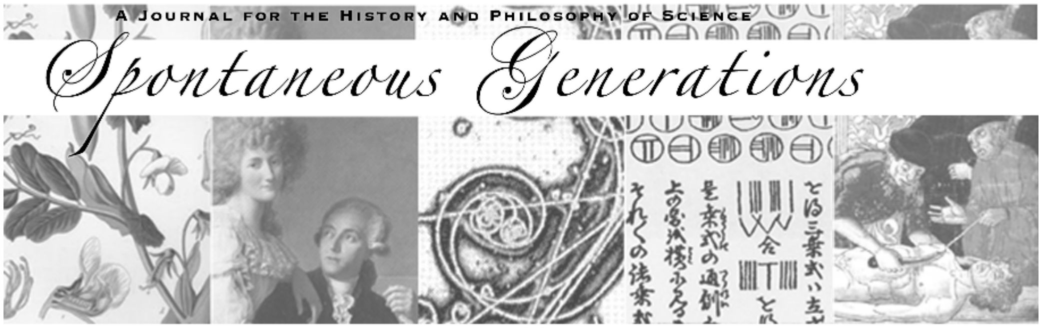

The Future of the Scientific Realism Debate: Contemporary Issues Concerning Scientific Realism

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Curtis Forbes*

I. Introduction

"Philosophy," Plato's Socrates said, "begins in wonder" (*Theaetetus*, 155d). Two and a half millennia later, Alfred North Whitehead saw fit to add: "And, at the end, when philosophical thought has done its best, the wonder remains" (1938, 168). Nevertheless, we tend to no longer wonder about many questions that would have stumped (if not vexed) the ancients:

> "Why does water expand when it freezes?"
>
> "How can one substance change into another?"
>
> "What allows the sun to continue to shine so brightly, day after day, while all other sources of light and warmth exhaust their fuel sources at a rate in proportion to their brilliance?"

Whitehead's addendum to Plato was not wrong, however, in the sense that we derive our answers to such questions from the theories, models, and methods of modern science, not the systems, speculations, and arguments of modern philosophy. Like philosophy, science often begins in wonder; but when scientific inquiry has done its best, it would seem, the wonder does not always remain.

Ex cathedra, the scientific image of reality found in our current best scientific theories, inculcated in our science classrooms, and artfully presented through more popular media proffers far more answers than, say, Aristotle's treatises on natural philosophy ever could; these answers, furthermore,

* Curtis Forbes recently received his PhD from the University of Toronto's Institute for the History and Philosophy of Science and Technology. His on-going research project aims to make the scientific realism question more personal than is usual—rather than trying to determine which philosophical interpretation of science is best *per se*, his research asks how we might determine which interpretation is best for specific people. Not coincidentally, he spends much of his free time wondering whether, why, and how much he himself should believe about the modern scientific worldview.

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seem vastly more precise, accurate, deep, consilient, elegant, empirically sound, and widely accepted than any given by the modern scientific worldview's alternatives and predecessors. Making sense of reality through the lenses of our current best scientific theories can leave a curious person feeling immensely proud of our collective epistemic achievements, incredibly intellectually satisfied, and utterly convinced that the picture of reality painted by these theories is true (at least approximately speaking).

But philosophers can't help wondering: is that picture true, even as an approximation? Is our world really the way modern physical theory describes it, abuzz with various unobservable (though perhaps detectable) entities whose causal interactions make everything appear to us as it does? Is it the aim of empirical science to discover some hidden reality behind our everyday experience, and thereby satisfy our wonder? Is that even possible?

These are distinctly philosophical questions, for although they are questions about the epistemic outcomes, long-term potential, and ultimate aims of scientific inquiry, scientific inquiry itself is unable to answer them satisfactorily. The theories of modern science, taken as true, provide extremely satisfying explanations when we wonder why ice has more volume than water, why iron turns to rust, or why the sun keeps shining day after day. But when we wonder whether these theories are true, whether the methods of empirical science could ever help us arrive at true theories in the first place, or whether scientific inquiry even aims to arrive at such theories, we have reached the limits of science and entered the domain of philosophy.

"Scientific realism" refers to any philosophical thesis that constitutes an affirmative answer to one or more of the questions above, e.g., by asserting that the theories of modern science paint an approximately true picture of unobservable reality, that scientific inquiry is capable of arriving at approximately true theories (even if it hasn't yet), and/or that the governing aim of scientific inquiry is to arrive at such theories (even if it never will). Whether they understand it as a metaphysical, epistemological, or axiological thesis, casual observation suggests that the average person finds some form of scientific realism to be intuitively correct; formal surveys establish that most academic philosophers do as well (Bourget and Chalmers 2014, 481).

