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Collaboration as a Window on What Science Has Become

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Abstract. Agnieszka Olechnicka et al. have nicely documented developments in the internationalization of science and collaboration which raise important broader questions. The processes they describe reflect the long-developing changes in the nature of science itself. The traditional view, elaborated by Michael Polanyi, was that the transmission of science at the level of discoverers required personal contact, which normally involved time spent in the laboratories of famous scientists, and hands-on experience with experiments and close interaction with colleagues, which in turn implied a few international centres. Has this changed through digitalization and the internet? One change is the increase in teamwork and the size and physical distribution of research teams, the outsourcing to the larger world of "open science," as well as novel forms of funding collaboration, which creates the need for new arrangements for patents and credit. The huge size of the grant system and other funding for science has been transformative, but has also re-oriented science from discovery and internal competition to "impact," which has inevitable effects on quality and the life-world orientation of scientists. Whether this has improved science is an open question: it has radically changed it.

Keywords: Michael Polanyi; conviviality; scientific collaboration; internationalization; science funding; open science

The brief closing of labs and limitations on collaboration resulting from the response to the coronavirus pandemic has generated new thinking and new practices in relation to scientific collaboration, including a greater reliance on the Internet for communication and expedited publication with less by way of peer review. Conferences have been cancelled or have turned into on-line events. Visas for research visits and student visas have been suspended. International collaboration with China has come under greater scrutiny, resulting, in the US, in the arrest and resignation of prominent researchers who failed to report income from foreign research work on grant applications and in other documents, or who failed to report this activity to their home institutions.

These events exposed something about the nature of collaboration and internationalization, but their significance is unclear. Agnieszka Olechnicka et al. (2019) help us make some sense of the current situation, but it also raises some deeper questions that one may fairly call "philosophical", because they relate to the image of science developed by philosophers in the twentieth century, and to profound questions about what "quality" in science might be. These are questions that the book did not set out to answer, and indeed sought largely to avoid. They are, however, also questions that lend significance to the book beyond its ostensible topic, i.e., the increasing role of collaboration and its internationalization. One may make two observations about what is revealed by the book, whether intentionally or not, as well as what is revealed by the recent pandemic crisis. The first is that they are an indication of the way the world of science has already changed a great deal in the direction of internationalization and openness, as well as pointing to some of the limits and inadequacies of the science it has produced. The second is a traditional point: science depends on personal contact, and great science is, as Michael Polanyi liked to say, an apostolic succession, a kind of transmission of powers of scientific intuition through personal contact, a laying-on of hands (Polanyi 1964, p. 44). It is for this reason that collaborating in the laboratories of renowned scientists was essential to the careers of scientists, especially those who returned "home" to establish a scientific career away from the centers of scientific power and money. This also happens on far more mundane levels. Harry Collins (1974) described the case of a scientific instrument, the TEA (Transversely Excited Atmospheric) laser, whose operation required special tacit skills, so that the laboratory that acquired it had to employ an operator from a laboratory that had already used it successfully. This aspect of collaboration, involving tacit knowledge, is central to the book and to the traditional image of science. We can elaborate on the familiar theme of tacit knowledge in various ways. Tacit knowledge implies the center-periphery relationship that is central to the book. Centers with advanced techniques and technology develop the tacit knowledge to use them first, and they are transmitted from person-to-person. Pilgrimages are made to the places in which such knowledge might be acquired. Those who acquire it are in demand elsewhere, and may convey this knowledge in person. This was Polanyi's core insight: scientific knowledge was personal knowledge (Polanyi [1958] 1972). It is also the core of the problems of collaboration and co-operation in science. One needs more than "knowledge" in the public sense of journal articles and findings. One needs people. The Wuhan case is revealing in many ways in relation to this, but Olechnicka, Płoszaj and Celińska-Janowicz focus on the larger phenomenon of collaboration, and defining it geographically is an excellent place to begin: people are in places. Digitization might appear to make this irrelevant: scientific information, as such initiatives as the Open Access movement seem to tell us, is highly fungible and may be made available digitally. The traditional view, at least Polanyi's, insists that in the end tacit knowledge, and with it personal contact and the transportation of knowledge in bodies, cannot be irrelevant. Was this idea a relic of the kind of science Polanyi had based his insights on, the science of the 1930s? Or does it persist even in the face of digitization and the changes in the nature of science since the rise of big science are moot points?

