Peer review

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Peer review serves a gatekeeping function both within and outside of academe. *Sub specie academicus*, academic excellence is validated by the process of peer review. Academic excellence, however, is often inversely proportional to societal relevance. Interdisciplinary research is increasingly encouraged as a way of making academic research more societally relevant. *Sub specie societatis*, academic research is also called upon to help societal decision makers craft evidence-based policies, and peer review is the preferred tool for ensuring the integrity and reliability of the research used by decision makers.

These trends toward interdisciplinarity and transdisciplinarity for research strain the process of peer review. The key issue for advocates of peer review is whether a tool that has been used mainly to determine academic excellence can be adapted to judge societal relevance without undermining the foundations of knowledge production (Sarewitz 2000).

1. Background: the view from inside academe

Peer review is a process by which a group of individuals renders judgment on the work of others in order to determine whether that work is meritorious enough to warrant consideration (*e.g.*, for publication or tenure) or support (*e.g.*, in the form of a grant or fellowship). Typically, the individuals asked to render such judgments are selected from a pool of reviewers who are considered to be 'peers' of whoever has produced the work to be judged. What constitutes a peer is more complicated than one might think; but given the uses to which the process of peer review has been commonly put, a peer has traditionally been characterized in terms of shared disciplinary expertise.

The *a priori* justification for using peer review as an assessment tool is relatively straightforward: no one is in a better position to assess the merit of work in a particular area than experts in that particular area. Thus, in order to judge whether work in area P is meritorious, it makes sense to ask individuals renowned for their expertise in area P rather than people who know comparatively little or nothing about P. Although individual non-conformists exist, along with several quasi-disciplines, which may or may not be evolving toward disciplinary status, areas of academic expertise are most often carved out by and within academic disciplines. Indeed, the connection between academic excellence and disciplinary expertise is so common that interdisciplinarity among academics is often perceived as amateurism (*cf.* Frodeman and Mitcham 2007).

Despite the fact that the standards of one academic discipline are incommensurable with those of other disciplines, relying as they do on expert (and often tacit) knowledge within the field, there is universal agreement across academe that peer review is essential for determining what counts as academic excellence. Indeed, publications that are not peer-reviewed typically do not count – either at all or as much as – peer-reviewed articles when it comes to tenure and promotion standards for higher education faculty; and the majority of grants from public funding agencies are allocated only after and on the basis of some form of peer review. For this reason, the process of peer review is usually characterized in terms of ‘quality control’ or as having a ‘gatekeeper’ function, and it is no exaggeration to say that peer review is the *sine qua non* of academic excellence.

The most common uses of peer review are in academic publishing (*e.g*., to determine whether a paper submitted for publication in an academic journal is worthy of being published in that journal) and in the review of proposals for grants (*e.g*., to determine whether the proposed activities deserve to receive funding). Both prepublication peer review and grant proposal peer review are *prospective* uses of peer review, which puts a great deal of pressure on reviewers to predict the future: will this paper (or this proposed research) ultimately be well-received by the field? In most, though not all, cases of prospective peer review, the identity of the reviewers is withheld from the reviewee (a process known as blind peer review); and in many cases of prospective peer review, the identity of the reviewee is also withheld from the reviewers (a process known as double-blind peer review).

The process of peer review is also increasingly employed to conduct *retrospective* analyses of particular people, practices, or institutions. Thus, for instance, peer review may be employed within an academic department to rank the performance of individual members of the department relative to other members of the department. Often, 'external' reviewers are brought in to assess the strengths and weaknesses of the business practices of a particular company or to identify strengths and weaknesses on an institutional level, judging a university, a particular program within a research funding agency, or the agency as a whole. Usually, cases of retrospective peer review make fewer, if any, attempts to hide the identity of reviewers and reviewees from one another through blinding. Because of dissimilarities with the typical peer review process, which relies heavily on the use of disciplinary peers as reviewers, many are reluctant to call retrospective institutional review peer review at all, preferring instead to refer to this practice as *expert* review. There also exist other 'extensions' of the peer review process, i.e., atypical uses of peer review, such as the use of peer review in relation to regulatory decision making (Jasanoff 1990).

