Catastrophism, Uniformitarianism, and a Scientific Realism Debate That Makes a Difference

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Some scientific realists suggest that scientific communities have improved in their ability to discover alternative theoretical possibilities and that the problem of unconceived alternatives therefore poses a less significant threat to contemporary scientific communities than it did to their historical predecessors. I first argue that the most profound and fundamental historical transformations of the scientific enterprise have actually increased rather than decreased our vulnerability to the problem. I then argue that whether we are troubled by even the prospect of increasing theoretical conservatism in science should depend on the position we occupy in the ongoing debate concerning scientific realism itself.

In earlier work I argued (Stanford 2006, 44–47) that one advantage of what I called the "problem of unconceived alternatives" over the traditional pessimistic induction is that it asserts a historical continuity between the the-

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orists rather than the theories of past and present science. After all, many contemporary scientific theories differ from their predecessors in ways that might reasonably affect our assessment of their likely truth, but we would seem to have little reason to think that today's scientists are more creative or better able to exhaust the space of well-confirmed theoretical possibilities than were even the most brilliant scientific minds of the past. But thoughtful commentators such as Peter Godfrey-Smith (2008) and Patrick Forber (2008) have rightly found room for concern here, noting that contemporary scientific communities might differ from their historical predecessors in ways that decrease their vulnerability to the problem even if individual scientists do not: "This may be a distinctive feature of the epistemology of eliminative inference—its unusual level of dependence, compared to other kinds of inference, on community-level properties . . . not any multiplication of personnel would help here, but I do think that community size and information flow are significant disanalogies between the situation in the 18th–19th Centuries and the situation we face when we ask about our own exercise of eliminative inference" (Godfrey-Smith 2008, 142–43). Godfrey-Smith presumably does not mean to suggest that today's scientific communities have nothing to fear from the problem of unconceived alternatives simply because they are bigger, better connected, better organized, better funded, and more sophisticated in a wide variety of ways than those of the past. To reach that reassuring conclusion, we would have to believe not simply that scientific communities have become better over time at systematically exploring the spaces of alternatives from which contemporary theoretical accounts of nature are drawn, but also that we have finally passed over some kind of threshold in this respect, and that we are now good enough at doing so to exhaust such spaces or at least come near enough that we can safely dismiss the theoretical options that remain unexamined.

We might reasonably think, however, that this line of argument at least establishes that we should expect contemporary scientific communities to be systematically less vulnerable to the problem than were their historical predecessors and/or that we should expect the problem of unconceived alternatives to become progressively less significant over time. Here I will contest even this more modest conclusion, arguing that those historical transformations of the scientific enterprise independently regarded by historians of science as most profound and fundamental—the professionalization of science in the middle decades of the nineteenth century, the shift to peer-reviewed funding of academic science by the state following World War II, and the ongoing expansion of so-called big science—each served instead to increase rather than decrease the vulnerability of the resulting

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scientific communities to the problem.[1] I first argue that the unprecedented obstacles generated by these developments for the pursuit of transformative or theoretically revolutionary science at least offer substantial reasons for doubting whether contemporary scientific communities are really less vulnerable to the problem of unconceived alternatives than were their historical predecessors, and perhaps even grounds for believing that they are instead more vulnerable to it. I then go on to suggest that even confronting this question invites us to reconceive what is most fundamentally in dispute between the two sides of a realism debate worth having.