The coronavirus might initially seem to refute this traditional idea. The university in Wuhan which first isolated and had for some years been reporting on its origin in bats is in a remote region of China, far from the traditional centers of science. The construction of the laboratories and their subsequent development, however, confirm the traditional story. Not only did China embark on a colossal campaign to recruit foreign scientists and to send Chinese scientists to foreign universities, both as students and as researchers, but this particular university recruited the chairman of the Harvard Department of Chemistry and Chemical Biology, the nanoscientist. Charles Leiber, who was employed by Wuhan University of Technology (WUT) "not less than nine months a year", largely for purposes of collaboration: "declaring international cooperation projects, cultivating young teachers and Ph.D. students, organizing international conference[s], applying for patents and publishing articles in the name of 'WUT'" (US Department of Justice, 2010). This was part of the Thousand Talents Plan[1]. The plan itself seems to assume that bodies, real people in close personal contact, are essential in collaboration, and for the achievement of scientific excellence.

Collaboration new and old

The case spotlights, albeit perhaps misleadingly, the vast business of collaboration, and raises many questions for which the book is a fascinating road map. What the book does not say, except in asides, is what the business of collaboration actually comprises, what motivates it, and how it works. Although there are no simple answers to these questions, we may outline a few taxonomic differences, and shed light on a problem that lurks in the shadows: the changes in the nature of science itself since the watershed events of the Second World War, the atomic bomb, penicillin, and to the Salk vaccine, and the subsequent vast expansion of science, not only in universities, but also in governments and the private sector. One of the sources of our image of science, Polanyi, was a constant and committed collaborator. Eugene Wigner wrote that the laboratory he ran in Germany "formed a closely knit society with almost a family atmosphere" (Jha 2002, p. 14). He went on to write on the theme of "conviviality" as essential to the scientific community and vital to the process of discovery (Polanyi [1958] 1962, p. 203–212). Similar observations may be made about many of the other laboratories and research groups in

[1] Described as "one of the most prominent Chinese Talent recruit plans that are designed to attract, recruit, and cultivate high-level scientific talent in furtherance of China's scientific development, economic prosperity and national security. These talent programs seek to lure Chinese overseas talent and foreign experts to bring their knowledge and experience to China and reward individuals for stealing proprietary information" (US Department of Justice, 2010, https://www.justice.gov/nsd/information-about-department-justce-archives/). The information may be outdated.

this earlier era of science. They may be applied to some laboratories today, though one wonders how convivial the high-pressure competitive laboratories of today might be. One must also ask, however, whether this is the kind of collaboration involved in research that is discussed in the book. A good deal of research is nominally collaborative in response to grant initiatives encouraging international or interdisciplinary collaboration, both in the EU and the US. To be sure, these may promote scientific contact and provide the benefits of the division of labor, something far removed from a family atmosphere.

The differences are reflected in the organizational structure of these collaborations, and in the structure of the organizations the book describes. They are not tightly knit groups, in which, as Polanyi put it, "the tacit sharing of knowing underlies every act of articulate communication" (Polanyi [1958] 1962, p. 203), but they are, as the book suggests, "porous":

> They rely on the inflow of knowledge developed elsewhere: R&D produced by other firms or research organisations. At the same time, open innovation companies allow their internally produced knowledge to cross organisational boundaries. They spread new ideas in the form of spin-offs, spin-outs, licencing agreements, technology transfer, or even completely freely, without any direct benefit to themselves (Chesbrough 2003). Such an approach naturally extends collaboration, both among spatially dispersed individuals and organisations (Olechnicka et al. 2019, p. 109).

There is certainly a tacit element here, in which knowledge follows bodies and is incorporated in them, but one must ask whether the ready availability of information, which is the goal of the Open Access movement in science and such initiatives as the FOSTER Program in the EU, is the same phenomenon, and what it facilitates. Information, the favorite of educational psychologists and cognitive scientists, is not knowledge. The Chinese Thousand Talents plan and the relentless push to send students all over the world, following the Japanese example of a century and a half ago, shows that the need for personal contact in the acquisition of knowledge persists.

