

## Should We Still Ask the Question That Scientific Realism Would Answer?

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Gary Gutting's paper brings to the tangled debates over scientific realism the combination of clarity, insight, and good sense that his readers have learned to expect from him. I not only admire his paper, I substantially agree with his main lines of argument. I will sketch these points of agreement quickly, then devote most of this response to teasing out the more subtle places where I think we may disagree.

Among positions within the realism debates, such as Bas van Fraassen's constructive empiricism, Richard Rorty's romantic vision of scientists as just another kind of strong poet, Richard Boyd's metaphysical realism, Harry Collins' social constructivism, or Steve Woolgar's reflexive skepticism, Gutting's "non-metaphysical" realism is surely the best position to take. Gutting's specific criticisms of Rorty and van Fraassen are well taken. Gutting and I also stand together in taking Ernan McMullin's "Master Argument" as a decisive objection to the retention of any epistemically weighty distinction between observable and unobservable entities. I find especially attractive Gutting's suggestion that empiricist scruples against inference to the existence of unobservables are best read as a surrogate for scruples against inferences from the familiar to the "weird." This suggestion makes it more intelligible to me why some otherwise very able philosophers still find empiricist antirealism compelling.

Readers may quite reasonably wonder at this point where Gutting and I might conceivably disagree. Here is my central concern: Gutting offers an especially sensible answer to the questions posed by realists and antirealists, whereas I want to raise doubts about the assumptions that make those questions intelligible. Part of what makes his answer better, in my view, is his attempt to distance himself from the representationalism shared by metaphysical realists, empiricists and social constructivists. I fear, however, that he has not fully moved out of their philosophical neighborhood. I shall spell out this concern in three ways.

My first point concerns the significance of predictive success in the sciences. Gutting discusses "prediction and control" in the most general and abstract way (as does Rorty, to whom Gutting is responding). Prediction and control are not a massive, univocal achievement, however, but a complex, multi-faceted, subtle, localized, and sometimes jury-rigged network of capacities. Only a more finely-grained description of those capacities can do them justice. Recent work on experiment and instrumentation often usefully emphasizes their relative autonomy from theoretical determination. These insights should still not obscure that we generally value not prediction and control per se, but a discursively articulated prediction and control which thereby yields understanding. Philosophers too often amalgamate such articulation under the catch-all heading of "explanation." Here too, I think we need more fine-grained accounts of the kind of conceptual grasp that the best scientific work provides, and of how and why such understanding should matter to us.

The point of such fine-grained articulation of scientific capacities is not skeptical. Just the opposite! Arguments for skepticism or relativism depend upon the possibility of a wholesale assessment of scientific capacities. The wholesale vindication of science that realists often hunger for is thus not objectionable merely for its superfluosity. Accepting the supposed need for wholesale justification concedes too much to skepticism. Scientific practices are in "the truth." The interesting question is not whether, when standing back from scientific practices, we can justify that these practices disclose reality; rather, we should ask, in particular contexts, what these practices disclose about the world, i.e., their scope, meaning, and significance. These are

not disengaged questions about the possibility of knowledge in general, but situated interpretations of how we ought to understand specific aspects of the scientifically accultured world we live in.

My second concern addresses not what science can tell us about the world, but what our theories of scientific knowledge tell us about ourselves as knowers. I emphatically do not claim that science cannot reach "outside" our webs of belief and desire to disclose things in the world. The mistake is instead to conceive of knowing as enclosed "inside" such a web in the first place. Here I find a residual tension between Gutting's rejection of representationalism, and his continued espousal of realism, which still connotes a correspondence between "internal" belief or meaning and "external" object. Here also we still have something to learn from Heidegger's reconception of intentionality as being-in-the-world, which denies any semantic, psychological or social intermediaries between us and the world. For Heidegger, the issue between realists and "idealists" never gets off the ground.

This second concern arises specifically in Gutting's paper when he insists upon distinguishing the natural sciences as the locus of his methodological realism. My point is not that we should, after all, equate the "cognitive successes" of sociologists and economists with those of physicists and biologists; rather, I think we may have misunderstood the successes of the latter. The standard story differentiating the natural from the human sciences is familiar: the natural world seems causally tidier than the social world, which is fraught with meaning, and includes objects of inquiry who talk back. The unspoken presumption, however, is that what the sciences disclose is the "natural world." Recent science studies indicate what successful scientists already knew, that the focus of natural scientific inquiry is a work-world, whose material and sociocultural articulation is thoroughly intertwined. That is partly because successful research requires an appropriately organized community, as Peter Galison's studies of the shifting social and material scale of 20th Century physics show especially clearly.<sup>1</sup> But it is also because the point of specific inquiries depends upon their culturally situated significance. Scientific inquiry can never be satisfied merely with truth; what matters in science is interesting truths, elegant theories, powerful insights, productive experimental systems, robust models, and important achievements.<sup>2</sup>