For most of its modern history, what we now call scientific inquiry was conducted by what historian Martin Rudwick (1985) famously called "gentlemanly specialists" supported largely by independent wealth or aristocratic patronage. As a leading textbook points out, these were "men who were leading figures in their field but who did not gain their income from science and would have been suspicious of anyone who did" (Bowler and Morus 2005, 320–21). And as Steven Shapin notes,

> Early modern students of nature conducted their inquiries in a variety of institutional settings and occupied a variety of social roles. Some were remunerated to conduct their inquiries, but not many. . . . The university professor was engaged to be a custodian of knowledge and to transmit it to the next generation. The physician and surgeon were remunerated to keep people healthy and to treat them when they were ill. The cleric was responsible for being a mouthpiece for God's words; for living a blameless, if not holy, life; and for ensuring the moral conduct of his community. All of the people occupying these roles might do scientific research (as we now put it), but doing it was not their *business*. The early modern Speaker of Truth about Nature was, almost without exception, not a professional but an amateur. (2008, 35, emphasis in original)

The fact that such gentlemanly amateurs did not make a living from their own original scientific research meant that they had enormous freedom to conduct their scientific work in whatever way and on whatever subjects they liked—they were free to simply satisfy their own curiosities, to ride idiosyncratic hobbyhorses, to grind ideological axes, and to otherwise pursue lines of research and theorizing that their colleagues might regard as misconceived, unpromising, or uninteresting, if only because they were not being paid to conduct that work in the first place. But with the advent of scientific professionalization across Europe and the United States in the middle decades of the nineteenth century, a scientist's own livelihood came to depend quite directly on the estimation of the significance of her own indi-

1. Because I review these historical developments in greater detail elsewhere (Stanford, forthcoming), I will describe them only briefly here.

vidual scientific achievements by her peers in the community of scientific professionals. In fact, imposing restrictions on the range of appropriate research questions, the activities undertaken in attempts to answer them, and the sorts of theoretical proposals regarded as promising, serious, or even genuinely scientific in the first place was typically an important part of the process by which the members of such newly professionalized scientific communities sought to establish themselves as professionals in the first place and to distinguish themselves from those they dismissed as mere amateurs, enthusiasts, or dilettantes. Following professionalization, the members of a scientific community who ignored its collective wisdom concerning reasonable assumptions, important problems, and promising theoretical approaches toward solving them did so at considerable risk to the fortunes of their own scientific careers and livelihoods, generating powerful disincentives for scientific professionals to pursue theoretically unorthodox or iconoclastic science.

Further pressures toward theoretical conservatism would emerge in the decades following World War II, however, as "massive changes in the social and cultural realities of American science" created "a state of affairs that had no substantial historical precedent or ancestry" (Shapin 2008, 64). The importance of radar and the Manhattan Project to the Allied victory generated considerable enthusiasm (especially in the United States) for efforts to enlist scientific inquiry itself in pursuit of military power, economic competitiveness, and other forms of strategic advantage by the state. Institutions like the National Science Foundation (NSF) were founded at this time to foster, in Vannevar Bush's famous words, "the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for the exploration of the unknown" (Bush 1945, 12). But the need for accountability and oversight in the distribution of public funds has since driven this process in what writers on science policy widely regard as a much more conservative direction:

> Perhaps times have changed, or perhaps free intellects were never so freely at play in well-funded laboratories. However that may be, today's free intellects do not play freely, but instead find themselves tethered to national goals for health, defense, economic competitiveness, and the like. Colleges, universities, and research institutes have come to depend on federal research support, a dependence that is transmitted (and perhaps amplified along the way) to the scientists and scholars they employ, further limiting intellectual "free play." New ideas must pass through the filter of peer review, which stimulates opposition and encourages applicants to be cautious, if not conservative, in their proposals. (Chubin and Hackett 1990, 10)

After all, a researcher who hopes to have her NSF or National Institutes of Health (NIH) grant proposal funded had better be proposing something new, but she had also better not stray too far from conventional wisdom in her

field about what are promising approaches, reasonable theoretical assumptions, and tractable questions. Even the prospect of such a review would seem likely to generate far more conservative grant proposals, as their authors anticipate the likely responses of review boards or committees and then simply seek to invest their own time and energy as efficiently as possible. And of course, the progress of scientific careers now depends on the funding of grants by such extramural agencies: external grants are no longer supererogatory incentives offered for the pursuit of particularly promising or exciting research, but are now instead the very backbone of the system by which academic science is conducted at all.