Nevertheless, realist interpretations of science certainly have their detractors, and the ongoing debate between realists and anti-realists has been a central part of philosophical thinking about science for at least the last four decades. Thinking earnestly about the merits of scientific realism now requires navigating contentious historiographical issues, being familiar with the technical details of various scientific theories, and addressing disparate philosophical problems spanning aesthetics, metaphysics, epistemology, and beyond. For all these reasons, reviewing the extant literature on the topic can be both fascinating and frustrating. With that in mind, this special issue of *Spontaneous Generations: A Journal for the History and Philosophy of Science* collects over twenty invited and peer-reviewed papers under the title "The Future of the Scientific Realism Debate: Contemporary Issues Concerning Scientific Realism." It was organized in the hope of providing a suitable forum for everyone to pause, catch their breath, and share their current thoughts on this topic. It should present a suitable place for any newly interested party to enter the debate and begin to make sense of the existing literature. Deeply immersed veterans whose thinking has shaped the course of the debate in recent years should find this collection valuable as well, including those who have contributed to it.

Ultimately the hope is that this collection will help all curious parties better understand, appreciate, and reflect on the evolution of the scientific realism debate, its current state, and how we might move it forward fruitfully. I will count this collection as a success, however, so long as a few others find it as enjoyable to read as I have, even if, at the end, the shared wonder that drives the scientific realism debate remains.

II. Articles

For such a thoroughly discussed topic, it might seem strange that ANJAN CHAKRAVARTTY and BAS VAN FRAASSEN—the former a champion of scientific realism and the latter an opponent—have contributed a dialogue entitled "What is Scientific Realism?" investigating that very question. Scientific realism now comes in a variety of subtypes—semirealism, structural realism, entity realism, etc.—as its defenders have tweaked, qualified, and clarified the basic position to fend off various anti-realist challenges. But as Chakravartty and van Fraassen's conversation makes clear, more abstract differences between people's conceptions of scientific realism have not always been sufficiently appreciated. Thankfully, this dialogue clarifies the differences between each author's conception of scientific realism, revealing some important common ground and better mapping the contested territory. Because it addresses such a basic question, and in doing so reveals the complexity of the dispute, Chakravartty and van Fraassen's piece provides an ideal place to start for anyone reading through this entire collection.

JEFFREY FOSS addresses the debate over scientific realism with a refreshing combination of candour and irreverence. Foss reminds us that our realist commitments to science should be retail rather than wholesale, so "philosophers really should respond to the question, 'Are you a scientific realist' with the question, 'Which bit of science do you have in mind?"' (26). At the same time, people's explicit pronouncements might not be the best way to determine their true ontological commitments, for their behaviour often implicitly presumes (or denies) the (likely) truth of various scientific theories. As Foss sees it, regardless of whether we assent to or dissent from a realist interpretation of science in the classroom, at conferences, or in our publications, each of us tends to treat as real whichever entities "*really* matter" (28) to us, pragmatically speaking.

Such a *pragmatic* focus departs from the usual terms of the scientific realism debate, which generally focuses on *epistemic* questions, e.g., do we know that the theories of modern science are (approximately) true, as scientific realists assert we do? Several of the papers in this issue begin by assuming the debate over such epistemic questions inevitably ends in a stalemate, a view Bradley Monton accurately described as a "growing consensus" (2007, 3) a full decade ago. Even so, there may still be pragmatic debates worth having about the relative utility of interpreting science realistically or anti-realistically, e.g., is it useful to believe that the theories of modern science are true? HASOK CHANG, for instance, thinks we should "face the fact that we cannot know whether we have got the objective Truth about the World (even if such a formulation is meaningful). Realists go astray by persisting in trying to find a way around this fact, as do anti-realists in engaging with that obsession" (31). Rather than treating scientific realism as a descriptive thesis, Chang looks to develop it as a kind of policy, a commitment to think and act in certain ways, a position whose virtues can be measured according to whether adopting it proves "useful for scientists and others who are actually engaged in empirical inquiries" (31). THEODORE ARABATZIS also suggests that realism might be best evaluated pragmatically rather than epistemically, specifically by assessing its potential as a historiographical tool for making sense of scientific practice. While Arabatzis suggests that historians of science might find adopting a realist outlook methodologically useful in limited circumstances (cf. 2001, 2006), HARRY COLLINS maintains his longstanding position that sociologists of science must always maintain a "methodological relativism" (39) when studying scientific practice (though he's come around to the idea that those same sociologists might find themselves "wallowing in realism" in other contexts, without contradiction). We can debate whether adopting an anti-realist attitude is a methodological necessity for the sociologist of science, of course, which raises an interesting point: even if we assume that the debate over scientific realism's epistemic merits inevitably ends in a stalemate, we might still fruitfully debate its pragmatic merits, investigating when (and whether) adopting it proves useful for scientists, historians, sociologists, or anyone else; for the truth of scientific realism, and the utility of treating it as true, are surely distinct issues.