Typical criticisms of the process of peer review include the worry that it may be potentially biased against people for reasons unrelated to the merit of their work (Wennerås and Wold 1997). Blinding reviewers and reviewees to the identity of the other is an attempt to allay this criticism. Some critics suggest that peer review is inefficient and unwieldy as a tool for evaluating large volumes of research. In response, some funding agencies have taken the step of limiting grant proposal submissions, *e.g.*, by shortening the allowable length of proposals, by previewing letters of intent and accepting only invited full proposals, limiting the number of proposals particular institutions may submit for particular calls, or limiting the number of submissions a particular researcher may make of the same proposal.

Another common criticism of peer review is that it is inherently conservative, tending to favor work conducted along traditional lines (in the sciences this concern is often expressed in terms of bias toward existing paradigms and against novel, transformative, or revolutionary ways of thinking). To counter conservatism, reviewers are sometimes instructed to value paradigm-shifting or 'transformative' ideas. Another tactic that funding agencies use to counter conservatism is to put out calls for interdisciplinary research proposals. Reviewing interdisciplinary proposals, however, presents special difficulties (Lamont 2009; Huutoniemi, this volume).

One of the most notorious criticisms of peer review is that it is ineffective at determining quality and/or detecting errors (*e.g*., the so-called Sokal Affair or the widely publicized failure of reviewers to detect the falsification of data by Hwang Woo-Suk in publications on stem cell research in 2004 and 2005 in the journal *Science*). The typical response to this criticism is to deflect it with humor: Winston Churchill's quip about the value of democracy is paraphrased, and peer review is admitted to be the worst form of research evaluation, except for all the others. In this way, advocates of peer review effectively divert the conversation back to considerations that do not threaten the very existence of peer review: how to improve its efficiency, reliability, responsiveness, and fairness (and hence its overall effectiveness).

2. A history of peer review

It is a commonly held belief that the process of peer review is venerable because it is ancient, as opposed to merely respectable because it is institutionally well-entrenched. Searching for "the first documented description of a peer review process," the *2007-2008 Peer Review Self Study* published by the National Institutes of Health (NIH) cites two articles published in a 1997 issue of the *Annals of Saudi Medicine* that note a peer review process described "more than a thousand years ago in the book *Ethics of the Physician*, authored by Syrian physician Ishaq bin Ali al-Rahwi (CE 854-931)" (NIH: 8). *Ethics of the Physician* "outlines a process whereby a local medical council reviewed and analyzed a physician's notes on patient care, to assess adherence to required standards of medical care" (NIH: 8). This description seems most reminiscent of medical peer review, which is a quasi-judicial, retrospective fact-finding procedure to determine whether (as with a grand jury) a hearing is necessary. Of course, according to a sufficiently broad definition of peer review, one might also cite the Athenian judicial system: Socrates' trial (as documented in Plato's *Apology*) might be seen as a kind of peer review process, and whose practice of confronting and examining his 'peers' in the *agora* (as documented throughout Plato's early dialogues) could also count.

Most histories of peer review trace the origin of prepublication peer review to the Royal Society of London and its journal *Philosophical Transactions*, founded by the Royal Society's first Joint Secretary, Henry Oldenburg, in 1665. Although no one questions whether Oldenburg deserves credit as the founder of the world's longest-running scientific journal, whether his practice of passing manuscripts around to members of the Royal Society prior to publishing them in the *Philosophical Transactions* actually constitutes the 'real' origin of the prepublication peer review process is the matter of some debate (Kronick 1990, Spier 2002, Royal Society 2009). Regardless of its 'real' origin, Spier (2002) notes that both the practice of prepublication peer review and the time of its adoption vary from journal to journal, and that the practice did not become widespread until after the Xerox photocopier became commercially available in 1959.

