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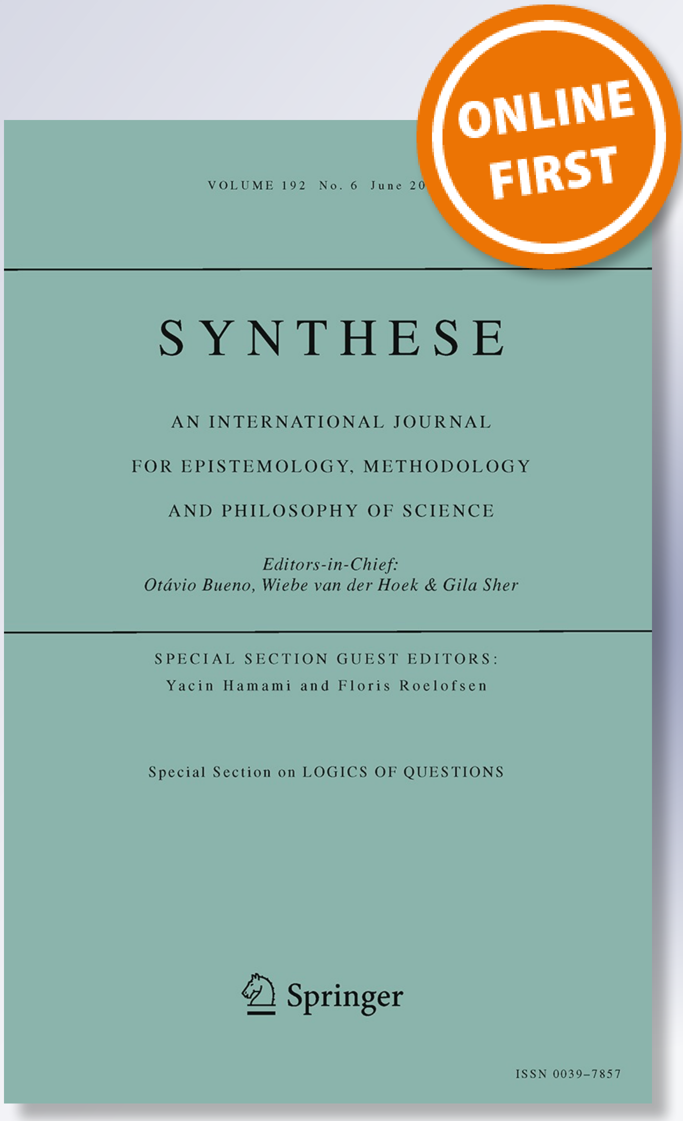

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S.I. : CONCEIVED ALTERNATIVES

Unconceived alternatives and conservatism in science: the impact of professionalization, peer-review, and Big Science

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Abstract Scientific realists have suggested that changes in our scientific communities over the course of their history have rendered those communities progressively less vulnerable to the problem of unconcieved alternatives over time. I argue in response not only that the most fundamental historical transformations of the scientific enterprise have generated steadily mounting obstacles to revolutionary, transformative, or unorthodox scientific theorizing, but also that we have substantial independent evidence that the institutional apparatus of contemporary scientific inquiry fosters an exceedingly and increasingly theoretically conservative form of that inquiry. I conclude that contemporary scientific communities are actually more vulnerable to the problem of unconceived alternatives than their historical predecessors, and I briefly suggest how we might seek to pursue scientific inquiry in a less theoretically conservative way.

Keywords Scientific Realism · Instrumentalism · Professionalization · Peer-review · Theoretical Conservatism · Transformative research

1 Revisiting the challenge: theories, theorists, and scientific communities

I have previously argued (Stanford 2001, 2006) that the most serious challenge to scientific realism is posed by what I called the problem of unconceived alternatives. The historical record of scientific inquiry itself, I suggested, offers abundant evidence

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of the repeated failure of scientists and scientific communities to even *conceive of* fundamentally distinct alternatives to extant theories that were nonetheless both scientifically serious and reasonably well-confirmed by the evidence available at the time. And I proposed that this robust historical pattern gives us every reason to believe that there are probably such unconceived alternatives to even the most successful scientific theories of our own day.

I also suggested that this particular line of historically-motivated challenge to scientific realism is substantially harder to dismiss or respond to than the traditional Pessimistic Induction because it projects into the future an historical pattern exhibited by *scientists* rather than scientific theories. That is, while the unprecedented predictive and explanatory power, scope, precision, and other virtues of at least some contemporary theories might protect them from invidious comparison with many earlier successful scientific theories ultimately discovered to be false, it seems much less plausible to think that today's scientists or scientific communities are more creative or otherwise better able to exhaust the space of well-confirmed alternative theoretical possibilities than were even the most ingenious and imaginative of their historical predecessors. But commentators like Forber (2008) and Godfrey-Smith (2008) have resisted this suggestion, arguing that even if individual scientists are no better able to exhaust the space of theoretical alternatives well-confirmed by given body of empirical evidence than their historical predecessors have turned out to be, contemporary scientific *communities* might be nonetheless. Godfrey Smith rightly points out that "[w]e have become used to the idea that a community or population can embody epistemic properties that no individual has" and goes on to suggest that