ARTHUR FINE agrees that it's futile to continue arguing about whether scientific realism is true, but he also believes it's futile to investigate the utility of adopting a realist outlook, arguing that we're unlikely to find people's philosophical outlooks on science making any practical difference.

He argues that we should expect scientists, for example, to operate equally well *as scientists* whether they're realists searching for true theories or anti-realists searching for something else, e.g., theories that are reliable tools for making predictions. Nevertheless, as several authors have pointed out over the years this is an *empirical* matter, so conducting a thorough historical investigation would be the only way to determine whether realists and anti-realists typically practice science in distinctive and differentially fruitful ways (Hendry 1995, 2001; Forbes 2016; cf. Wray 2015; McArthur 2006). Depending on the results of such an investigation, working scientists might find that the history of science gives them a pragmatic reason to prefer one philosophical interpretation of science over all others. But, Fine argues, we shouldn't expect the history of science to reveal any reason for anyone to prefer one interpretation over any other, and accordingly he offers a prescription for which he's already well-known: stop trying to find any reason—pragmatic, epistemic, or otherwise—to be a realist or an anti-realist about science. JOSEPH ROUSE likewise argues that the scientific realism debate is a fruitless endeavour, though his reasoning is different than Fine's. As Rouse sees it, the various conflicts between realists and anti-realists only arise and seem pressing because both parties share specious philosophical assumptions. He identifies and challenges several such assumptions, arguing that once we give them up we'll have no need to keep asking the kinds of questions to which realism or anti-realism would be an answer; if we think about it properly, he suggests, our desire to continue the scientific realism debate will simply vanish.

ALAN MUSGRAVE also has little interest in continuing to debate whether one should be a scientific realist, but not because the debate is futile or confused; rather, as he sees it, the debate is already settled, for realism about science is the "obvious, commonsensical view" (52). Thus, his concern is not with defending realism about science against anti-realist challenges, but rather with figuring out how to avoid slipping into "mad-dog dogmatic realism" while maintaining a more modest "lap-dog critical realism." His considerations take him through many episodes from the history of science and several classic issues in epistemology and the philosophy of science: the problem of induction, the nature and limits of scientific explanation, the possibility of having justified beliefs in the face of uncertainty, the distinction between observables and unobservables, and so on. His guiding thought throughout is this: if science is to be believed, as the realist maintains it is, what does that entail about the way things are? But HOWARD SANKEY points out that, *prima facie*, there is an epistemological problem with the claim that scientific realism is just common sense, for a realist interpretation of science actually *contradicts* our commonsense picture of the world. The issue is that the empirical evidence we have for the scientific picture of reality is based on observations made through the lens of our commonsense understanding of the world, so when our scientific worldview undercuts that commonsense worldview, it seems to discredit the evidential basis upon which it was initially accepted. Sankey ultimately believes that this apparent conflict between common sense and scientific realism is an illusion, but he reminds us that any would-be scientific realist must be able to explain how the scientific method can reveal the truth about reality by first looking through a lens that makes reality appear very different than it truly is.

