

Why is Epistemology Still Relevant to the Sociology of Science? Comments on Kale-Lostuvali's "Two Sociologies of Science in Search of Truth"

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Elif Kale-Lostuvali's paper, "Two Sociologies of Science in Search of Truth: Bourdieu Versus Latour" reads like a chapter in the sociology of science textbook for graduate students. Although she summarizes and contrasts—well, not always in a satisfactory manner—Bourdieu's and Latour's view on the relationship among scientific objectivity, autonomy of the scientific field and scientific truth, she fails to provide us with a persuasive critique of Bourdieu and Latour, to say nothing of a promising alternative to their views.

Let me begin with the problem of epistemology which, as Kale-Lostuvali argues, Latour abandons as sterile. For Latour, statements become true or false not because they correspond, or don't correspond, to the way the world is, but because positive or negative "grammatical modalities" are added to or subtracted from them during the journey in which the fates of those statements are determined by the various 'actants'. Behind such semiotic understanding of the production of facts and artifacts lies what philosophers call the Quine-Duhem-Lakatos problem of "underdetermination of theory by data". Latour, however, went one step further and argued that, just as scientific theories are constructed by the scientists, society, being constantly interpreted and constructed, should not be given a privileged status in explaining technoscience. This is what Latour calls the "extended symmetry" as opposed to the symmetry thesis of the strong programme sociologists.

Since the boundary between nature and society is always in the making and, in that sense, is constantly being redrawn, Latour argues, neither nature nor society should be used to explain the shape of the association. Thus, for Latour, the premature introduction of nature or society will only mangle the true picture of how nature and society are codetermined. We cannot know what the nature and society are like until the association stabilizes itself. For Latour, therefore, epistemology should be replaced with ontology, a study of how humans and nonhumans are "hung together" rather than why they are hung together as they are. The "facticity" of facts is simply a matter of the "length" of the association rather than their fits with reality. As Kale-Lostuvali wrote, "what matters is how these actants associate" (11), not *why* the association takes the form as it does.

Explanation and Description

Latour's proposal for abandoning explanation in favor of the "description" of the trajectory of the association of human and nonhuman things, however, poses a serious problem that he himself cannot resolve because he fails to demonstrate the difference between description and explanation. Rejecting the need for an explanatory theory accompanied with a dualism of various sorts, e.g., subject/object, word/world, theory/practice, society/nature, Latour argues that the study of the association should be conducted after the manner of an ethnomethodologist, describing how the association expands or contracts without introducing a theory from without. However, in view of the fact that ethnomethodological descriptions of practical actions including scientist's

practice have been heavily criticized just for the simple reason that no pure description of whatever practice can be obtained without sneaking in certain theoretical ideas through the backdoor (Kim 1999; Habermas 1984; Pollner 1991), Latour's attempt to defend his descriptivism has only a bleak future.

Let me make this criticism more perspicuous by showing why Latour's supposedly 'theory-free' descriptivism cannot do what it professes to do. Here's Latour's own remark that vividly shows that he reverts back to the dualism he so vehemently rejected.

Understanding what facts ... are is the same task as understanding who the people are. If you *describe* the *controlling elements* that have been gathered together you will understand the groups which are controlled [my italics] (1987, 140).

Here, Latour deliberately avoids using the term, "explanation" and instead uses the word, "understanding" and argues that understanding the production of facts and truth is equivalent to describing the "controlling elements" and "the controlled groups." But, to describe the relationship between the things that are controlled and the things that control, Latour should first *distinguish* and *separate* the controlling elements from the controlled elements. To put it in another words, Latour cannot even identify what the controlling elements are without presupposing a "causal relationship" between those elements and a certain kind of effect that those elements are supposed to bring about. This shows that Latour's allegedly descriptive account of how facts are produced through the association turns out to involve an explanatory theory. This is surely a return to the dualism that he so abhors.

Let me quickly relate this criticism to Latour's "translation of interest" argument. Although Latour argues that an interest is always in flux and as such disqualifies as the fixed explaining factor, the translation of interest argument does not make sense unless we suppose a deeper level interest that explains why the actors *want* to translate' the interests of others into their own. If Latour retorts that such deeper interests are also merely in the middle of translation, he is required to answer why those interests are being translated. This only leads him to an infinite regress! The concept of interest is still fixed and works as a "causal factor" in Latour's rendering of technoscience no matter how many times interests are translated! Latour could not successfully defend his arguments for descriptivism and the extended symmetry unless he offers convincing answers to such criticisms. But Kale-Lostuvali is silent on how Latour can handle such criticisms and simply asserts that Latour is more successful in rendering intelligible the production of scientific truth than Bourdieu (17).

Regarding Bourdieu and Popper

Now it is time to turn to Bourdieu. Kale-Lostuvali is right when she complains that Bourdieu has not provided us with any empirical evidence to support his argument for the rational dialectic within the scientific field. But she is certainly wrong when she argues that Bourdieu merely reiterates the circular relationship between the autonomy of the scientific field and the production of the transhistorical truth without telling us how to

“verify” that the “ensuing product” of the scientific field is transhistorical (17). And, referring to my defense of Bourdieu (Kim 2009), she argues that my historical-sociological study of the conversions of biologists to the Mendelian theory in the early 20th century “cannot resolve the circularity of Bourdieu’s theory of truth” (21).