The view that the contemporary apparatus of peer-reviewed grant proposals for specific research projects has generated increasingly conservative scientific research represents not only something approaching a consensus among writers on science policy and a frequent complaint among scientists themselves but also (and more surprisingly) a persistent point of concern among the very administrators who oversee that apparatus and its distribution of resources at institutions like the NSF and NIH (see Stanford, forthcoming). What little experimental evidence we have on closely related questions seems simply to reinforce this concern: Mahoney (1977), for example, found that referees rated a fictitious manuscript's methodology, data presentation, and overall scientific contribution as significantly higher and were more likely to recommend publication when the results agreed with rather than contradicted the referee's own presumed theoretical perspective, while Resch, Ernst, and Garrow (2000) found that reviewers rated fictitious studies supporting unorthodox therapies less favorably than those supporting more conventional treatments even when faced with equally strong supporting evidence.

Of course, Kuhn argued long ago that most science is and always has been "normal science" seeking to make progress along the lines of contemporary theoretical orthodoxy. But the contemporary system of peer-reviewed grant proposals and competitive funding for specific research projects in academic science has nonetheless made it the case for the first time in history that peer judgments of plausibility and promise now determine not only the professional standing, status, and remuneration of the scientists who have actually achieved particular results or developed particular theoretical proposals but also the lines of research and theoretical development that will be supported (and therefore can even be pursued) in the first place. We might reasonably wonder whether Kuhn's influential description of what he called "normal science" isn't actually better regarded as a description of grant-driven research in physics after World War II, and whether the historical evidence doesn't suggest a consistent evolution of what Kuhn called the "essential tension" toward science that is ever more 'normal' in character.

Moreover, Kuhn also famously suggested that a crucial ingredient in the possibility of theoretical revolution was the intellectual flexibility and freedom of younger scholars and those new to a given scientific field, but this very flexibility and freedom would seem to be profoundly threatened by the combination of this system of peer-reviewed grant proposals with so-called big science: the increasing amalgamation of scientific activity into ever larger and more complex research projects involving the increasingly widely distributed efforts of ever larger groups of scientists and institutions. Within such collaborations, of course, a given proposal's degree of theoretical iconoclasm is limited by the perceived chances of rejection that the most risk-averse member of the collaboration is willing to tolerate. But far more importantly, the ongoing expansion of big science has established and entrenched a much stricter hierarchical organization in the pursuit of both scientific work and scientific careers. Younger scholars and others new to a scientific field must now spend many years working as graduate students and postdocs under the supervision of (and advancing the existing research programs of) more established researchers before starting research programs of their own. Learning science today thus involves finding, proposing, and conducting research projects in collaboration with one's advisor or mentor with the best chances of being approved by groups of established researchers in the field, and graduate education in the sciences now typically includes explicit instruction (often entire courses) dedicated to teaching graduate students how to write grant proposals maximally likely to be accepted by review panels at institutions like the NSF and NIH. Nor are the many somewhat more advanced scholars still working toward securing permanent academic posts free to risk investing significant time or energy in ambitious or revolutionary proposals without near guarantees of predictable results in the short term.

This worry is reinforced by a striking recent examination of the proportions of primary research grants awarded to younger and newer researchers by the NIH, which notes that the median age at which a PhD researcher first becomes a principal investigator on her own NIH grant has risen steadily from age 36 in 1980 to age 42 in 2002 (National Research Council 2005, 39). In addition, the authors report, "The number and percentage of grants awarded to younger researchers has been decreasing. While investigators under the age of 40 received over half of the competitive research awards in 1980, that age cohort received fewer than 17 percent of awards in 2003. . . . Moreover, the percentage and absolute number of awards made to new investigators—regardless of age—has declined over the last several years, with new investigators receiving less than 4 percent of NIH research awards made in 2002" (1). While the increasingly hierarchical organization of scientific work and careers multiplies opportunities for contact, training, and mentorship between more senior and more junior scientists and surely

improves the research produced thereby in a variety of ways, it just as surely serves to radically limit the extent to which younger or newer scholars are free to strike out on their own to explore new or unorthodox ideas that challenge existing theoretical conceptions of nature. Those who point out that most of modern science has happened since World War II and that we have seen few truly fundamental theoretical revolutions in that time might consider an alternative to the truth of contemporary theories as an explanation of this fact: science in this period has entrenched powerful incentives for seeking incremental improvements to existing theoretical orthodoxy and unprecedented obstacles to pursuing revolutionary, transformative, and/or theoretically unorthodox scientific inquiry.