> for eliminative inference to work well in theoretical science, there has to be a decent-sized community, and an appropriate incentive structure. 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 eighteenth–nineteenth centuries and the situation we face when we ask about our own exercise of eliminative inference. (Godfrey-Smith 2008, pp. 142–143; references and footnote omitted)

Godfrey-Smith surely makes an important point here about the disanalogies between earlier scientific communities and those of the present day: contemporary scientific communities are unquestionably bigger, better-connected, better-organized, better-funded, and more sophisticated by almost any conceivable measure than those of the past. Of course, to show that contemporary scientific communities have little or nothing left to fear from the problem of unconceived alternatives we would need to establish not simply that such communities have improved over time in their ability to examine and consider alternatives to extant scientific theories, but also that this improvement has been so dramatic that we are now able to exhaust the spaces of theoretical alternatives from which contemporary accounts of nature are drawn, or at least come near enough that we can afford to ignore whatever theoretical options remain presently unexamined. And if we acknowledge that our scientific communities have remained systematically vulnerable to the problem throughout the history of the scientific enterprise to date, any such conclusion might seem unreasonably optimistic.

Nonetheless, we might see this Godfrey-Smith/Forber line of argument as seeking instead to establish merely that contemporary scientific communities are substantially *less* vulnerable to the problem of unconceived alternatives than their predecessors and that we should expect the magnitude or severity of the problem to have systematically decreased over the history of the modern scientific enterprise. In the remainder of this essay I will argue that even this considerably more cautious conclusion is fundamentally misguided. Although some historical developments have surely rendered our scientific communities better able to examine a wide range of serious and well-confirmed theoretical possibilities,[1] I will argue that each of the historical transformations of the scientific enterprise independently recognized as most profound and significant by historians of science has had just the opposite effect. That is, I will argue that the professionalization of science in the middle decades of the nineteenth century, the shift to state support of academic science through peer-reviewed proposals for particular research projects following World War II, and the ongoing acceleration and expansion of so-called 'Big Science' have served to reduce not only the incentives but also the freedom scientists have to pursue research that challenges existing theoretical orthodoxy or seeks to develop fundamental theoretical innovation. Along the way I will point out evidence of the mounting concerns expressed by a wide range of scientists, administrators, and writers on science policy regarding the resulting intellectual and theoretical conservatism that has come to characterize contemporary scientific inquiry. I will suggest that the combination of these various forms of evidence should convince us that contemporary scientific communities are in fact more vulnerable to the problem of unconceived alternatives than their historical predecessors rather than less so, and I will briefly suggest how we might seek to pursue contemporary science in a less theoretically conservative way.

1.1 From gentlemen to professionals

Historians of science have long noted that the most profound transformation in the social organization of modern scientific activity was the professionalization of the enterprise over the middle decades of the nineteenth century in Europe and the United States. Prior to such professionalization, science was pursued primarily by what Martin Rudwick (1985) famously called 'gentlemanly specialists' largely supported by their

[1] It is worth noting, however, that Godfrey-Smith's "information flow" may not be an especially promising candidate here: as Zollman (2007, 2010) has shown, increasing information flow among the agents in an idealized scientific community increases the speed at which those communities converge on a single view but also increases the chances that such communities will reach premature consensus in favor of a mistaken or suboptimal view by excluding alternatives too quickly. This research is part of a rich literature concerning the social organization of scientific inquiry (including work by David Hull, Philip Kitcher, Michael Strevens, Miriam Solomon, and many others) that I will henceforth largely ignore. This tradition has tended to focus on questions about how to optimize or explain various aspects of our own scientific communities, while I am instead seeking to establish the epistemic consequences of the most important changes to those communities over the course of their history.

own wealth, royal and aristocratic patronage, or other independent means. As Steven Shapin points out,

> 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. (Shapin 2008, p. 35; original emphasis)

Early modern scientific societies were typically subscription-based gatherings of wealthy enthusiasts and hobbyists who paid for the privilege of membership, helping to fund the society's activities. As Bowler and Morus point out, even their most influential members 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" (2005, pp. 320–321). And even those few who risked courting such suspicion by making a living through science in the seventeenth and eighteenth centuries typically did so by providing public lectures and demonstrations of existing principles and applications rather than by their own research—Michael Faraday, for example, was much better known for his entertaining public lectures and demonstrations of known electrical effects than for his own electrical theories. Although states and other institutions gradually came to recognize the practical military and commercial power of science and to offer financial incentives for engaging in scientific research, these often took the form of prizes awarded for solutions to particular practical challenges (such as the famous Longitude Prize) or theoretical innovations (such as the French Academy of Sciences Prize awarded to Augustin-Jean Fresnel for his improved formulation of the wave theory of light), rather than salaries for employees primarily engaged in research (with the early exception of France under Louis XIV and expanding in the Napoleonic period[2]). Thus, while the memberships of early modern scientific communities were certainly exclusive, within the broad parameters set by their commitment to the new natural philosophy the members of such communities were relatively unconstrained in the approaches they took to their scientific research, if only because virtually no one made a living from or was even paid to conduct that research in the first place.