Many would-be scientific realists have been especially preoccupied with evading an anti-realist challenge commonly known as the "pessimistic meta-induction," most memorably pressed by Larry Laudan (1981). The idea, roughly, is that history shows that taking a realist attitude toward any contemporary scientific consensus is an unreliable belief-forming strategy, for anyone who believed in the truth of Aristotle's physics, Ptolemy's cosmos, Newton's absolute space, Priestley's phlogiston-based chemistry, Lavoisier's conception of oxygen or heat, or innumerable other examples was eventually proven wrong. We should, so the argument goes, expect our current theories to meet the same fate, and accordingly refrain from interpreting modern science realistically. STATHIS PSILLOS points out that this challenge to scientific realism has an extensive pedigree not yet fully appreciated, having been expressed not only by scientists like Poincaré but also by more popular writers like Tolstoy. Thus, trying to identify the true and belief-worthy portions of one's contemporary scientific worldview, while acknowledging that scientific consensuses shift over time, is not a new concern, nor one that only vexes philosophers. The lesson for realists and anti-realists is the same: we make mummies of history if we portray historical scientists as naïve realists that felt it was impossible (or even unlikely) for their most stridently defended theories to be replaced one day by something better and radically different. But to meet the challenge presented by the pessimistic meta-induction, realists need to develop a principled way of deciding which portions of any successful scientific theory are true, belief-worthy, and likely to be retained into the future, as scientists continue to develop better and better theories. P. KYLE STANFORD suggests that the scientific realism debate might be more productive if it focused less on debating whether our current best theories are approximately true, and more on debating whether it's possible to predict the future development of science reliably, and thereby rebut the pessimistic meta-induction. The division between those who believe we can make such predictions and those who do not, Stanford maintains, is more important for understanding the scientific realism debate in its current form than the division between those who think modern science is at least approximately true and those who believe something else. Focusing on this issue might also be well-advised for pragmatic reasons. Because realists and anti-realists have different expectations regarding the way science will develop in the future, JAMIE SHAW argues that "our adoption of a realist or anti-realist position has normative implications for scientific practices" (83). Using the Human Brain Project as a case study, Shaw argues—*pace* Fine but in line with the suggestions of Chang, Arabatzis, and others—that our philosophical commitments can also have normative implications for science policy making, e.g., regarding the allocation of research funds. In that case there would remain great pragmatic value in continuing to ask whether scientists, philosophers, policy makers, and others should interpret our current best scientific theories realistically, and whether realists can predict the future development of science reliably enough to rebut the pessimistic meta-induction.

Structural realists such as JAMES LADYMAN argue that the *structure* of well-established scientific theories is generally retained into whatever theories eventually replace them, for empirical theories are successful when their structure identifies "real patterns" in the modalities of nature. Ladyman offers structural realism as a way to avoid the pessimistic meta-induction, but he also claims it makes better sense of science as a successful and rational enterprise than anti-realist alternatives, providing a metaphysical account of both the various modal structures posited by our current best theories and the way those structures relate to each other at different scales. ROBIN HENDRY rightly emphasizes in his contribution that epistemic concerns about scientific theory change have been raised by many working scientists throughout history. Through a discussion of the development of structural theory in the history of chemistry, Hendry shows how the field displays a much more cumulative progression than physics, especially in light of the interpretive caution expressed by the chemists who actually developed structural theory. Like Ladyman, Hendry suggests that physics need not be treated as the only discipline with ontological authority, and that by paying more attention and respect to the special sciences the realist will be able to paint a more conservative picture of scientific theory change that supports a more optimistic induction.

In any case, PETER VICKERS argues that the historical evidence fails to support the anti-realist's pessimistic challenge to the realist. By showing that the realist can account for most instances of radical theory change, Vickers argues that "if the historical challenge is supposed to be a pessimistic induction, the realist is in a strong position since the inductive base looks rather weak. But if it is not an induction, then it seems to miss the mark, since the realist proposes a *defeasible* success-to-truth inference which will survive one or two historical counter-instances" (120). MARIO ALAI takes a less historically focused approach in his effort to defend a popular form of scientific realism—*deployment realism*—from one of its critics. Scientific realists certainly need to ensure that their philosophies of science are supported by (or at least consistent with) the historical facts of scientific change, but Alai's paper shows that the more purely philosophical issues still matter. If scientific realism can't serve the philosophical tasks it is generally meant to (e.g., by explaining the many successes of modern science), then there is really no reason to defend it against historical challenges from anti-realists.