Scarpa (2009) dates the very first (*ad hoc*) peer review of grant proposals to 1879, and Germany's *Notgemeinschaft der Deutschen Wissenschaft*, predecessor of the *Deutsche Forschungsgemeinschaft* (DFG), had a review system during the 1920s, which was later adopted by the DFG in 1951. But the robust institutionalization of grant proposal peer review began around the middle of the twentieth century with the passage of the Public Health Service Act of 1944 in the US, which authorized the NIH to make grants, an extension of the power that in 1938 had been limited to the National Cancer Institute. NIH quickly established a Division of Research Grants to oversee NIH's peer review process. In the late 1940s, the US Office of Naval Research (ONR) also began making grants, although no process of peer review was required. Instead, grants officers sometimes asked experts to review proposals in order to help them make their decisions. In 1950, the US National Science Foundation (NSF) was founded, and NSF adopted a process of grant allocation that not only copied the strong program manager model from ONR, but that also incorporated a process of peer review like NIH. NSF's peer review process remains to this day less standardized than that of NIH, but more standardized than that of ONR.

Two salient features regarding peer review stand out from the foregoing historical account: (1) peer review is not as ancient a practice as many assume – it was not widely practiced in either publication or grantmaking until after the middle of the twentieth century; and (2) in both prepublication peer review and grant proposal peer review, practices vary widely. Nevertheless, despite some criticisms of the process, members of the academic community are almost unanimous in their support of the peer review as a decision-making tool, both for publication and for grantmaking purposes (Boden Report 2006). This near unanimity of support cannot stem from the fact that peer review is the way things have always been decided in academe, for that simply is not the case.

3. Autonomy and expertise: the disciplining of peer review

In part, the institutionalization of peer review is motivated by the growth of academic disciplines, both in terms of the *fact* of their growth (*i.e.*, the fact that academic disciplines became, in the nineteenth century, the new model for how research was to be conducted within the German and American research universities) and in terms of the *need* for growing particular disciplines (a need generated by the invention of this new model of the university). Along with the disciplinary division of labor advocated by Kant at the end of the eighteenth century, this new model for the university incorporated a strong demand for autonomy. Wilhelm von Humboldt's "On the Spirit and Organizational Framework of Intellectual Institutions in Berlin" proclaims: "The state must always remain conscious of the fact that it never has and in principle never can, by its own action, bring about the fruitfulness of intellectual activity. It must indeed be aware that it can only have a prejudicial influence if it intervenes. The state must understand that intellectual work will go on infinitely better if it does not intrude" (244). According to Humboldt's vision, the state's only role should be to facilitate the conditions necessary for the greatest production of knowledge (for the sake of knowledge, rather than for the sake of the state) – to serve an instituting, but not an institutional role *vis-à-vis* the university. Humboldt's justification for the state's playing this facilitating role is that the state will ultimately benefit from supporting the unfettered pursuit of knowledge in the university.

Incorporating both a division of labor and a strong sense of autonomy, the new universities produced both more knowledge and more specialized knowledge, thus simultaneously cultivating depth (as defined by particular disciplines) as the mark of excellent research and reinforcing the divisions between disciplines. Just as the desire to form the 'new science' led to the formation of the Royal Society of London and to Oldenburg's establishment of the *Philosophical Transactions*, the desire to form new disciplines led to the establishment of new, disciplinary journals. As disciplines grew, they produced both more and more specialized knowledge, which spawned both more and more specialized journals. Competition for resources between universities, between different disciplines within universities, and between faculty members within departments eventually led to the 'publish or perish' mentality, as well as to increasingly sophisticated ways of judging whether one journal were better than another, ranging from the relative prestige of the editors or the academic home of the journal to circulation and impact factors. The most widely used – and crudest – measure of the worth of any particular journal, however, is whether that journal is peer-reviewed. This is true despite the fact that the peer review process across journals varies widely. The case is much the same for the outputs of research, *i.e.*, publications. Indeed, that a particular line of research does not appear in the peer-reviewed literature is taken as *prima facie* evidence of its lack of quality (*e.g.*, the case of Intelligent Design Theory); and publication in peer-reviewed journals is the coin of the realm of many disciplines, largely determining the outcome of many tenure and promotion cases. The close link between peer review and disciplines also presents problems for those who are seeking to explore interdisciplinarity in their own scholarship (Graybill, this volume).