This arrangement shifted dramatically following what Shapin (2008, p. 14) describes as the "social and cultural transition from science as a calling to science as a job", not long after William Whewell coined the term 'scientist' to mark a new

[2] There are, of course, exceptions to each of the extremely broad historical patterns I will describe, and the early professionalization of science in France is only one prominent example. In the present context, however, the most important point to keep in mind is that these are the *exceptions* to well-documented and widely recognized general historical patterns (here, that the professionalization of science occurred *by and large* in the middle decades of the nineteenth century). Most important for our purposes is a clear bird's-eye view of the central features of the scientific terrain as a whole.

institutional identity emerging from the earlier traditions of natural philosophy and natural history. Many historians have emphasized just how fundamental and wide-ranging the effects of this transformation were:

> Recent writers on the history of science in America generally agree that conditions underlying the pursuit of science changed drastically during the nineteenth century. By the middle of the century, the earlier pattern of gentlemanly scientific activity was rapidly becoming obsolete. The amateur was in the process of being replaced by the trained specialist—the professional who had a single-minded dedication to the interests of science. The emergence of a community of such professionals was the most significant development in nineteenth-century American science. (Daniels 1976, p. 63)

Most critically, where gentlemanly specialists had been largely free to conduct their research in whatever way and on whatever subjects they liked, this emerging class of scientific professionals depended *for their livelihoods* on the estimation of the achievements and further promise of their research by their professional colleagues, especially following the incorporation of science into the changing academic curriculum of the nascent modern research university. Thus, after the middle decades of the 1800's, scientists could no longer afford to be indifferent to those colleagues' collective assessment of the interest and importance of their own scientific research because that research was how they made a living. There is surely much to celebrate in the emergence of such professional communities and many ways in which these developments presumably improved the quality of the resulting scientific work. But such a community of scientific professionals is also, almost by definition, far more homogeneous in its thinking, in its assumptions, in its motives, and in the dimensions of its creative freedom, than a community made up largely of gentlemanly amateurs supported by independent wealth, aristocratic patronage, and the like. Indeed, restricting the sorts of research questions regarded as appropriate to the discipline, the sorts of activities undertaken in attempts to answer them, and the sorts of answers and theoretical proposals regarded as plausible or even genuinely scientific in the first place were among the most common ways in which groups of scientific practitioners sought to establish and mark themselves off *as professionals* and to distinguish themselves from those they dismissed as mere amateurs and dilettantes.

Of course, not all gentlemanly scientists were independently wealthy and the earlier traditions of aristocratic and royal patronage did sometimes involve restrictions or constraints on the content of scientific research, but these were not typically restrictions on the methods or theoretical presuppositions of that research because few scientific patrons were sufficiently expert to even formulate restrictions in these terms (to say nothing of the many self-supported gentleman scientists who answered to no patron at all). And in any case, such constraints as were explicitly imposed on particular researchers were typically idiosyncratic: they did not compromise the diversity of theoretical approaches within a given scientific community because they did not derive from a common *source* in the collective wisdom of that community, while professional scientists ignored that collective wisdom only at the peril of losing their professional standing, academic positions, and sources of income. Professionalization thus seems to have changed not merely the sorts of incentives that would henceforth motivate the bulk of scientific activity, but also (and much less obviously) the character of the scientific work thereby incentivized. Early modern gentleman scholars engaged in science in order to cultivate their own intellects, to impress other members of the gentlemanly class, and perhaps most importantly to establish reputations by the *originality* of their scientific contributions. With the advent of scientific professionalization, it would seem that scientists themselves became substantially less free to simply satisfy their own curiosities on their own terms, to ride idiosyncratic hobbyhorses, to grind ideological axes, and to pursue lines of research and/or theoretical suggestions that their colleagues might regard as fundamentally misconceived, unpromising, or uninteresting.

1.2 Science meets the state

Shapin goes on to point out, however, that professionalization was itself a gradual and piecemeal process. Charles Darwin, the most famous scientist of the nineteenth century, was a gentleman-amateur, while Gregor Mendel was a monk, and "In Britain alone, the list of amateur-scientists in the late eighteenth and nineteenth century includes some of the most influential figures in all the sciences" (Shapin 2008, pp. 41–42). It was World War II and its aftermath, he argues, which served to fully integrate scientific inquiry into the machinery of commerce and the Cold War State, as the importance of radar and the Manhattan Project to the Allied victory generated widespread support for efforts to more firmly enlist scientific inquiry in the state's own pursuit of military, economic, and other forms of competitive advantage. This process, he notes,

> brought about massive changes in the social and cultural realities of American science, in understandings of what science was and who the scientist was. These changes were matters of degree, but they occurred on such a scale that they appeared to participants, as they do to later commentators, to bring about a state of affairs that had no substantial historical precedent or ancestry. (Shapin 2008, p. 64)

Most crucially, along with the steady expansion of public resources devoted to its pursuit came a new system of incentives and constraints for the conduct of scientific work, including what is essentially the contemporary apparatus of peer-reviewed grant proposals and competitive funding for research in academic science by a small number of centralized agencies of the state.