Recognizing that natural scientific work is fraught with meaning does not take us back toward social constructivism, however. It would do so only if human communities (or agents) were fully authoritative over what is at issue and at stake in what they do. What is interesting, elegant, powerful, productive, robust, or important is not simply given by nature, but neither is it entirely up to "us." The presumption that meaning is fully determined by human beings (whether individually or collectively) often plays a prominent role within the human sciences. But my suspicion, although I cannot even begin to defend the point here, is that some of the predictive and interpretive difficulties of the human sciences stem from this: there is no "social world," at least not in the sense of a relatively autonomous field of meaning and action. Social practices, meanings, and stakes are materially situated in ways that need greater recognition within the human sciences.

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<sup>1</sup> Peter Galison, How Experiments End, Chicago: University of Chicago Press, 1987; Image and Logic: A Material Culture of Microphysics, Chicago: University of Chicago Press, 1997.

<sup>2</sup> For an illuminating discussion of this point, see Catherine Z. Elgin, "Understanding in Art and Science," Midwest Studies in Philosophy 16 (1991), p. 196-208.

My final concern starts from Gutting's insight that empiricist scruples may reflect an understandable caution about accepting "weird" theoretical entities or properties. This insight sorely needs historical contextualization. After all, the boundary between the familiar world of everyday experience and the "weird" has not always been located in quantum physics and general relativity. The unimaginably strange and wonderful has at various times been found in the superlunary sphere, the vitality of living things, magnetism, atoms, or "microscopic" organisms.<sup>3</sup> The partial assimilation of these phenomena has materially and conceptually transformed "the world we experientially live in." In retrospect, however, their apparent "weirdness" has turned out to have been all too often an artifact of the conceptual configuration within which these phenomena were initially encountered, an artifact which subsequent conceptual transformation partly dissolves.

A reader of contemporary anglophone philosophy might well add "normativity" to the list of suspiciously weird properties sometimes posited to make sense of familiar features of our experience. In a world of causal powers and reliable mechanisms, the intentionality and accountability of thought, meaning and action have come to seem increasingly weird. In such a world, how could one thing come to be "about" or responsible to another? I mention this seemingly tangential point, because I think it is not after all so far removed from the "weirdness" of quantum mechanical phenomena. In suggesting such a connection, I am not thinking of those interpretations of quantum mechanics that would explain the strange by the mysterious, through an appeal to consciousness or the like. Rather, I am noticing that normativity and quantum physics come to seem "weird" against the same background conceptualization of causal powers and real properties. My underlying suspicion, prompted in part by Karen Barad's reflections on Niels Bohr, is that more adequate conceptualization of the quantum world and of agency and material practice will dissolve some of the "weirdness" now attributed to both normativity and microphysics.<sup>4</sup> I cannot explore that suspicion in this paper, but I can suggest a relevant addendum to Gutting's discussion: inferences from the familiar to the apparently "weird" should sometimes prompt conceptual reform instead of just theoretical caution.

I conclude with a brief summary of the common import of my three concerns. Gary Gutting is right to reject skeptical challenges to the "reality" of unobservable entities, to the importance of scientific prediction and control, and to the existence of worldly constraints upon

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<sup>3</sup> Indeed, as Lorraine Daston, "The Nature of Nature in Early Modern Europe," Configurations 6 (1998):149-72, has compellingly shown, even the classification of the "weird" has changed within the modern scientific tradition, as the concept of the "preternatural" has disappeared, and those of the "supernatural," "unnatural," and "artificial" have changed their meanings and referents.

<sup>4</sup> Karen Barad, "Meeting the Universe Halfway: Realism and Social Constructivism Without Contradiction," in L. H. Nelson and J. Nelson, ed., Feminism, Science, and the Philosophy of Science, Dordrecht: Reidel, 1996, p. 161-94.

the sociological or "strong poetic" imagination. His carefully circumscribed realism is a valuable antidote to these philosophical maladies, and admirably serves this role without carrying much tendentious philosophical baggage. In the end, however, I suspect that the best response to these maladies would not be to seek a better version of scientific realism, but to attempt a more richly detailed philosophical engagement with scientific practices. My hope is that we would thereby get beyond the debates over realism, and toward a constructive conceptual reconfiguration of meaning, agency, causality, and the world.

## NOTES