The passing on of tacit skills is inherently hierarchical in the sense that someone has them and others have to acquire them by subordinating themselves in a kind of apprenticeship. This relationship need not match an actual social hierarchy. A student may teach a professor how to perform a task, but for the most part it corresponds to a hierarchy, at least a hierarchy of age and experience, since those who had this knowledge were older. Scientists grow older as they progress through their careers, and tend to advance through the ranks. Collaboration based on the division of labor and its benefits tends, however, to produce only a modicum of equality. The main benefit is that collaboration with a division of labor allows things to be done that could not be done by an individual. It creates a relationship of mutual dependence. There are reasons to try to overcome hierarchical relationships: to flatten them to improve collaboration. This is one of the threads in the book.

> The collaborative turn increasingly affects members of the scientific community, no matter their position and experience. For centuries, the privilege of working with other researchers was reserved for the most prominent scholars (Beaver and Rosen 1978). In the last few decades, teamwork has not only intensified within the scientific elite, but has also become more widespread in the whole scientific community. Today, co-working with partners from all over the world has become an everyday reality for top scientists and for researchers in non-elite institutions, as well as scientists from countries that a few decades ago were absent from the global scientific collaboration network (cf. Schubert and Sooryamoorthy 2010, quoted after Olechnicka et al. 2019, p. 35).

This is an interesting prediction and an important aspiration. It will also be tested by recent events. This kind of collaboration, however, seems not to resemble the kind described, or experienced, by Polanyi, and other scientists trained in pre-war science. This statement suggests a tension between two models of collaboration, which may be considered as two end points of a continuum. The Polanyi end is that in which a laboratory, functioning almost as a family group, comes to share tacit knowledge and communicate in terms of it. The other end is a team-like division of labor in which people with different resources and skills are linked primarily by information. Brad Wray (2007), to whom we will return, combines the two: the cross-validation of assumptions and the division of labor. There may well be an element of each in all collaborations, but the new forms of collaboration discussed in the book are closer to the division of labor and information end of the continuum than the tacit knowledge end. This reflects larger changes in science itself.

The discussion thus points to a question: is the new kind of collaboration better, or better science, than the old kind, or is it simply oriented to different ends and suitable for different kinds of projects? The book clearly suggests that the new kind of collaboration is simply better. Brad Wray gives another reason for collaboration: collaboration helps in gaining acknowledgment and an acceptance of ideas, and may even be justified as epistemically superior:

> The results achieved by a group of collaborating researchers with different backgrounds, knowledge, and methodological approaches are supposed to have a more objective overtone. Collaboration allows for not only the cross-fertilization of ideas, but also for a cross-validation of assumptions, processes, and outcomes. In turn, the results of collaborative work are more likely to be acknowledged by the academic community. In other words, collaborative work enhances the epistemic validity of research outputs (Olechnicka et al. 2019, p. 121).

All of this is consistent with a reasonable extension of the older view of science, but there is an ambiguity in this line of argument, to which we will return: between "epistemic validity" and being "likely to be acknowledged".

From pure science to impact science

My concern in this commentary is the relationship between changes in the nature and objects of science and the problem of "quality". One of the most telling lines in the book, signals changes in the object of science, concerns the breakdown of an attempt to reach an agreement on collaboration between an American and a Chilean university. The American university demanded exclusive patent rights and the Chilean university refused. This would be shocking from the point of view of the old image of science: the gain in tacit knowledge, the gain in pure knowledge, and the validity of the results would be unaffected by such an agreement. The issue of patents was not new. Polanyi himself wrote about patent policy in the early 1940s, at the same time as he was developing his defence of pure science, and the commercialization of academic science was well-established in the mid-nineteenth century laboratory of Justus Leibig. The new centrality of this consideration is nevertheless a signal of something much deeper. If we read the rest of the book in the light of this deeper change, it also documents the change.