Perhaps ironically, the primary motivation for this unprecedented system of incentives was to *protect* the independence and bold creativity of scientific thought while putting its increasingly evident practical power more firmly in service of the state itself. The heart and soul of the successful case made by Vannevar Bush to then-U.S. President Franklin Roosevelt in 1945 for establishing what would ultimately become the National Science Foundation (NSF) was the proposition that

> Scientific progress on a broad front results from 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, p. 12)

Of course, from the point of view of contemporary scientists, engaged in an endless process of carefully identifying, preparing, and putting forward precisely those research proposals that they think have the best chances of being funded by the NSF and institutions like it, this description of unfettered inquiry in which scientists boldly stride in whatever directions their intellectual curiosity happens to take them might seem quaint or even charming. Commenting on the view of scientific activity implicit within this famous passage, Chubin and Hackett write:

> 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, p. 10)

Many prominent and successful scientists have been profoundly troubled by these developments. Luis Alvarez, for example, once famously described the peer review system as "the greatest disaster visited upon the scientific community in [the twentieth] century" noting that "No group of peers would have approved my building the 72-inch bubble chamber" (1987, pp. 200–201). Such suspicions would seem to be supported by experimental studies of peer-review in publication decisions. Mahoney (1977) found that referees whose presumed theoretical perspective agreed with that of a submitted manuscript were more likely to recommend publication and gave significantly higher ratings to its methodology, data presentation, and overall scientific contribution. And Resch et al. (2000) found that reviewers rated fictitious studies supporting conventional treatments more highly than those supporting unorthodox therapies even when the supporting evidence was equally strong.

Writing in *Science*, the physicist Richard Muller offers a further diagnosis of how the institutional apparatus of modern scientific inquiry serves to further entrench intellectual conservatism:

> In U.S. funding agencies there appears to be little reward for initiative.…it is safer to turn down requests (or to delay them by submitting them to superiors for approval) than to take a chance. Taking a risk by funding an innovative project can lead to trouble, and there are many projects that are risk-free and whose support can easily be defended…. Referees frequently expect all potential problems to be identified and their solutions outlined. Unfortunately, it is not an exaggeration to say that the agencies expect a proposal to outline the anticipated discoveries." (Muller 1980, pp. 881–883)

This last remark in particular seems almost incredible taken on its own, but is nonetheless a familiar feature of the scientific landscape for those who regularly submit research proposals to the NSF and similar institutions. As cancer researcher Susan Love writes,

> There is little chance, much less financing, for the wild idea that might prove revolutionary…. our academic and research institutions reward projects with clearly defined objectives that have a good chance of quickly leading to publications and tenure. (Love 2007, n.p.)

Moreover, even the *prospect* of such review seems likely to generate far more conservative research proposals, as the authors of such proposals simply invest their own time and energy as efficiently as possible while anticipating the likely responses of review boards or committees. As Travis and Collins note (1991, p. 336),

> If *proposers* come to accept that unorthodox projects are less likely to be funded, they will try to play down the novel aspects of their applications—or to change their research intentions [footnote omitted]….Indeed, a recent survey of NSF applicants reported that two-thirds of proposers agreed that 'NSF is not likely to fund high-risk exploratory research because the likelihood of obtaining favorable reviews is slim' (McCullough 1989, p. 83).

Compounding the problem, of course, is the fact that grants from extramural agencies are no longer obtained simply for the pursuit of research of particular interest to the state or even for particularly promising or exciting research. As Chubin and Hackett note above, universities and institutions have come to depend on federal support for scientific research, and obtaining such external support is now an essential step in the process by which the vast majority of work in contemporary academic science comes to be conducted in the first place.[3]

We might sensibly wonder, however, whether this arrangement really represents any striking departure from science as we have always known it: after all, Thomas Kuhn argued influentially long ago (1996 [1962]) that most science is 'normal' science seeking simply to make conservative incremental progress along the lines suggested by existing theoretical orthodoxy. But this comforting suggestion ignores a crucial respect in which the pursuit of scientific inquiry under the apparatus of peer-reviewed grant proposals for particular projects or programs of research truly was and is historically unprecedented. Although professionalization ensured that scientists could not afford to be indifferent to their peers' judgments of their scientific *accomplishments*, they nonetheless remained free to pursue those accomplishments in whatever way and by whatever means they chose. By contrast, the widespread adoption of the contemporary apparatus of peer-reviewed grant proposals for specific research projects in academic science ensures that today's scientists are only free to pursue particular lines of experimental investigation or theoretical development if they can first convince a panel of peers broadly steeped in existing theoretical orthodoxy that doing

[3] Of course scientists routinely use money from one grant to support an embryonic research idea that is not yet funded, and often scientists actually write grant "proposals" for the research they have currently underway and then use the secured resources to support work on their next project. But so long as the grants must keep coming in, a researcher cannot afford to use the resources from the last successful grant proposal to support a new project that does not *itself* have a high probability of ultimately being funded by an extramural agency, or she risks having no source of support for the next project after that one. And of course, even if it is barely possible for scientists to conduct research that challenges existing theoretical orthodoxy in the interstices of their main research programs, it matters immensely if such research must indeed be pushed into the interstices.

so represents a wise investment of scarce and limited resources. It might therefore seem that the distinctive incentive structure of contemporary scientific research could hardly be better constituted so as to ensure that inquiry in any given field will seek simply to make modest and secure incremental progress along the tracks laid down by our current theoretical perspective in that field, rather than seeking to develop or even identify fundamentally distinct and previously unconceived theoretical alternatives.