There is a remarkable unity of themes between Kant's call for the division of labor in research, Humboldt's plea for facilitated autonomy for the university, and the canonical document of post World War II science funding policy in the US, Vannevar Bush's *Science – the Endless Frontier* (1945). Echoing both Kant and Humboldt, Bush argues for state support of autonomously pursued basic research, that is, research pursued for its own sake, without concern for the practical ends that are the proper province of applied research. According to the Bush conception, applied research, which yields technological, medical, and military advancements, fundamentally depends on basic research. Just as Humboldt had argued at the turn of the nineteenth century, Bush suggests that although the particular uses of basic research and the eventual benefits that will accrue are difficult to predict, societal benefits cannot occur unless scientists are allowed to pursue science without interference from the state – a notion that was later labeled as the linear model (or sometimes, linear-reservoir model) of science.

Because Bush was asking for large outlays of public funds, and on a continuing basis, in support of the unfettered pursuit of basic scientific research, some form of accountability needed to be built into the system. Indeed, there was a great deal of debate between the strong-autonomy advocates in the Bush camp and the more pragmatic adherents of the views expressed in the Steelman Report (1947), which advocated more limited scientific autonomy in the name of a stronger connection to public benefit. Bush's advocacy of a strong form of autonomy ultimately won the day when NSF was created in 1950. Arguably, however, one reason NSF abandoned the ONR model for grants decision making, in which a program officer can make funding decisions without subjecting proposals to peer review at all, was the controversy over the demands for the autonomy of research and the demands for more closely linking research to societal benefits. Peer review of grant proposals is meant to guarantee that scientists have a large degree of autonomy when it comes to making decisions about which particular research proposals ought to receive funding, while simultaneously demonstrating their accountability for making wise use of public funds.

The success of the process of peer review in guaranteeing autonomy for the academic pursuit of knowledge, along with concomitant financial support in the form of public funding for research, are key drivers of academe's love affair with peer review. But the fact that society allows peer review to serve this dual function – providing autonomy and asking only self-regulation as accountability – perhaps needs some explanation, given society's ambivalence, or what Jasanoff (1990) terms "oscillation between deference and skepticism," toward experts (9). Even as we profess our distrust of experts, we evidence faith in expertise. In part, this faith can be attributed to what Chubin and Hackett (1990) call "enclaves of expertise" in the face of which "we usually delegate to experts the authority for making decisions in areas we do not understand" (4). We routinely follow the advice of doctors when it comes to our health and of mechanics when it comes to our cars. Indeed, we ignore the advice of experts at our own risk. It is also the case that what constitutes an autonomous academic discipline, at least in part, is there being something it is, some field of knowledge, which is its special task to pursue. Academic journals mark out this disciplinary territory, and prepublication peer review ensures that this territory is marked well (i.e., according to the standards of the discipline). Academics are

Comment: Check out this piece as it relates to the lit on expert opionion experts, and even within academe, perhaps especially so in the context of peer review, scholars from different disciplines display a remarkable deference to the expertise of scholars from other disciplines (Lamont 2009). The experts trust the other experts; is it really any wonder, then, that non-academics should have some faith in peer review?

There is also a growing political problem for anyone who would question society's faith in peer review, as much of the current rhetoric surrounding global climate change attests: so-called climate deniers are routinely characterized as having ulterior motives (something other than truth, such as greed), and decisionmakers who question scientific consensus – which was gained only after a thorough trial by peer review – run the risk of being charged with the politicization of science (Mooney 2005). Although Sarewitz (2009) is correct that the Obama administration's attempt to "restore science to its rightful place" in US policy-making – in contrast to the presumably wrongful place science occupied in the Bush administration – is yet another politicization of science, the political appeal of Obama's strategy rests on a more basic faith in the value of knowledge and a philosophical presumption about what knowledge actually is.

