Intellectual Merit and Broader Impact: The National Science Foundation’s Broader Impacts Criterion and the Question of Peer Review

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Over the last 300 years science has been quite successful at revealing the nature of physical reality. In so doing it has provided an epistemological basis for scientific discovery and technological innovation. But science has been decidedly less successful at guiding political debate. How do we conceive of the science-society relation in the 21st century? How does scientific research hook onto the world in a multi-faceted, pluralistic, and global age? This essay seeks to reframe our thinking about the broader impacts of science by awakening an appreciation of the inescapably political and (and as a consequence, philosophical) dimension of all knowledge, scientific or otherwise.

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Introduction

In The Creation, his 2006 appeal for conserving the Earth’s biodiversity, Harvard biologist E. O. Wilson emphasizes the critical importance of educating the public into the dangers of massive species loss (Wilson 2006). While known as a brilliantly creative scientist, in this call Wilson follows a well-worn path, what Brian Wynne (1992) has called the deficit model of the public understanding of science. The deficit model believes that on the question of biodiversity—and it seems, on any number of other pressing societal
problems—scientists possess the crucial facts of the matter. Political action will be prompted by getting the public up to speed with the current state of scientific knowledge.

Such claims distinguish Wilson neither from the bulk of scientists nor from most of the attentive public. It has long been considered a truism (at least for those susceptible to rational appeals) that scientific facts over time should and will drive social and political values. This is despite the fact that studies have shown that increased scientific knowledge on the public’s part does not track well with a growing consensus on societal controversies. Just as commonly, new knowledge is fitted within a pre-existing worldview, thus simply becoming fodder for additional controversy and debate.

The linear model of scientific facts driving public values is due for an overhaul. Over the past 300 years science has been quite successful at revealing the nature of physical reality. In so doing, it has provided an epistemological basis for scientific discovery and technological innovation. But science has been decidedly less successful at guiding political debate. No political consensus has come out of 20 years of funding climate science, despite the massive increase in our understanding of the climate system. Similar points can be made about energy studies (e.g. peak oil) and ecological restoration (e.g. salmon, dam removal). Of course, there have been some striking successes in terms of addressing major societal problems (e.g. the polio vaccine). But it is likely that we have reached the end of the age of magic bullets: the problems we face are both epistemologically complex and politically contentious.

The question thus arises, how do we conceive of the science–society relation in the 21st century? How does scientific research hook onto the world in a multi-faceted, pluralistic, and global age? And, rather than there being a straightforward and linear connection between the two, might the science–society relation need a tertium quid?

This essay seeks to reframe our thinking about the broader impacts of science by awakening an appreciation of the inescapably political and (and as a consequence, philosophical) dimension of all knowledge, scientific or otherwise. Not political in the sense of the science wars of the 1990s and their unhelpful battles over social constructivism, but in the more basic and unavoidable sense that science is always funded and pursued for one or another societal end, and thus always bears the imprint of our needs, values, and aspirations. Political in the sense that, while facts can be stubborn things, scientists’ own daily experience testifies to the ineluctably social dimension of their work, as they spend their days (and nights) arguing over the correct interpretation of the facts of a given phenomenon. Such activity—the give and take of conversation, working through differing interpretations in the search for consensus—makes science in principle no different from any other type of reasoned political debate. It is a point worth emphasizing in an age where “political interests” have been reduced to bare-knuckle politicking, push polls, and sloganeering.

The tertium quid, then, between science and society is politics—politics in the rich (and original) sense of a reasoned community discussion of societal ends, and the best means to reach them. (We could as well say, philosophy.) Framed in this way, raising such questions constitutes a new type of the philosophy of science. For 60 years the philosophy of science has focused on questions internal to the scientific process, while ignoring the larger social, political, and economic aspects of science. Today, however,
we find a burgeoning philosophy of science policy that placed reflections on science within the context of societal decision-making (for example, Frodeman and Mitcham 2004). Such reflections have gained salience as it has become clear that the pillars of our previous understanding of how science connects onto the world have given way. One such pillar is the aforementioned belief that scientific facts can straightforwardly answer political debates. A second is the previously noted belief that science is something separate from political debate, rather than simply a well-ordered form of it. What we have instead is a growing appreciation that the science–society relation is both intricate and of decisive importance.

We thus set ourselves to the task of fashioning a better understanding of how social, political, and philosophical values play out within the science–society relation. Our case study here is the peer review of federal grant proposals, specifically the situation at the National Science Foundation (NSF). The evolution of the NSF peer review process since the early 1990s casts a powerful light on the ways that science is becoming both more crucial to and more accountable for the development of society.