1.3 The rise of Big Science

Indeed, Kuhn's famous description of 'normal science' might now strike us as a more apt description of grant-driven research in physics following World War II than of scientific research throughout the history of the enterprise. But Kuhn himself also argued influentially that even in the course of such normal science the intellectual flexibility and freedom of younger scholars and those new to a given scientific field to propose and pursue alternatives to existing theoretical orthodoxy was the most crucial ingredient in the possibility of any truly fundamental or revolutionary change in our scientific beliefs. And in recent decades that very freedom has been systematically eroded further by the emergence and acceleration of what historians of science (following De Solla Price (1963), see also Galison and Hevly 1992) now call 'Big Science'. As biologist Aaron Hirsch describes this development,

> Across many different fields, new data are generated by a smaller and smaller number of bigger and bigger projects.…If the nineteenth century was an age of far-flung investigators alone in the wilderness or the book-lined study, the twenty first century is, so far, an age of scientists as administrators. Many of the best-known scientists of our day are men and women exceptionally talented in herding the resources—human and otherwise—required to plan, construct, and use big sophisticated facilities. (2009, n.p.)

Of course, such larger and more collaborative projects involve steadily increasing numbers of central players and institutions, while the degree to which any given proposal departs from existing theoretical orthodoxy remains limited by the perceived chances of rejection by a review panel or funding agency that the most risk-*averse* member of that collaboration will accept.

Far more importantly, however, the rise and ongoing expansion of "Big Science" has also introduced an increasingly stratified and hierarchical social organization into the pursuit of scientific work as well as scientific careers. As Hirsh goes on to note,

> There's something disturbingly hierarchical about the new architecture of the scientific community: what was before something like a network of small villages is today more like an urban high-rise, with big offices at the top and a lot of cubicles down below. (2009, n.p.)

Within such a hierarchy, the senior scientist in charge of a given lab or research group (these days often referred to simply as "the PI", a standard abbreviation for the Principal Investigator on extramural grant proposals) retains primary responsibility for bringing in the grants that keep the group afloat financially while a variety of more junior scientists including post-docs and students at various levels working under her direction and in collaboration with her conduct the details of the research for which the lab or group is known. Those more junior scientists seek to publish results coauthored by the PI and advancing the PI's own research program while simultaneously learning how to apply successfully for extramural funding of their own. Indeed, graduate education in the sciences now incorporates explicit instruction intended to teach graduate students how to choose projects and write grant proposals with the best chances of being funded by review panels at institutions like the NSF and NIH, and even those more advanced scientists who are still seeking permanent academic positions cannot afford to risk significant time and effort in theoretically iconoclastic or revolutionary proposals that do not promise reliable and predictable results in the short term. For all but the most senior scientists, then, both learning and practicing science today is a matter of working in close collaboration with a much more senior advisor or mentor to find, propose, and conduct research projects with the best chances of being accepted and funded by review committees made up of more established researchers in the same field. This hierarchical arrangement surely improves the resulting scientific work in a wide variety of ways and undoubtedly expands opportunities for valuable contact, training, and mentorship between more senior and more junior scholars, but it just as surely limits the freedom of younger or newer scientists to strike out on their own or to pursue theoretically unorthodox ideas that challenge the currently accepted theoretical conception of nature in any given field.

A recent examination of the proportions of primary research grants awarded to younger and newer researchers by the National Institutes of Health (NIH) offers striking empirical confirmation for this claim (see Fig. 1), noting that the median age at which a Ph.D. researcher first becomes a Principal Investigator on her own NIH research grant rose steadily from age 36 in 1980 to age 42 in 2002 (Committee on Bridges to Independence 2005, p. 39).

In addition,

> 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 % 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 % of NIH research awards made in 2002. (CBI 2005, p. 1)

Indeed, as these authors go on to point out, "the number of awards made to researchers age 35 and younger declined by over 75 % since 1980, even as the overall number of grants has increased" (see Fig. 2; CBI (2005, p. 15). The National Research Council committee reporting these results shares Kuhn's sense of the distinctive role of such younger and newer researchers in fostering creativity and innovation in scientific thought, and therefore regards these developments as profoundly troubling:

> Academic biomedical researchers are therefore spending long periods of time at the beginning of their careers unable to set their own research directions or establishing their independence….Moreover, there is a serious concern that new