It is not at all clear, however, how we should trade off the impact of these developments either against those emphasized by Godfrey-Smith or against others that are also undoubtedly important, such as the increasing inclusiveness and diversity in the membership of contemporary scientific communities. Thus, the historical evidence alone does not allow us to conclude with confidence that contemporary scientific communities are on balance less effective vehicles for proposing, exploring, and developing fundamentally new theoretical conceptions of various parts of nature than their historical predecessors were, only that the most profound historical transformations of those communities offer some substantial reasons for doubting whether they are any more so.

As we noted above, however, in recent decades the NSF and other granting agencies have themselves become increasingly concerned about the extent to which existing processes of review are able to foster what they call "transformative research" dedicated to "revolutionizing entire disciplines; creating entirely new fields; or disrupting accepted theories and perspectives" (Bement 2007). Accordingly, the NSF now requires authors and referees to explicitly comment at length on the "potentially transformative" character of the proposals they submit or review and exhorts review committees and program directors to support such research. Elsewhere I suggest (Stanford, forthcoming) that we could pursue transformative science more effectively by instead diversifying the methods we use to distribute the resources to conduct that inquiry, but it is crucial to recognize that any effective means of pursuing this objective will certainly have costs as well as benefits: if we fund more theoretically unorthodox science that contemporary experts judge to be risky and uncertain, we will almost certainly wind up funding more science that goes nowhere and achieves nothing. We must therefore first ask whether the NSF and similar institutions are right to think that we should be seeking to fund contemporary science in a less theoretically conservative way than we do at present, and this question brings us back to the dispute concerning scientific realism with which we began.

Perhaps surprisingly, I suggest that the classical scientific realist can afford to be cavalier or even enthusiastic about evidence of increasing theo-

retical conservatism in science. After all, she thinks that contemporary theories have things sorted out at least roughly right and that our remaining errors are simply errors of detail. She is confident that the theories embraced by future scientific communities will seem both to us and to the members of those communities simply to be corrected, expanded, and more sophisticated versions of the ones that we ourselves have accepted. As long as review panels police only the most broadly accepted points of theoretical orthodoxy in their funding decisions, the realist should be perfectly happy to rule out consideration of lines of research or theoretical proposals that are radically or fundamentally at odds with existing theories, as she thinks it quite unlikely that any of these will ultimately come to be accepted in the future. Indeed, the farther from existing theoretical orthodoxy a proposed research project strays, the more confident the realist will be that it is misguided in some fundamental way—of course, we might learn something important and useful from research willing to call fundamental theoretical claims or principles into question, but the realist has absolutely no reason to think we will learn anything more important or useful than we would by instead supporting a line of research adhering more closely to the theoretical orthodoxy that she assures us is at least approximately true. Indeed, the scientific realist might well celebrate any evidence of increasing theoretical conservatism in our distribution of what are, after all, scarce public resources for scientific inquiry.