The Genealogy of Broader Impacts

For a long time, the peer review of grant proposals to NSF was seen as a rather straightforward affair, at least in principle: scientific peers judged proposals in terms of their specific technical and scientific merit.

From 1981 to 1997, NSF guidelines identified four criteria for the evaluation of proposals:

● Research performance competence.
● Intrinsic merit of the research.
● Utility or relevance of the research.
● Effect of the research on the infrastructure of science and engineering.

Since 1997, however, NSF has used two criteria for the review of grant proposals: one focuses on the “intellectual merit” of a proposed activity, while a second asks for evaluation of the “broader impacts” of the research.

Since the implementation of these new guidelines, the overwhelming majority of thought and criticism of these criteria have been directed at the Broader Impacts Criterion (BIC). Intellectual merit was seen as a relatively unproblematic concept, adequately judged by competent disciplinary peers. NSF guidelines on BIC state that proposals can fulfil this requirement in a number of ways. Two of the most common are: education and public outreach (e.g. through K–12 programmes at museums), and broadening of the diversity of those involved in the research (e.g. by increasing the number of under-represented minorities). Both of these approaches, of course, are variations of Wilson’s model for improving the dissemination of scientific insights. It is worth noting in passing, however, that at least the goal of increasing diversity also opens the possibility of new groups bringing new perspectives and insights into the scientific process itself—a point more fully developed in Intemann’s contribution to this volume.
Now, NSF does list other activities that can fulfill BIC: enhancing the infrastructure for research and education, and identifying the proposed benefits of the research for society. But the radical nature of the last possibility is generally overlooked. The issue of what are the “societal benefits” of basic science is typically framed by unproblematic examples. For instance, recent NSF research has led to the discovery of a process to create stronger, more flexible concrete that will help prevent tragedies such as the 2007 Interstate 35W bridge collapse in Minneapolis. But are all the broader impacts of science and technology as innocuous as that?

In actuality, “societal benefits” implicitly raises a host of ancient and perennial questions concerning the nature of the good life. What counts as a benefit? By what standard? Who benefits—and who loses out? It is naïve to assume that there will not be some “negative” benefits as part of the innovation process. In the preamble to its list of representative activities, NSF encourages researchers to use their imagination. What of imagining the possible harms of the proposed research? Could this not also be a benefit to society? The single act of requiring each proposal to consider benefits and harms of its research could have a transformative effect on our thinking about the science–society relation.

As it turns out, continuing and increasing pressures are forcing modifications of the Wilsonian linear understanding of the broader impacts criterion—pressures that are providing an impetus for larger, fundamentally philosophical discussions. On 9 August 2007, President Bush signed into law H.R.2272, known as the America COMPETES Act. This act is in part a response to the perceived deficiency in American science education. It outlines the need to stay internationally competitive and at the forefront of scientific, technological, and thus economic innovation. It calls for improvement of education at various levels, and advocates the doubling of the budgets of NSF and the Department of Energy’s Office of Science. And finally it requires a suite of new measures expressing concern with the broader impacts of science. In particular, the America COMPETES Act expressly requires a report to Congress from NSF within one year detailing what effect the broader impacts criterion has had on scientific research and on society in general (ACA 7022).

This increased attention devoted to BIC, including increased (if haphazard) Congressional oversight, makes it imperative to reflect on not just how to measure the effects of BIC, but also on the nature of the criterion itself. In fact, the continuing evolution of BIC constitutes a real-world study in the philosophy of science policy. We are witnessing the practical breakdown of traditional philosophy of science: a shift from simply emphasizing the need for increased scientific knowledge on the part of the public, to making critical reflection on the relation of scientific discovery to societal priorities as part of the scientific research process itself. BIC is thus a mechanism for promoting a self-consciously critical, dialogical, and normative component in the relation between science and society.

Granted, and as already noted, the current version of BIC has a “benefits to society” dimension. But practically speaking, BIC “benefits” clause has been either ignored or has only a marginal influence on the peer review of grant proposals. BIC is not simply
an education and public outreach (EPO) criterion—but it generally gets (mis)interpreted in this way.

Mainstreaming Broader Impacts

According to the standard (and still dominant) model of science funding, scientific researchers create a reservoir of knowledge in the hopes of future downstream practical applications (Pielke and Byerly 1998). This reservoir model creates the conditions for societal progress without the need for scientists to explicitly aim for particular societal outcomes. Researchers who focus on the broader impacts of science, if involved in this process at all, are brought in at the completion of the research project.