The change is reflected in the fact that Olechnicka, Płoszaj and Celińska-Janowicz basically run R&D departments alongside science laboratories. They note that traditional R&D activities were in-house projects to "individually develop, implement, and introduce innovations into the market" (Olechnicka et al. 2019, p. 109). The emerging order they describe is different: it is one in which development is not in-house and, therefore, limited in its collaboration with employees, but relies on open science. They describe new kinds of organizations responding to this order as "built on the assumption that lack of restrictions in developing someone else's ideas, combined with collaboration in non-hierarchical networks that can be widely distributed spatially, makes it possible to develop solutions that would otherwise be difficult to comprehend, or even not viable to accomplish" (Olechnicka et al. 2019, p. 109). This sounds substantially different from the old order, and seems to focus on the benefits of an extended division of labor. It also treats the transfer of tacit knowledge, albeit not necessarily the knowledge itself, as less important than in the past, and implies that the older structures were a constraint that has been eliminated. The image is no longer of a team with an internal division of labor, but of a network, in which the division of labor has been externalized to the network itself.

The shift in terms from the traditional notion of science to "development" and related research is justifiable and reflects a change in science itself. This requires a little history. Daryl Chubin and I have recently commented on this shift by making the following observation: that science in the first half of the twentieth century and a little beyond was motivated by what we called an ethic of discovery (Turner and Chubin 2020). Jonas Salk's development of a polio vaccine was a classic example. He asked for no financial stake in it and was lionized for this in the press, as well as within the scientific community itself. A generation earlier, Elmer McCollum did the same with vitamins with the same result[2]. The world changed with the rise of big science, a vast public grant system, and the post-war connection between science and defence, a world often described as Cold War science[3]. In this period, the rationale for science changed: while fundamental discovery was still important, it was no longer considered full validation. Science came to be justified for its pay-offs, or impact. This was important because the new justification enabled more funding for science. The initial justification for the vast expansion of funding for science was instrumental, based on the idea of a deep link between what had been known as "pure" science, also known as "basic science", and technological and practical benefits.

The growth years of the 1950s and 60s gave rise to an overproduction of scientists and the crisis of funding of the 1970s, when growth in the number of scientists seeking funding outstripped the growth in funding. Although this was primarily an American phenomenon, internationalization had already gone sufficiently far for its effects on international science to be severe. Internationalization and other changes that reflected the rapid rise of scientifically relevant enterprises, notably what we now refer to as Big Pharma, were, however, already changing the face of science in complex ways (Tobbell 2009). By the late 1980s, the rhetoric of the US National Science Foundation had changed: its goals now explicitly economic. Even its resources were tiny, however, compared to the US National Institutes of Health. The Dole-Bayh act of 1980 allowed, indeed encouraged, universities to make money on patents based on federally funded research. Technology transfer offices sprouted, along with university journalist promoters of discoveries that could be turned into products. This had major consequences for medical schools and science departments. Big Pharma and university science collaborated in massive projects beyond the capacity of universities alone. A new, large class of university scientists, liberated from teaching duties, also emerged to work in laborato-

[2] Instances of McCollum's selflessness and financial generosity to the institutions that supported him are recorded in his biography in the National Academy of Sciences; http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/mccollum-elmer.pdf.

[3] As Luis Alvarez's biographer put it: "the war changed everything forever for physicists. The nature, scale, and recognition of the value of our science prompted a revised golden rule: 'why use lead when gold will do?' The essentially solitary pre-war way of doing physics gave way to that pioneered at Lawrence's lab: collaborative, industrialized-scale activity, which the grateful nation underwrote" (Trower 2009, p. 9); http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/alvarez-luis-w.pdf.

ries designed to generate new products. There could have been no better indication of internationalization than the fact that almost every major scientific producer soon followed with similar legislation, with similar effects (AUTM: Bayh-Dole Act, https://autm.net/about-tech-transfer/advocacy/legislation/bayh-dole-act)[4].

By the turn of this century, another change had become apparent. Investment in STEM (Science, Technology, Engineering, and Mathematics) had become a national strategy of many countries' education systems, and indeed a priority that was ousting traditional subjects even at secondary and primary levels. STEM education was presented as a requirement of national economies, and the guarantor of future prosperity. Ph.D. students wound up in non-academic positions, in private or bureaucratic R and D, and unsurprisingly grew increasingly to prefer them (Fox and Stephan 2001), as they were more lucrative, provided more security, and freed them from the expectations and demands of academic positions, as well as the escalating bureaucratic requirements imposed on researchers. Traditional scholar-teacher positions in research universities became scarcer, and a new division opened up between scientists hired as pure researchers for grants, and lecturers, whose sole responsibility was to teach. The hyper-competitive grant system, especially brutal in the US, and the attendant burden of peer review and scrutiny became the defining reality for those few young researchers filling academic positions in research universities. They were given generous start-up money, but were expected to repay it. The system was designed to make this possible: fixed investments and costs of the university were repaid through a system in which the university was paid a large additional percentage of the "direct costs" of the grant, so that the costs of any investment made by the university could be recovered, and new investments could be made with the proceeds. The amount of money granted became a measure of success; yet, at the same time, the universities made financial losses on science, leaving science to acquire money from other programs (Holbrook and Sanberg 2013). In Europe, where grant money was more accessible, the demands also increased. Even junior applicants for faculty positions were expected to demonstrate that they had generated grant money and supported others.