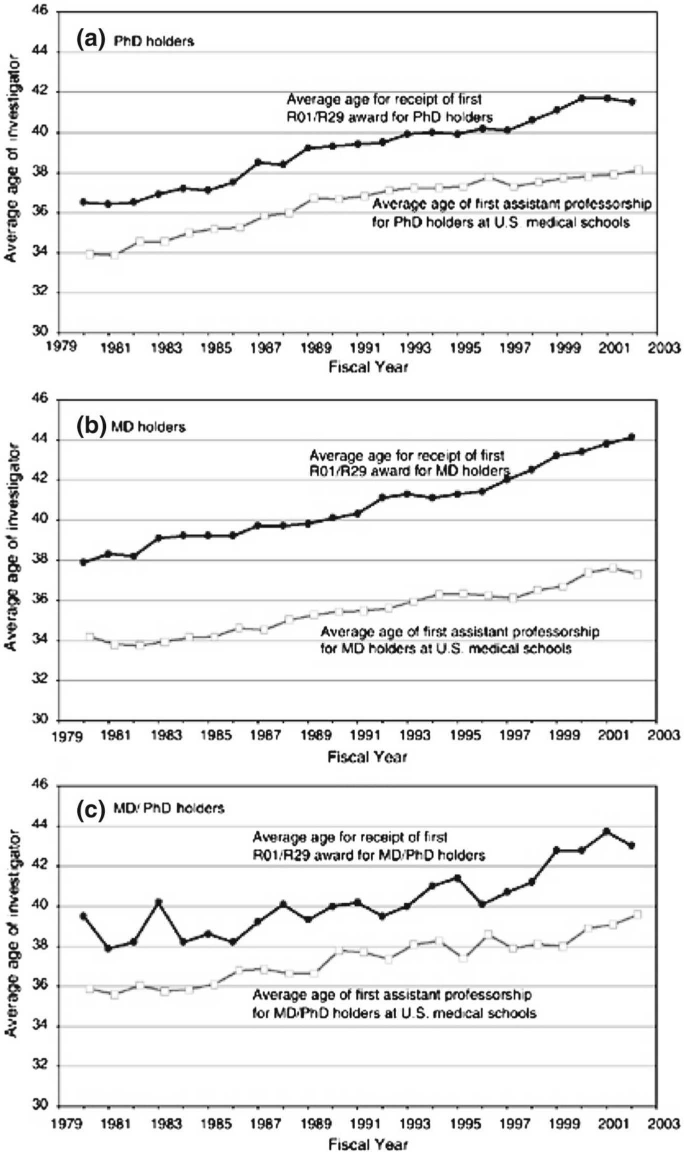

Fig. 1 Average age at time of first assistant professorship at U.S. medical schools and receipt of first R01/R29 award. **a** PhD holders. **b** MD holders. **c** MD/PhD holders. Reprinted with permission from (Committee on Bridges to Independence 2005, p. 39), Courtesy of the National Academies Press, Washington D.C.

investigators are being driven to pursue more conservative research projects instead of the high-risk, high-reward research that can significantly advance science. The special creativity that younger scientists may bring to their work is also lost as these investigators are forced to focus on others' research. (2005, pp. 1–2)

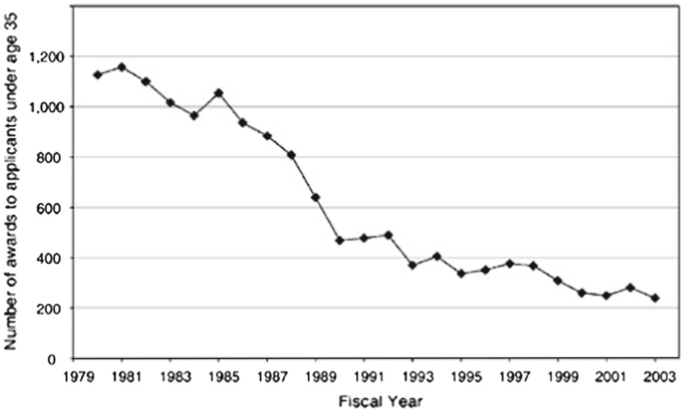

Fig. 2 Number of NIH research awards made to PIs 35 years of age and younger. Reprinted with permission from (Committee on Bridges to Independence 2005, p. 17), Courtesy of the National Academies Press, Washington D.C.

This might help explain why the Foundation for the National Institutes of Health's recently announced Lurie Prize intended for "a promising young scientist in biomedical research" (Foundation for the National Institutes of Health 2013) requires that nominees be no older than 52!

Senior scientists are not, of course, seeking to prevent more junior researchers from pursuing unorthodox or revolutionary theoretical ideas, but are instead simply teaching them how to conduct research and pursue scientific careers most effectively in the existing institutional environment. Indeed, as mentors and advisors to aspiring professional scientists, it is arguably irresponsible for them to do anything else. But such responsibility actually motivates further intellectual conservatism on the part of advisors and mentors themselves, as a PI who elects to pursue a genuinely revolutionary, transformative, or theoretically iconoclastic research program more likely to provoke skepticism from a granting agencies' program managers or review committees must now be willing to risk not only her own scientific fortunes but also those of the small army of less well-situated scientific workers whose careers presently depend upon her own. And of course these younger and less senior scientists the PI seeks to protect, who might otherwise pursue riskier and/or more theoretically iconoclastic projects, have a diminishing say in the direction of the lab's research as well as their own.

All this might seem simply to confirm dire prophecies made by an earlier generation of scientists regarding what Norbert Weiner once called this "latter day feudal system of the intellect" in which a younger scientist would simply be "a cog in a modern scientific factory, doing what I was told, accepting the problems given me by my superiors, and holding my own brain only *in commendam* as a medieval vassal held his fiefs". Weiner adds that "From the bottom of my heart I pity the present generation of scientists, many of whom, whether they wish it or not, are doomed by the 'spirit of the age' to be intellectual lackeys and clock punchers" (1956, pp. 359–360) and elsewhere laments "the degradation of the position of the scientist as an independent worker and thinker to that of morally irresponsible stooge in a science-factory" (1948, pp. 338–339). Lest this concern seem overwrought, note that a recent study (Mobley et al. 2013) found that fully 31.4 % of graduate students and post-docs at the University of Texas' M.D. Anderson Cancer Center reported that they had "felt pressure to prove the mentors' hypothesis, even when the data that the trainee generated did not support it", and 18.6 % reported that "they had been pressured to publish findings about which they had doubts".