As I show below, however, this argument against Bourdieu and myself stems from her hopeless misunderstanding of the elementary philosophy of science and, more specifically, the nature of the transhistorical truth produced within the field. To show why Kale-Lostuvali’s criticism of Bourdieu cannot withstand scrutiny, a brief digression to Popper’s epistemology is necessary because Popper’s philosophy of science gives us an important clue to the understanding of Bourdieu’s sociological theory of truth production.

It is ironic to note here that Popper, in his *Logic of Scientific Discovery* (1959), already pointed out the “irreducibly circular” nature of scientific truth, for which Latour only recently claims originality. Contrary to the misunderstanding of many people including the author of “Two Sociologies of Science in Search of Truth”, Popper’s falsificationist methodology which seems at first glance diametrically opposed to what Latour says, is actually no different from Latour’s argument as far as the *status of facts* is concerned.

Popper argues that there is no way to escape from the ‘irreducible circle’ in which we move in acquiring scientific knowledge, for all statements including “basic statements” which are used to test a theory are themselves theory-laden and need further tests. But, if so, how could Popper suggest falsifiability as the “demarcation” criterion that can be used to distinguish science from pseudo-science? For Popper, basic statements are not basic in the sense that they don’t need further empirical tests. Rather, they are basic because the scientific community puts “collective trust” in those statements and use them to test theoretical statements that are “less well” trusted. For Popper, such collective trust on the “background” knowledge was made available through the scientists’ criticism of one another’s arguments and demonstrations, not just through the rhetorical maneuvering as Latour argues.

Although, like Popper, Bourdieu believes that there is no apodictic truth untouched by human hands, he argues that scientists can actually break the circularity involved in the production of truth through the applications of the specific rules embodied in the autonomous scientific field. Bourdieu argues that scientific statements rely on the “socially constituted conditions and a prioris” in the Durkheimian sense (Bourdieu 2004, 78) which, in contrast to Kant’s universal conditions and *a prioris*, are embodied in the “conventional codes, socially grounded presuppositions, [and] historically constituted classificatory schemas” (1996:335). According to Bourdieu, these socially constituted conditions and *a prioris*—the *habitus* of the scientists—can help scientists break the circularity involved in the truth production because it makes theory testing possible by providing the backdrop against which a less well trusted claim to truth can be tested.

Arbitrating the Real

For Bourdieu, unlike Latour, the ‘socially constituted conditions and a prioris’ embodied in the *habitus* of the scientists are not simply obtained in a “referential vacuum” through

a series of semiotic operations on the statements. Rather, they are the result of the “arbitration of the real” which in turn is the product of the social process in which scientists with different theoretical orientations stay together in a focused controversy, mutually monitoring and cross-validating each other’s arguments and demonstrations without being interfered by the non-experts outside of the field. It is precisely in this sense that Bourdieu argues that scientific knowledge can transcend the historical conditions, i.e., socially constituted conditions and *a priori*s, of which it is a product. The historical conditions under which scientific truth is produced work, on the one hand, as the backdrop against which a specific claim to knowledge can be tested, but they are on the other hand continuously transcended and transformed through the attempts to criticize and revise the very basis upon which such criticism is initially based. This is precisely what Bourdieu means by the ‘transhistorical’ truth produced within the field.

Failing to understand what the transhistorical truth means for Bourdieu, Kale-Lostuvali keeps coming back to the mistaken argument that Bourdieu’s Kantian rationalism “does not invite empirical investigations” (17). In arguing so, she ignores the existing literature in the sociology and history of science that amply attest to the power of Bourdieuan sociology of scientific truth. For example, in my (Kim 1994) and Rudwick’s (1985) narrative account of the evolution of the early 20th century genetics field and the late 19th century geology field respectively, competing scientific theories were shown to be constructed against the backdrop of the “socially constituted conditions and a priori” of each field. Those theories were not, in that sense, the results of the direct perception of the nature. However, these two narrative accounts of the evolving scientific fields show quite convincingly that the decisions as to which of the competing social constructions is the most plausible representation of the natural world were made possible through what Bourdieu calls the “rational dialectic” of the field, i.e., through the process of mutual criticisms and persuasion which is regulated by the specific rules of the scientific field rather than through the purely political and verbal maneuvering as Latour argues.

What is most interesting about these two studies is that such rational dialectic within the autonomous fields made many biologists and geologists concede their errors and accept the interpretation they once fiercely opposed. For example, William Castle, a Harvard geneticist, candidly admitted in 1919 that, without acknowledging the validity of his opponents’ and even his own students’ criticism, he could not indeed resolve the “anomalies” that made his cherished theory incongruent with the data obtained by himself. Castle’s decision to abandon his cherished theory in light of the “recalcitrant” experimental results, however, cannot be made comprehensible within Latour’s account of truth that denies the logic of practice specific to the scientific field.

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