Priorities reversed. Rather than seeking money to support a laboratory to carry out a long-term project of discovery, the priority of a scientist in charge of a lab-

[4] The list of members of lobby groups in support of the act and its protection includes the Association of University Research Parks, AUTM, BIOCOM, BioHealth Innovation, Biotechnology Innovation Organization, Columbia Technology Ventures (CTV), Council on Competitiveness, Council on Governmental Relations, Fuentek, Information Technology and Innovation Foundation, IPWatchdog, Lehigh University Office of Economic Engagement, Licensing Executives Society (LES), Licensing Executives Society (LES) Silicon Valley Chapter, National Venture Capital Association, Pharmaceutical Research and Manufacturers of America, Pristine Surgical, STC.UNM, the IDEA Center at the University of Notre Dame, Wisconsin Alumni Research Foundation, and the Yale Office of Cooperative Research (www.bayhdole40.org). Suffice to say that this is also an indication of the far more extensive network of interests and organizations that make up the world of "science" in the US, and in the world at large.

oratory was to acquire funding to maintain the laboratory. Systems of evaluation also inverted their priorities. Rather than making a discovery and later having its importance reaffirmed by citations, citations became the measure of quality, and an end in themselves for researchers. Science advanced not through discoveries and solving problems incidentally, but rather oriented itself to solving problems directly. What had begun in the 1930s as an effort to increase the demand for science from a skeptical public became a regime of what I described as "science on demand": science, or rather the use of scientific techniques, tailored to producing results for public and private needs (Turner 2020). Productivity replaced discovery, and quantitative methods for grading productivity replaced a sense of the personal quality of the scientist. The constraints of the hyper-competitive grant system and the relentless demands of peer reviewing drove top scientists out of academia in favor of institutions with greater resources and more freedom. Scientists who had already become impact-oriented freed themselves from the constraints of academia. Young scientists who had never tasted autonomy and were never likely to experience it had no regrets about taking a non-academic route and selling their skills where they were valued.

The New Order

The images we have of science from philosophers of science are static. They mostly comprise the experiences of the likes of Polanyi and Karl Popper and taken from the 1930s. The new world of science is, however, the product of a more or less conscious policy, unlike the academic world of science of the past, which built on the institutional traditions of universities where it had played a minor, mostly subordinate role. That world prized the autonomy of professors, an autonomy that grew into an even greater autonomy granted to the research institutes that sprung up to free researchers from the constraints of universities, such as the Kaiser-Wilhelm-Gesellschaft laboratories, including a part run by Polanyi, Cold Spring Harbor, the Koch and Pasteur Institutes, and the Rockefeller Institute, which in the 1950s became the Rockefeller University. These places provided homes for researchers and supported long-term projects. Barbara McClintock, the Nobel laureate who flourished at Cold Spring Harbor, "insisted that she would never have become a scientist in today's world of grants because she could not have committed herself to a written research plan. It was the unexpected that fascinated her, and she was always ready to pursue an observation that didn't fit" (Federoff 1995, p. 222).