By contrast, consider the long line of opposition to scientific realism rooted in evidence drawn from the historical record of scientific inquiry itself, including not only the challenge from unconceived alternatives with which we began but also prominent and influential lines of argument articulated by figures such as Duhem, Poincaré, Kuhn, and Laudan. Although there are important differences between such "historicist" critics of scientific realism, there is an even more significant commonality: each sees us as being somewhere in the midst of an ongoing historical process in which successful scientific accounts of various parts of nature are repeatedly replaced with even more impressive and powerful successors making fundamentally distinct claims about the constitution and/or operation of those parts of nature. Such historicists doubt that even the best currently available conceptual tools we have for thinking about nature will retain that status indefinitely as future inquiry proceeds, and they shudder to think of all the 'transformative' work throughout the history of science that would never have been conducted in an environment in which the serious pursuit of scientific research required convincing a panel of peers broadly steeped in current theoretical orthodoxy that it was likely to bear worthwhile fruit. Accordingly, the historicist critic of scientific realism thinks that one of the most important ambitions of the scientific enterprise should be identifying and developing the fundamentally distinct and even more powerful successors that will ultimately replace even

the most impressive theories of the present day, provided that we continue to look for them.

But what of the increasingly influential and more historically sophisticated variety of scientific realist who cheerfully concedes the historicist's claim that further discoveries and theoretical developments probably will overthrow important parts of existing scientific theories or at least lead us to see them in a very different light than we now do? This sophisticated latter-day scientific realist grants that the future of science is likely to be characterized by quite a lot of both continuity and upheaval, and that such upheaval will periodically be on the order of the rediscovery of Mendel, the eclipse of Dalton's atomism, and the replacement of Newton's mechanics with general and special relativity. She allows that our successors will ultimately come to see the scientists of our own day in very much the same way that we ourselves view our own historical predecessors: as having grasped what later theoretical lights would count as many central and important truths about the world but also as holding plenty of beliefs about nature that are by those same lights no less misleading, misguided, mistaken in emphasis, incomplete, or even downright false than Newton's mechanics or Dalton's atomic chemistry or Weismann's theory of the germ plasm now seem to us. But she does not see such concessions as giving us any reason to deny that contemporary theories are nonetheless approximately true.

I now want to suggest that the time has come to abandon this way of framing the central locus of disagreement in the dispute concerning scientific realism. To see why, notice that this more sophisticated realist has just granted everything that her historicist opponent was concerned to assert in the first place: after all, the historicist critic's central commitment was to the idea that we are in the midst of an ongoing historical process in which our theoretical conceptions of nature will continue to change just as profoundly and fundamentally as they have in the past. That is, she expects the future of a sufficiently vigorously and creatively pursued scientific enterprise to look very much like its past and for our successors to see our own grasp of nature in very much the same way that we see that of our own historical predecessors. The latter-day realist may emphasize different aspects of this shared vision of the future of science by insisting that the profound continuities between past scientific theories and those of the present justify the claim that those past theories were at least "approximately true," while her historicist opponent instead emphasizes the depth and significance of the discontinuities and revisions by denying that those same past theories were even "approximately true"; however, this is simply a difference of style or taste in applying the expression "approximately true" rather than a substantive disagreement between them.

In essence, I am suggesting that we should reconceive the most important locus of disagreement in the debate concerning scientific realism along

the lines of the great clash between catastrophism and uniformitarianism in nineteenth-century geology. Uniformitarians argued that the broad topographic and geographic features of the Earth were produced by earthquakes, floods, volcanoes, and other natural causes acting consistently over long periods of time at the same frequencies and magnitudes we now observe. By contrast, their catastrophist opponents held instead that such causes had operated in considerably stronger degrees in the past, on the order of the difference between a contemporary flood and the great Noachian deluge reported in the Christian Bible, and that the earth has steadily and progressively quieted down over the course of its history. Catastrophists thus saw the central features of the earth's large-scale geography and topography as having been laid down by the truly violent and profound geological changes already confined to the distant past, and they saw present-day natural causes as capable of modifying that large-scale geography or topography only in what are by comparison fairly marginal, limited, and minor ways. Uniformitarians held instead that given enough time to work, present causes will continue to modify even the fundamentals of that geography and/or topography just as profoundly as they have been modified in the past. Similarly, a scientific catastrophist expects the future of scientific inquiry to be quite different from its past. She doubts that there will be further profound theoretical revolutions of the sort by which Newton's mechanics, or Dalton's atomism, or Weismann's theory of the germ plasm ultimately came to be supplanted, qualified, amended, adjusted, or transformed; she insists that even the most creatively and vigorously pursued scientific inquiry will probably not produce such upheavals, transformations, or revolutions in the fundamentals of the scientific image by which we are now possessed. It is in this sense that catastrophists in both cases see us as having largely completed historical processes that uniformitarians insist are instead still ongoing.

