

Hobbling Science: New Strategies for Reductionists

In my briefs comments, I want to focus on what seems to me to be the most scientifically pernicious idea in their paper, namely, their unmotivated methodological restrictions on causal talk in the social and behavioral sciences. I want to know 1) what the restrictions are really supposed to be and 2) I want to know why we should follow them.

I. What are the restrictions?

By Kostko and Bickle's pernicious principles, I have in mind generalizations that might be drawn from the following two statements. First,

It makes no sense to talk about social rank as a cause of behavior until we can specify the neuronal and molecular mechanisms whereby social rank exerts its influence so as to generate the specific receptor distribution changes and cocaine self-administration behavior. (Kostko and Bickle, p. 9).

Two sentences later Kostko and Bickle conflate this combination "downlink-uplink" principle with a second principle that requires only knowledge of the downlink:

before we can refer to the environment, or any abstraction postulated by social scientists (including social neuroscientists), as a *cause* of measured neurobiological or behavioral effects, we must be able to explain how they are transduced down to neuronal and molecular mechanisms (Kostko and Bickle, pp. 9-10).

So, what I have in mind are generalizations such as,

It makes no sense to talk about social causes and effects until we can specify the neuronal mechanisms underlying these causes and effects,

and

We cannot talk about environmental causes of neurobiological or behavioral effects until we explain how environmental causes are transduced down into neuronal and molecular mechanisms.

Note that these are methodological principles about what makes sense or what one can talk about; they are not metaphysical principles about the nature of causes.

I can think of three reasons not to take these principles literally. One would have to do a lot of song and dance to explain what Kostko and Bickle “really” mean. First of all, the very scientists that Kostko and Bickle refer to seem to violate these principles construed literally.

Morgan, et al., claim that

Whereas the present study represents the first examination of intravenous drug self-administration in socially housed monkeys, there have been several studies examining other behavioral *effects* of psychomotor stimulants in similarly housed monkeys. These experiments have demonstrated differential *effects* of cocaine and d-amphetamine as a function of social rank. Several rodent studies have examined the *influence* of housing conditions (individual versus) social on cocaine and amphetamine self-administration ... The present study provides experimental evidence that individual differences in susceptibility to cocaine abuse within a population may be *mediated* by social dominance rank.” (Morgan, et al, p. ?, emphasis added).

Is this really nonsense they are spouting?

Second, and more embarrassing for Kostko and Bickle should be the fact that they can barely avoid flouting these principles themselves. Kostko and Bickle observe that

Previous rodent studies have shown that disruptions to dopaminergic systems alter responses to reward and the reinforcing effects of cocaine. Nader’s lab extends this research to nonhuman primates, measuring the impact of individual versus social housing on number and availability of dopamine D₂ receptors and cocaine self-administration (p. 5.)

They also claim that “Nadel’s work clearly demonstrates a role for environmental factors for understanding the causal mechanisms of social behaviors,” (p. 7) and that “Dominant status confers the benefits of increased access to environmental features such as food, mates, and territory. Subordinate status decreases access to all these factors and adds the burden of social isolation” (p. 11).

The third reason we can’t take these restrictions literally is that Kostko and Bickle seem to wish to claim that causal claims in social science have some heuristic value:

From the reductionistic neuroscientist’s perspective, the role that remains for these

abstractions, and for the social sciences in general, is purely *heuristic* (p. 11).

How much heuristic value can there be to what is literally nonsense? How much heuristic value can there be to referring to things one is not supposed to refer to?

So, given that we can't take the obvious generalizations literally, how are we supposed to take them? What do Kostko and Bickle really mean here?

2. Why should causal claims in social science wait for neuroscience?

No less difficult a question is why we should adhere to such principles. Why should social science in any sense wait on the emergence of a discipline that, by Kostko and Bickle's own account, is now largely non-existent? What methodological advantages could there be to this? Kostko and Bickle hint at three possible reasons, none of which seem to me to be compelling.