These careers also depended on personal sponsors with the power to support their research rather than the complicated processes of peer-review governed by concerns over citations and the ability to attract grant money. The old system doubtless led to injustices. It was not for nothing that Max Weber said that academic life was a mad hazard, and that to be a professor one must accept that incompetents would be promoted ahead of you (Weber 1946, p. 134). It did, however, allow lines of research to venture outside the conventional lines, the significance of which was sometimes not recognized for decades. This would be impossible today: committees and peer review have replaced personal authority for this kind of decision. Whether the filters have changed in response to the new realities of a peer-review-driven, grant-oriented, academic world, and in response to the much larger non-academic world of science, which influences the way academic science is done – its standards, and its aims. This is a possibility implicit in the text, in the comment on Wray. It notes that "Wray argues that the power of collaborative research lies in its ability to justify scientific discoveries by the scholarly community", and interprets this as meaning that it is *better* research as a result of "not only the cross-fertilization of ideas, but also for a cross-validation of assumptions, processes, and outcomes" (Olechnicka et al. 2019, p. 121). There is, however, a different, or additional interpretation that suggests itself: that collaboration is a method of publicity that is effective in energizing a larger and more diverse network of influencers. This is certainly the case.

The same ambiguity recurs in a comment they make on the persistence of center-periphery relations.

> On the one hand, the localization of research centers forms a playing field for scientific flows: after all, links do not exist without nodes. On the other hand, flows in the form of scholarly collaboration constitute a significant factor for the progress and impact of scientific places. In a certain sense, "these places are not meaningful in themselves but only as nodes of these networks" (Castells and Ince 2003, p. 57; quoted after Olechnicka et al. 2019, p. 13).

Whether the places are meaningful in themselves, i.e., as special sources of "progress and impact", thus quality or whether they are meaningful only as "nodes", i.e., as advantageous locations in the middle of rich networks whose activation enables wider acceptance, is a moot point.

The book neglects to address the complex relation between quality and recognition, especially in the form of peer approval and citations, the usual two measures. This tends to equate citations and quality. The question of their relation, however, is significant, and it goes to the heart of the question of whether the new order of science has undermined traditional scientific values and misdirected scientific progress. The issues come into focus in relation to the pervasive nature of peer review. Peer review depends on the assumption of independent and autonomous judgment by equals, peers. The very phenomenon of peer review, however, creates a novel kind of dependence on the opinions of others; not merely an epistemic dependence, but a political dependence. To be supported one must achieve the support of peers in the form of formal acts, e.g., votes and ratings of proposals. Authentic autonomy is impossible as long as support, the crucial condition of current science, depends on these opinions. Consciously or unconsciously, or through the filtering effect of grant decisions and other evaluations, one is subservient rather than "autonomous".

Citations raise related issues. They are normally assessed in the short term, but as some of the bibliometric research has shown, citation rates in the short term are a measure of currency, or prevalence, rather than the ultimate impact, which is better measured by long term citations. There are also many other issues related to the question of what aspect of quality is measured by citations (Leydesdorff et al. 2016; Aksnes, Langfeldt and Wouters 2019). The pursuit of citations in any case has a similar effect as the use of peer review insofar as it undermines the assumption that citations are unbiased and uninfluenced by the aim of producing citations. The phenomenon of citation farming, and the *de facto* transformation of groups into mutual citation societies in which the right citations are a condition of publication, transform the measure into an instrument of strategy (Muller 2018). Gloria Origgi has aptly described our acceptance of this system as "voluntary epistemic servitude" (Origgi 2017, p. 206–240). The incessant demand for evaluation inevitably has hidden psychic effects on the choices of what to research, what to publish, and ultimately on how to think. These may be difficult to quantify or study, and we are inevitably limited in what we may know about what science would develop under a different regime, but it would be unrealistic to imagine that there are no consequences.

Powerful organizational forces have changed science from a small, highly personal, enterprise with its spiritual heart in pure science to a major part of economic life, closely integrated with a complex regulatory system, highly organized and dependent on decision-making systems disciplined by intense competition, and influenced by the large private sector of innovation and development. We cannot stand outside this system and judge it, since our sense of what science is and might be is based on what science is now. We may, however, reflect on the history of science, and ask what the costs in terms of the scientific opportunities of the new order of science might be, and also ask how our understanding of science should be revised in order to grasp this new order. The changes in science that are discussed in the book, i.e., the changes in the geography of collaboration, raise the large and probably unanswerable questions: whether the present state of science is the natural and inevitable outcome of the progress of knowledge and the development of techniques and whether the changes in patterns of collaboration are associated with improvements in science, or a result of this larger change; whether we have reached the limits of a certain kind of explanatory science, so that these changes merely reflect the state of scientific knowledge; and whether the changes in the organization of science themselves channel science in ways that limit its capacity to explore.

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