Are there any such catastrophists? Indeed there are, for this is surely the most natural reading of the claim that the incredible practical and explanatory achievements of contemporary scientific theories show that they must be at least approximately true or would otherwise be a 'miracle'. It is certainly what our students take us to mean when we formulate the standard "explanationist" defense of realism: that the best (or only) explanation for the success of many contemporary scientific theories is that they are true. They (and most of us, for that matter) would be mystified if that defense claimed instead that "contemporary theories are approximately true . . . just like the wave theory of light, Newton's mechanics, the caloric theory of heat, Dalton's atomic chemistry, phlogistic chemistry, and Weismann's theory of the germ-plasm!" In short, the realist cannot have it both ways: if scientific realism claims to represent or at least vindicate the judgment of common sense concerning the truth of our best scientific theories, it must attribute a form of "approximate truth" to them that is simply not consistent with the

uniformitarian conviction that in the course of further inquiry those theories will ultimately be overturned, supplanted, or transformed in the manner of their historical predecessors. And a uniformitarian who insists on retaining the realist label and/or the right to characterize contemporary scientific theories as "approximately true" has not thereby substantively distinguished her uniformitarian variety of "realism" from the views of traditional historicist critics of realism such as Duhem, Poincaré, Kuhn, and Laudan.[2]

At present, however, the profound difference between catastrophist and uniformitarian scientific realists is thoroughly obscured by their common allegiance to the elastic verbal formula of "approximate truth." What I am suggesting is that the most fundamental issue is not what is or is not "approximately true," but instead the extent to which the scientific future will (or at least still could[3]) resemble the scientific past. The uniformitarian holds that it will (or could) and will be commensurately troubled by evidence that scientific inquiry has become systematically more theoretically conservative throughout its recent history; like the historicist critic of scientific realism, she will be concerned to identify ways to pursue contemporary science that do not entrench such theoretical conservatism. By contrast, the catastrophist need not be troubled by any evidence of increasing theoretical conservatism in science, for she does not think that there is really any need for "revolutionizing entire disciplines" or "disrupting accepted theories and perspectives," nor does she have any obvious reason to be worried about investing nearly all public resources for scientific inquiry in ways that maximize theoretical conservatism (even if other sorts of conservatism might still trouble her). Whatever reasons the catastrophist may have for encouraging institutions like the NSF and NIH to support "transformative" or revolutionary science, the uniformitarian has all those reasons and at least one more that is far more compelling: she expects the search for fundamentally distinct and even more instrumentally powerful successors to contemporary scientific theories to ultimately achieve its intended object, whether in general or in some particular domain of science. Thus, unlike the vast majority of differences separating various parties to debates concerning scientific realism,

2. Of course, some uniformitarians also believe that the historical record puts us in a position to prospectively identify those parts or aspects of sufficiently successful current scientific theories that will be retained by their historical successors. I think that the historical record shows instead that the continuities between successful theories and their successors are fundamentally unpredictable, but this remains an important intramural disagreement within the uniformitarian camp in any case. My suggestion is not that the difference between catastrophists and uniformitarians is the only one that matters, merely that it is the one that matters most.

3. Of course, if contemporary theories persist simply because we are no longer vigorously searching for alternatives to them, this hardly vindicates catastrophism or classical scientific realism.

the difference between catastrophism and uniformitarianism actually makes a difference to how we should go about pursuing scientific inquiry itself, and perhaps that is ultimately the best reason of all to see it as the central axis of disagreement in that debate.

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