Perhaps more revealing still is Albert Einstein's report to an American journalist in 1954:

> If I were a young man again and had to decide how to make a living, I would not try to become a scientist or scholar or teacher. I would rather choose to be a plumber or peddler, in the hope of finding that modest degree of independence still available under present circumstances. (Nathan and Norden 1960, p. 613)

The ongoing expansion of Big Science in the decades since Einstein made this remark has, of course, simply accelerated the evaporation of such intellectual independence. An especially dramatic illustration can be found in the system of academic authorship adopted in 1998 by the Collider Detector at Fermilab (CDF). All scientists and engineers at the CDF are added after one year of full-time work to its 'standard authors list', and they are removed from this list one year after leaving the institution. And any publication coming out of the CDF at any given time is automatically 'authored' by the entire current listing of standard authors who work at the institution, listed in alphabetical order (currently running between 400 and 500 members). Such an arrangement certainly rewards cooperation and teamwork, and duly recognizes the contributions of all those involved in a given piece of scientific research, but it seems hard to imagine a system of incentives better designed to favor theoretically conservative and incremental scientific contributions over theoretical iconoclasm, individual creativity, and intellectual independence.

2 Bush's nightmare

Thus, although realist critics of the problem of unconceived alternatives are surely right to suggest that contemporary scientific communities differ from their historical predecessors in a wide variety of important ways, I have argued that each of the most profound historical transformations of those communities has in fact rendered them less effective in conceiving, exploring, or developing fundamentally novel theoretical conceptions of nature. But it is admittedly hard to know how to trade off the impact of such changes against those emphasized by Godfrey-Smith and Forber, as well as others that might be important such as the increasing inclusiveness and diversity in the membership of those communities. By itself, simply recognizing the full range of systematic changes in our scientific communities over the course of their history might lead us to conclude only that our reasons for thinking that scientific communities have become progressively more vulnerable to the problem of unconceived alternatives over time are at least as compelling as any reasons we might have for thinking that they have become less so.

But of course we have evidential resources bearing on this question that do not simply document the most fundamental historical transformations of our scientific communities. In the course of describing those transformations I have also presented evidence of a widespread and growing conviction among the members of those very communities that the resulting institutional apparatus of contemporary scientific inquiry has erected severe and/or unprecedented obstacles to the pursuit of genuinely revolutionary, transformative, or unorthodox scientific theorizing. Moreover, in recent decades a wide and growing range of writers on science policy have either expressed mounting concern regarding what they see as the excessive and/or increasing intellectual and theoretical conservatism engendered by the contemporary institutional apparatus of peer-reviewed grant proposals for particular projects in academic science or reported such concerns to be widespread among scientists, reviewers, and administrators themselves (e.g., Roy 1985; Horrobin 1990; Chubin and Hackett 1990; Travis and Collins 1991; Wesseley 1998; Shatz 2004; Braben 2004; Luukkonen 2012; Lee et al. 2013). As Luukkonen reports in her review of this literature,

> the majority of the research on peer review concludes that it is inherently conservative and unable to select truly innovative research proposals [references omitted]. Braben (2004, p. 70) even goes so far as to maintain that 'the natural inclination to oppose major challenges to the status quo has become institutionalized' in peer review. (2012, p. 50)

Of course, any system for distributing scarce resources is bound to disappoint some hopes and expectations, and it might be unsurprising to find such disappointment expressed as a complaint about the conservatism of the system itself. For this reason, among the most striking evidence we have that the institutional apparatus of the modern scientific enterprise produces an excessively intellectually and theoretically conservative form of scientific inquiry consists in the concerns about such conservatism regularly expressed *by the very administrators who themselves direct and supervise that institutional apparatus.* In testimony before the House Subcommittee on Science, Research, and Technolology in 1979, for example, Carl Leopold, a plant physiologist at Cornell and an aide to Guyford Stever during the latter's tenure as director of NSF, noted that

> [NSF program directors are constrained to support] conservative proposals, and proposals which are 'sure bets' in that they are most liable to provide some definable product in a short period of time….[they are] under pressure not to take 'longer shots' on more imaginative or longer-term projects…. (Carter 1979, pp. 1064–1065).

More recent remarks made by Raynard Kington as acting director of the National Institutes of Health (NIH) suggest that the ensuing decades have witnessed little progress in this regard:

> We have a system that works over all pretty well, and is very good at ruling out bad things—we don't fund bad research. But given that, we also recognize that the system probably provides disincentives to funding really transformative research. (Kolata 2009, n.p.)

Likewise, Richard Klausner, former director of the National Cancer Institute, notes that although "Scientists don't like talking about it publicly" because they don't wish to be seen to be biting the hands that feed them,

> There is no conversation that I have ever had about the grant system that doesn't have an incredible sense of consensus that it is not working. That is a terrible wasted opportunity for the scientists, patients, the nation and the world….[Although important discoveries have been achieved in research that was funded by the N.I.H.], I actually believe that by and large it is despite, rather than because of, the review system. (Kolata 2009, n.p.)

In addition to evidence documenting the fundamental transformations of the scientific enterprise over the course of its history, then, we also find mounting concerns regarding the excessive and/or increasing intellectual conservatism of the resulting institutional apparatus of scientific inquiry expressed by scientists, reviewers, writers on science policy, and even the very administrators who oversee that institutional apparatus, as well as experimental and statistical forms of empirical evidence supporting such concerns. When these diverse forms of evidence are taken together, I suggest, they should lead us to conclude that contemporary scientific communities are in fact more vulnerable to the problem of unconceived alternatives than were their historical predecessors rather than less so.