First, there is some sort of "authority of science" argument. At one point, Kostko and Bickle assert, "That [principle is] not a metaphysical directive; that's science talking" (p. 10). It's not 100% clear that they wish to invoke "science talking" to justify their claim, but if so, this is simplistic. A more nuanced account is in order. It is not as though scientists have univocally endorsed the kinds of reductionist methodological principles Kostko and Bickle propose. For example, there are qualitative sociologists who really couldn't give a damn about the biological mechanisms underlying sociological regularities. More embarrassing to Kostko and Bickle, however, should be the fact that the scientists they cite don't advocate these sorts of methodological restrictions. Morgan, et al, do not stigmatize causal claims in social science. They seem to accept them at face value, then try to discover the underlying biological regularities. They don't denigrate them, then try to discover the underlying biological regularities.

Well, suppose we can set aside what those who call themselves scientists say. Suppose, simply, that science does assert these principles. Suppose there were group think on an unprecedented scale. Suppose that everyone and every thing associated with science endorsed these principles. Still, we would want to know what warrants these principles. Still we would want to know why we should refrain from asserting causal connections in social science until we know the underlying neuroscience.

A second reason one might think we should adopt the methodological strictures Kostko and Bickle propose is based on the desire to explain putative causal laws in social science in terms of underlying biochemical mechanisms. Ok. Suppose we do. This is clearly a standard sort of scientific goal. Nevertheless, why does this require saying that putative causal connections in the social sciences are not really causal, that they are merely heuristic? Suppose we want to explain Newton's law of universal gravitation. Why do we have to begin by saying that Newton's law has merely heuristic value, that it is part of some second rate science, or that it is not really a law? Suppose we want to explain why gases obey the law $PV = nRT$ by appeal to the kinetic theory of gases. Why should we not assert that $PV = nRT$ prior to the development of the kinetic theory of gases? Why should we begin by saying that the branch of chemistry of which the ideal gas law is a part is a junior partner in science and that the so-called "ideal gas law" is not really a law? What reason is there to say that $PV = nRT$ has mere heuristic value? It would seem that the theoretical relations between the higher-level science and the lower level science are completely unchanged by saying that the higher-level science is second rate or merely heuristic.

A third possible reason to think we should adopt the methodological strictures Kostko and Bickle propose appeals to the idea that neuroscientists sometime "intervene

cellularly/molecularly and track behaviorally”. Recall what Kostko and Bickle have to say about this:

it should be possible to generate specific social behaviors by directly manipulating their hypothesized neuronal or molecular mechanisms. This will require a better understanding of the neuronal and molecular mechanisms involved, but the strategy seems clear enough. In light of Nader’s work, for example, once we get a clearer picture of the neuronal and molecular mechanisms whereby social rank is transduced into neural currency, we could then strive to manipulate these mechanisms directly, independent of actual social circumstances, to produce the specific social behaviors: to induce, e.g., vulnerability to drug abuse or future position within a dominance hierarchy (Kostko and Bickle, p. 13.)

The idea is that social science is second rate because we can perform neuroscientific manipulations to get social results without manipulating social conditions. Yet, this argument seems to backfire on the reductionist’s dreams of relegating social science to second class status.¹ If you relegate a level to second class status by circumventing it, then much of social science has already relegated neuroscience to second class status. Suppose you want to know how, say, class size casually influences academic achievement. Surely you can manipulate class size directly, without having to interfere at the neuroscientific level. Suppose you want to understand how housing conditions influence academic achievement. Surely you can manipulate housing conditions directly without worrying about the neuroscience. So, if this is the rationale for making social science a junior partner, it seems to me to backfire.

To conclude, it seems to me that Kostko and Bickle need to do some work to articulate a plausible version of their methodological restrictions on the social sciences, then do some additional work to justify adhering to them.

¹ The work by Morgan, et al., seems to be a bad example for Kostko and Bickle. Those experiments involved manipulating socially and tracking cellularly/molecularly. Change housing conditions and see what happens to dopamine receptors.