Academics and non-academics tend to share the presumption that knowledge is something that comes along with specialization and the depth that such specialization brings – what Fodeman (2004) critiques as an epistemology of external relations and opposes to a kind of epistemological holism. An epistemology of external relations – or epistemological reductionism – tends to support analysis: knowledge is gained by examining parts of reality, which can later be pieced together (somehow – reductionism tends not to spend too much time on how this might happen). Epistemological holism, however, holds that knowledge of the whole is always greater than the sum of knowledge of its parts. Epistemological reductionism tends to support the idea of expertise, whereas an epistemological holism tends to undermine the idea of expertise (Sarewitz, this volume). Epistemological reductionists tend also to think that more knowledge is always a good thing, whereas epistemological holists tend to believe in limits to knowledge. Discipline-based peer review is essentially founded upon an epistemology of external relations, and part of the explanation for our overall acceptance of the process of peer review is that we tend – whether we realize it or not – to view knowledge in (reductionist) terms of external relations. Because we tend to view knowledge in reductionist terms, the notion of expertise seems intuitively obvious to us. (Note that, although this last point is a holistic claim, there is no necessary incompatibility between holism and reductionism. The seeming opposition between the two ways of viewing knowledge simply reveals our own reductionist tendencies.)

Another factor supporting our faith in peer review is that we tend to ignore the fact that peer review has a history – and it has a far shorter one than many presume. Adhering to the process of peer review is not simply a disinterested matter of scholarly housekeeping on the part of academe or objectivity on the part of grantmaking institutions or societal decisonmakers. Rather, the process of peer review has its roots in the institutional disciplinization of knowledge production, a process that has always been as political as it has been epistemological. Within the university setting, disciplines deserve at least as much identification with power as knowledge does: in its role as the valuator of academic and scholarly work, the process of peer review acts to wall off disciplines from each other, guaranteeing the existence of disciplinary islands where petty princes (or tyrants) rule. In its role as guarantor of autonomy from societal influence, peer review also walls off academe from the rest of society, guaranteeing autonomy at the price of isolation. Discipline-based peer review *is* the gatekeeper – not only of the little disciplinary hearths within academe, but also of the Ivory Tower itself.

Comment: Key point

4. Interdisciplinary and transdisciplinary pressures on peer review

Academic excellence is one thing; relevance to anything in the real world outside academe, however, is something altogether different. Often, academic rigor – and relevance within disciplinary scholarship – is achieved only at the price of irrelevance to anyone outside that academic discipline or sub-discipline. Put differently, academe has disciplines and the real world outside of academe has problems – none of which are 'merely academic'.

Interdisciplinarity is often touted as the way to free academics of their disciplinary blinders so that they can begin to develop real solutions to real problems. Yet interdisciplinarity creates all sorts of problems within academe, not the least of which are problems with peer review. As Huutoniemi (this volume) points out, evaluating interdisciplinary research is exceedingly difficult given the lack of agreed upon standards that disciplines provide. Graybill and Shandas (this volume) also point to problems for early career academics trained as interdisciplinarians, who are caught between publishing for the discipline that houses them or for a "new academy" that is yet to materialize: promotion and tenure decisions invariably turn on a record of publication in high quality journals, which are invariably organized (and peer reviewed) along disciplinary lines. Both of these chapters raise the fundamental question for academic interdisciplinarity: who counts as a peer?

