of social-biophysical dynamics, we know this not to hold. Biophysical substrata cannot merely be inter-substituted without significant consequence to such "higher" level phenomena as culture, language, and the like (a point currently being learned in the life sciences with the rise of gene therapy [Azzouz et al. 2004; Ehrlich 2000; Fukuyama 2002; Stock 2003]). Second, an aggregated whole should remain qualitatively similar under a part's removal or addition. This property too does not appear to universally hold. We can see this, for example, in the slight genetic variations that exist between us and chimpanzees (humans, for instance, have twenty-three chromosome pairs while chimpanzees have twenty-four; 98.5% of our gene sequences are identical to that of chimpanzees; etc.). Or take what happens when an individual receives an extra copy of our tiniest chromosome, number 21 (with a mere 225 genes): Down Syndrome. Third, the composition function of the whole remains steady under conditions of decomposition and reaggregation. This property can likewise be shown to not hold by reference to the abovementioned examples. Finally, the fourth property that Wimsatt argues must be met for phenomena to be reducible is that the relation between parts and whole is linear. And, as argued throughout this paper, linearity does not adequately capture the multidirectional causal tendencies that exist between biophysical and social strata (much to the chagrin of biological determinists). *Every* effect, therefore, that Wimsatt argues must be met for phenomena to be reducible to their "lower" substratum is absent when assessing whether or not social phenomena constitute emergent properties.
10. Indeed, there remains immense debate as to whether biology plays any role in how we *do* gender: for the naysayers of a socio-biological account of gender, see, for example, Butler (1989, 1994), Epstein (1988), Lorber (1994), Oakley (1997), and Risman (2001); for the other "side" of the debate see, for example, Birke (2000, 2003), Fausto-Sterling (2000, 2003), Hrды (1997), Lopreato and Crippen (1999), Smuts (1995), and Udry (1995, 2000, 2001).
11. Readers will note that Bhaskar (1997 [1975], 47) frequently speaks of "mechanisms" rather than "events": "[Mechanisms] may be said to be real, though it is rarely that they are actually manifest and rarer

still that they are empirically identified by men [sic]. They are the intransitive objects of scientific theory. They are quite independent of men [sic]—as thinkers, causal agents and perceivers....But neither are they Platonic forms. For they can become manifest to men [sic] in experience. Thus we are not imprisoned in caves, either of our own or of nature's making. We are not doomed to ignorance. But neither are we spontaneously free. This is the arduous task of science: the production of the knowledge of those enduring and continually active mechanisms of nature that produce the phenomena of our world."

12. Named after political scientist James Flynn who identified the trend in the 1980s.
13. It is widely regarded that chimpanzees are the reservoir for HIV (Gao et al. 1999).
14. As detailed by Steinmetz and Chae (2002), while there is a sociology of knowledge to be found in Bhaskarian critical realism, it is far less developed than his philosophy of science.
15. It is when we move to the social world, and begin asking questions about what types of things causal mechanisms are, that significant differences between critical realists emerge. This can be seen, for example, in the methodological individualism of Rom Harré (2001) and Charles Varela (1999, 2001, 2002; Varela and Harré 1996) versus the methodological collectivism of Roy Bhaskar (1994, 1998 [1979]) and Margaret Archer (1995). I have intentionally side-step the "micro-macro" (e.g., "breath") argument entirely, focusing instead on the "nature-society" interrelationship (e.g., "depth").
16. I would like to thank one of the reviewers for highlighting this distinction for me.
17. Some may wonder why I have said nothing about E. O Wilson's (1975, 1978, 1998) sociobiology. Like Dawkins (see endnote 2), I too question Wilson's methodological position. In places, Wilson does appear to ascribe to non-determinism in his explanation of human behavior: e.g., "virtually all human behavior is transmitted by culture" (Wilson 1998, 126). Yet other phrases lead me to question his sincerity to this position (others have also noted such discrepancies [e.g., Erhlich 2000; Sussman 2002]): e.g., "[Sociobiology is] the systematic study of the biological basis of all social behavior" (Wilson 1975, 9) (my emphasis); "Sociobiology can account for the very origin of mythology by the principle of natural selection acting on the genetically evolving material structure of the human brain" (Wilson 1978, 192).

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