3 Conclusion: what's next?

As we noted above, in recent decades the NSF and other granting agencies themselves have become increasingly concerned about evidence of the mounting intellectual and theoretical conservatism imposed by existing systems of incentives and review on the pursuit of scientific research. And in recent years, these agencies have responded to such concerns by redoubling their efforts to foster what the NSF calls "transformative research" dedicated to "revolutionizing entire disciplines; creating entirely new fields; or disrupting accepted theories and perspectives" (Bement 2007). Unfortunately, the NSF has largely sought to encourage such transformative research simply by exhorting reviewers and program managers to support it and instituting a requirement that both authors and reviewers of all research proposals comment explicitly on the 'potentially transformative' character of those proposals. But there is little reason to expect scientists or panels of scientists to be able to set aside deep-rooted biases in favor of theoretical orthodoxy when they gather to evaluate a set of specific proposals and (ultimately) decide which among them are simply 'the best' or most deserving of support (c.f. Mahoney, as well as Resch, Ernst, and Garrow, above). And this form of collective decision-making nonetheless still characterizes nearly everything that the NSF does. Even the NSF CAREER Awards, intended to support exceptionally promising young researchers, are awarded on the basis of a peer-reviewed competitive proposal for a specific program of research, and the same is true for similar programs intended to support especially groundbreaking and transformative research like the NIH's Pioneer Program, the ERC's Synergy grants, and the NSF's CREATIV program.

A recent study of peer review at the European Research Council (ERC) notes that even when institutions explicitly seek to encourage and support transformative or groundbreaking scientific research,

> the peer review process in some ways constrains the promotion of truly innovative research. These constraints arise from the very essence of peer review, namely, its basic function of judging the value of proposed research against current knowledge boundaries. (Lukkonen 2012, p. 58)

This analysis suggests not only that the existing institutional apparatus of scientific inquiry favors intellectual and theoretical conservatism, but also that we are unlikely to make truly substantial progress towards moderating that bias while continuing to distribute nearly all of the public resources available for the pursuit of scientific inquiry by means of peer-reviewed grant proposals for particular projects and programs of research.

The historical transformations of the scientific enterprise we have explored above, however, suggest that it might be more effective to instead diversify the *methods* we use to distribute resources for scientific inquiry. This might ultimately involve the invention of whole new forms of evaluation and distribution, but in the meantime we already have the models by which such resources were distributed in earlier historical eras. Nor are such alternatives entirely unknown in our own day: rather like aristocratic patronage, the Macarthur Foundation's so-called "genius" grants and the Howard Hughes Medical Institute famously support exceptionally creative and successful *scientists* rather than particular projects,[4] and scientific prizes are now offered for particular technological accomplishments by corporations like Nexflix and private nonprofit institutions like the X Prize Foundation. We should not, of course, simply *abandon* peer-reviewed grant proposals in favor of one or another of these alternatives, for *any* single way of distributing the available resources for scientific inquiry is virtually certain to have its own distinctive advantages and drawbacks. But we might nonetheless seek to moderate the intellectual and theoretical conservatism of contemporary scientific inquiry by diversifying our portfolio, so to speak, and expanding the proportion of scientific research, especially *publicly-funded* scientific research, that is supported in these as well as any promising novel alternative ways we can devise.

Of course, we must acknowledge that seeking to support more theoretically unorthodox, iconoclastic, revolutionary, or transformative science in this way would almost certainly incur substantial costs along with any potential benefits. Much of contemporary scientific research is enormously expensive, and much of the pressure for conservatism is created or reinforced by steady increases in the difficulty and cost of securing new data at the scientific frontiers using increasingly complex and expensive instruments and procedures, while institutions like peer review strive for accountability, meritocracy, and efficiency in distributing the resources available for scientific inquiry. If we fund more science that contemporary experts regard as risky, unpromising, or misguided, we will almost certainly wind up funding more science that goes nowhere and achieves nothing. The question, then, is not whether revolutionary or

[4] Indeed, the NIH is now considering whether or not to begin dedicating a substantial proportion of their resources to funding "people not projects" (Kaiser 2014).

transformative science is a worthy goal, but instead whether we expect to reap sufficient benefits from more aggressively pursuing it to outweigh the inevitable costs of doing so. Elsewhere I've argued that the answer we should give to this question depends in important ways on the position we take in the ongoing debate concerning scientific realism itself: after all, the need for "revolutionizing entire disciplines; creating entirely new fields; or disrupting accepted theories and perspectives" (Bement 2007) is considerably less pressing if scientific realists are right and their historicist opponents are wrong than if the reverse is true (Stanford 2015). But however that issue is ultimately decided, it can only benefit from a clearer view of how our own scientific communities compare to those of the past. Today's scientific communities are almost certainly more effective vehicles for *testing, evaluating, and applying* theoretical conceptions of various parts of the natural world than were their historical predecessors, but I have argued that we have compelling reasons to believe that they are actually less effective than those same predecessors in conceiving, exploring, or developing fundamentally *novel* theoretical conceptions of nature in the first place.

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