Although this question does arise for the 'old academy' – for instance, it is typical to question whether more established investigators within a field are truly peers of early career academics or vice versa – the typical answer is that *disciplines define peers*. It is this answer that brings into relief the difficulty of evaluating interdisciplinary research (whether publications or grant proposals). Lamont (2009) provides a way of viewing the process of peer review – as an interactive social process in which the participants (all multidisciplinary panels of reviewers in her study) aim at a kind of Habermasian ideal speech situation, in which reviewers from different disciplines respect each other's differing disciplinary standards and aim to reach a consensus decision – that may prove useful in the review of interdisciplinary grant proposals. She also suggests that more intensive training of personnel at public funding institutions may be necessary in order to sensitize agencies to the exigencies of evaluating interdisciplinary research. Since many journal editors do not aim for consensus among reviewers, but treat reviews as a way to improve submissions, it may be easier for them to navigate the difficulties presented by an interdisciplinary submission, provided they are attuned to those difficulties and sympathetic to the approach the author takes. It may not be intellectually satisfying, but it may simply be a case of waiting things out until more and more of the old guard is replaced by members of the "new academy" for which Graybill and Shandas yearn, much the way that Kuhn suggested paradigm shifts might ultimately occur. Once the Graybills replace the graybeards, it is likely that things will be different.

Comment: Just wait

Although it is tempting to think of interdisciplinarity as only the labor pains that accompany the birth of 'new disciplines' for a 'new academy' – a kind of organic-developmental timeline view – interdisciplinarity within academe could also be seen as a kind of mean between the extremes of isolated disciplinarity and engaged transdisciplinarity. Disciplines serve both to carve out territory within academe and to separate academe from the real world. Interdisciplinarity breaks down disciplinary boundaries within the halls of academe; but transdisciplinarity is needed to tear down the walls of the Ivory Tower. This may sound like what Huutoniemi (this volume) terms a critical approach to disciplinarity, in which case it would make sense to reference "Mode 2 science," "Post-normal Science," and "Knowledge Policy" –

one might also add “well-ordered science” (Kitcher 2001) and “Pasteur’s Quadrant” (Stokes 1997) – and to call for some form of extension of peer review beyond academe to include not just reviewers from different academic disciplines, but also other stakeholders in the decisionmaking process. But such an approach can always be criticized as overly theoretical (or even ideologically committed to epistemological holism).

Rather than approaching the issue of transdisciplining peer review from an ideological or theoretical standpoint – *i.e.*, from an academic point of view – let us begin with a problem in the real world, one for which some empirical evidence already exists, and on which experiments could be conducted: the Government Performance and Results Act (GPRA) of 1993. GPRA is designed to focus US Federal agencies on measuring and improving results, which, once communicated to Congress, will provide decisionmakers with the necessary data to assess the “relative effectiveness and efficiency of Federal programs and spending.” GPRA’s explicit mandate is to require three things of all Federal agencies: (1) multi-year strategic plans, (2) annual performance plans, and (3) the development of metrics that would gauge adherence to the annual performance plans. The underlying message of GPRA is that agency plans must be tied to societally relevant outcomes. This presented a particular challenge to NSF, since it is the one Federal agency devoted to supporting basic research.

Basic research, as Vannevar Bush had so clearly articulated, is conducted without consideration for the results. With the passage of GPRA, NSF found itself, more starkly than before, caught between politics and science. NSF is what Guston (2000) refers to as a “boundary organization” – as the federal agency responsible for supporting basic research, it owes allegiance both to the government and to scientists. While the government wanted to see results, basic scientists wanted still to be able to pursue basic, rather than applied, research (Kostoff 1997). How did NSF respond to these conflicting demands?

Not surprisingly, NSF did not respond as an academic might, by turning to the literature about post-normal, well-ordered, Mode 2, use-inspired science to create a new Knowledge Policy. Instead, the National Science Board (NSB), NSF's policy branch, restructured NSF's peer review process (known as 'merit review') to enlist the scientific community – both as proposers and as reviewers – in the task of articulating the societal relevance of the basic research NSF funds (Holbrook 2005). In 1997, the new merit review criteria were introduced, and they asked only two questions: *What is the intellectual merit of the proposed activity?* and *What are the broader impacts of the proposed activity?* Essentially, NSF engaged in what Miller (2001) calls "hybrid management." Peer review has always served both academic and political purposes – NSF simply manipulated these elements to place a greater emphasis on the political function of peer review, without stripping scientists of the academic autonomy they demand. Proposers and reviewers were still asked to articulate and evaluate the intellectual merit of proposals (for which they could still appeal, in most cases, to disciplinary standards of excellence); but they were also asked to articulate and evaluate the impact of basic research on society (for which they lacked the expertise).