Like Ladyman, many would-be scientific realists have developed various strategies for portraying well-established scientific theories as approximations of their successors, hoping to rebut the pessimistic meta-induction by understanding scientific theory change as a conservative process that vindicates properly considered realist commitments (e.g., Chakravartty 2007, Psillos 1999). But even if some such strategy succeeds, scientific theory change poses an additional challenge for any realist seeking to draw her metaphysical beliefs from the contemporary scientific consensus. As KERRY MCKENZIE points out, even assuming that our current best scientific theories will approximate whatever eventually replaces them, this doesn't imply that the metaphysical framework we use to make sense of our current science will "approximate" whatever framework we'll use to make sense of future science. Indeed, the realist might be able to argue that classical mechanics approximates quantum mechanics, but it's hard to understand how the different metaphysical frameworks used to give realist interpretations of those theories—determinism and indeterminism, respectively—could be said to "approximate" one another in any meaningful sense. So, if scientific realism depends on interpreting our current best science through some metaphysical framework, yet we should expect that framework to be replaced by something entirely different when science changes in the future, then scientific realism seems to be built on rather shaky metaphysical foundations in the present. K. BRAD WRAY suggests that realists haven't actually managed to rebut the basic pessimistic meta-induction anyway, claiming that they are generally exaggerating "the anti-realists' need for evidence. Any radical change of theory in a field threatens to undermine the realist's claim that changes of theory generally preserve the successes of the theories they replace by appeal to the same mechanisms and entities" (144). This contrasts with Vickers's position on this matter, and is reminiscent of TIMOTHY LYONS's claim (2002) that the pessimistic meta-induction is not actually an inductive argument needing a plethora of cases to fuel it, as it is usually understood, but rather a deductive argument that can be carried through with a single case. Lyons's contribution to this volume presents four challenges to the most common variety of scientific realism, which he calls "epistemic scientific realism," i.e., the claim that our most successful scientific theories are approximately true. He takes these challenges to be devastating, but he nevertheless maintains an axiological form of scientific realism, i.e., a realism about the aims of science: "*in changing its theoretical systems, the scientific enterprise seeks, not truth per se, but an increase in a particular subclass of true claims, those whose truth—including deep theoretical truth—is experientially concretized*" (148, italics in original). This claim need not be maintained as an article of faith, however, for it is "an empirical hypothesis ... a testable tool for further empirical inquiry" (149), one whose plausibility can be assessed through historical investigations of the way that scientists actually evaluate competing scientific theories.

PAUL TELLER raises another puzzle for the would-be scientific realist, for the "complexity of the world ensures the failure of our theoretical terms to attach to specific extensions, extension-determining characteristics, and properties and quantities generally" (157). When realists claim that the terms of their favoured theories refer to real things, he contends, this is at best an idealization, for these terms are too exacting to refer successfully to anything, strictly speaking. He goes on to sketch a way of understanding non-referring representations as true, suggesting that this can also help make sense of Ronald Giere's provocative but underdeveloped "perspectival realism" (2006). While NANCY CARTWRIGHT maintains we have "considerable evidence" (166) that the theoretical terms of our best scientific theories successfully refer, she maintains that our most general (and most prized) scientific theories cannot be given a realist interpretation, insofar as that would mean taking such universal claims to be literally true, even approximately speaking. So, instead of asking whether (or arguing that) our best scientific theories are true, Cartwright proposes that realists should ask, "What image of the world makes intelligible the successes and failures of our theoretical practices?" (165). That image, she emphasizes, cannot be one in which we've simply discovered the true, universal laws by which Nature determines what will happen, for none of our most cherished theoretical principles can be rendered as both true and universal. Instead, she suggests, our theoretical practices help us make accurate predictions because Nature employs a much looser patchwork of local laws and highly abstract principles, determining what happens "just as we do, from representations and practices like ours" (172).

Cartwright thinks this practice-centred understanding of how our most general theoretical principles relate to the world allows us to interpret many lower-level scientific models realistically, i.e., as true factual claims. But the scientific models of the future might not be the familiar type she has in mind, for as CLIFF HOOKER and GILES HOOKER discuss, machine learning is increasingly being used in the construction of predictive models. The resulting models are generally black-boxed in a way that makes it difficult (if not impossible) to give anything like a realist interpretation of them. While there are reasons for the realist to find this concerning, the use of machine learning is not obviously a bad thing from the perspective of someone seeking only to predict the behaviour of some system, device, or phenomenon. Would it have been better or worse, the Hookers ask us to consider, if Newton had used machine learning to develop a computer program capable of making incredibly precise and accurate predictions of how any system of massive objects will interact, but incapable of revealing the inverse square law of universal gravitation? In general, is it better to have theories and models that paint a conceivable picture of reality, or instead to have a set of ontologically uninterpretable algorithms that make far better predictions (so far as we know!)? Perhaps future scientists will come to prefer the latter, but here's the issue: if scientific realists want to update their beliefs about reality as science itself develops, but the future of science is the production of models that permit no realist interpretation, what would that mean for the future of the scientific realism debate?

And the future of the debate over scientific realism is, of course, exactly what this collection is about. In the end, what it makes most clear is that there is no significant consensus regarding what the future of the scientific realism debate should be; as a corollary, however, it also makes clear that the future of this debate is full of possibilities.

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