In effect, NSF was asking scientists to break free from their disciplinary bounds and to engage in activities that involve interdisciplinary and transdisciplinary interactions (*e.g.*, communicating one's research beyond one's discipline, either to academics in different fields, or in novel ways to non-academic society; communicating one's research to political decisionmakers in useful ways; enhancing diversity in ways that go beyond a simple head count of minorities; training graduate students and mentoring postdoctoral researchers in the ethics of research; etc.). Scientists, to put it baldly, balked (Frodeman and Holbrook 2007). In part, this is because most scientists trained along disciplinary lines to conduct basic scientific research are generally *not* trained either to articulate or evaluate the societal impacts of their work. The Broader Impacts Criterion (BIC) was at first simply ignored, until NSF announced that they would begin returning without review proposals that failed to address BIC, at which point compliance began to rise. Even after more than a decade, however, the quality of responses to BIC remains a persistent problem.

Beginning in this way with a real world problem – NSF's response to GPRA, scientists' response to BIC – allows for an important point: science studies scholars need not call for a "transdisciplinarization" of the process of peer review, for the transdisciplining of peer review has already begun. Moreover, the case of NSF, unique as it is, is not unlike changes to peer review processes at other public science funding agencies around the world, many of which have incorporated similar societal impacts criteria into the process of peer review (CAPR).

5. **Evaluating disciplinary, interdisciplinary and transdisciplinary relevance**

Disciplinary expertise is required to assess disciplinary excellence. Hence, reviewers charged only with assessing the disciplinary merit of a grant proposal (or article submission) need only be selected from the particular discipline under consideration. A mix of disciplinary expertise(s) is required to assess academic excellence beyond a single discipline. Hence, reviewers charged with assessing the merit of multidisciplinary or interdisciplinary proposals ought ideally to be selected from all the disciplines included in the proposals. Although review of such multidisciplinary and interdisciplinary proposals is more complicated than mono-disciplinary review, it nevertheless takes place within academe, where each reviewer is ideally accorded a kind of authority over her own disciplinary domain. What sorts of expertise are required to address and assess societal relevance?

To the extent that societal impacts criteria ask proposers and reviewers to address issues that can be addressed from within academe, experts can be drawn from the relevant disciplines to address those issues in the proposal and its review. For example, some societal impacts criteria can be addressed in terms of educational impact – in which case it would seem necessary to employ experts in education both in writing and in reviewing the proposals. This would simply present another case of interdisciplinarity with which peer review must cope. However, some societal impacts criteria take peer review beyond the disciplines to such issues as offering policy-relevant knowledge for societal decision makers. When societal impacts criteria go beyond the realm of academe to address societal relevance, if proposers are to make their research societally relevant and reviewers are to judge societal relevance, then who counts as a peer must be extended to include non-academic members of society at large.

Although these claims are normative, they are not based on an ideological imposition of theory onto reality. The claim is not that peer review should be de-disciplined, and either interdisciplined or transdisciplined in order to pursue some ideal form of knowledge. There is no ideology of epistemological holism at work here. Instead, the point can be expressed as a hypothetical imperative: If we introduce transdisciplinary criteria into the process of peer review, then we should expand the definition of who counts as a peer beyond the boundaries of the disciplines.

There is also a more comprehensive lesson to be learned: instead of thinking of peer review only in terms of its academic disciplinary use as an evaluation tool (according to which interdisciplinarity presents a special problem for peer review), peer review must also be addressed in terms of its larger social context. Doing so will allow us to see that peer review has never been only a disciplinary activity, one that ought to be jettisoned as an artifact of prepostdisciplinarity, but has always been a transdisciplinary activity, as well. Patrolling the border between academe and society, peer review can be the ultimate tool of transdisciplinary hybridization.

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