In truth, even to refer to “broader impacts researchers” is to speak in terms of a neologism. While the social studies of science go back some decades, such scholars have traditionally taken an outsider’s ex-post facto perspective. Real-time assessment, where researchers are involved in and contribute to thinking on the broader impacts as part of an ongoing scientific research, is a quite recent development. (Even well-known studies such as Latour and Woolgar’s [1979] Laboratory Life examined the scientific process without attempting to affect it.) It has only been since the turn of the 21st century that we have seen the formation of a self-conscious community of researchers who seek to understand, and engage with scientists at the project level, on how to evaluate the broader impacts of scientific research. This is a group that Holbrook and Frodeman (2007) have named “ROSTS”—researchers on science, technology, and society. Others (for example, Frodeman 2008) have referred to such an approach as “field philosophy”.

One sign of the development of this incipient community was the formation of a new scholarly organization, the Philosophy of Science in Practice, in 2006. A second sign was the August 2007 workshop held at the Colorado School of Mines, on “Making Sense of the Broader Impacts of Science and Technology”. A third was the 2007 inception of a new programme at NSF called SciSIP, the Science of Science and Innovation Policy.

As Fuller (2008) notes, it is possible to see the history of the field of science and technology studies as going back as far as Popper and the logical positivists. But until the 1970s the direction of argument was different: in the wake of the First World War (think mustard gas), and again even more so after World War Two, the imperative was not to show the relevance of science to society, but rather the opposite. Many were ready to blame science for the destruction of Dresden and Hiroshima. Consciously or not, the focus of thinkers such as Hempel on the pure epistemology of scientific thinking served to insulate and isolate scientists from political controversies and the policy process. Counter-movements such as the Russell–Einstein Manifesto and the subsequent Pugwash conferences dating from 1957 did little to challenge the orthodoxy.

In the 1960s the first science, technology, and society programmes were created at Cornell, Penn State, Rensselaer, and MIT. The 1970s and 1980s saw the development of the sociology of scientific knowledge, and the promulgation of the “strong programme”—the claim that both true and false scientific theories should be treated the same way, in that both can be seen as importantly the result of social factors and
conditions. This led to the onset of the science wars, where extreme (and largely unhelpful) claims were traded back and forth about the objectivity or subjectivity of science knowledge.

It was not until the 1990s that sustained academic attention was turned to the policy implications of scientific knowledge, with the development of positions within policy studies that questioned the validity of the linear hypothesis—that there was a linear (i.e. automatic) relationship between science and societal benefit, and that scientific knowledge is by its very nature benign (for example, Sarewitz 1996). The questioning of this automatic relationship between science and the common good by policy researchers then set the stage for the development of new, fundamentally philosophical approaches to understanding the relation between science and society—the philosophy of science policy.

At first, evaluation of scientific research by ROSTS was “downstream” in nature: the possible broader impacts of the research (described by the terms “ethical, legal, or societal implications” [ELSI] of science and technology, or as “ethics and values studies”) were totalled up after the scientific work was done. This was emphatically the case with the Human Genome Project, where there was little or no effective contact between ROSTS and scientists. This despite the fact that between 3% and 5% of the annual Human Genome Project budgets were devoted to studies of the ELSI aspects of the project (for a discussion of the failure of the ELSI aspects of the Human Genome Project, see Kitcher [2001]).

It was soon clear that, to be effective, evaluation of the ELSI aspects of science and technology needed to be “mainstreamed”. Interaction between scientists and ROSTS should occur at all stages—upstream (in the design of the research programme and the creation of the call for proposals), midstream (during the ongoing research project itself), and downstream (at the end of the project). Promoting such an approach, Fisher (2007) spoke of “mid-stream modulation” where “embedded philosophers” work on a daily basis with scientific researchers, affecting research in progress rather than after it is complete.

Mainstreaming consideration of broader impacts implies greater focus on the more political and philosophical aspects of broader impacts—for if BIC is interpreted in terms of public outreach, there is no need for anything other than downstream involvement on the part of the public. This does, however, still leave the two criteria in place. Questions are limited to how intimately to relate the two. But does it make sense to have two criteria at all? What is the distinction between intellectual merit and broader impacts?

**Conclusion: Revisiting the Criteria**

The distinction between the two criteria turns on our faith in disciplinary boundaries. The evaluation of intellectual merit as distinct from broader impact depends on the identification of a group of peers who have expertise within a given intellectual domain. These experts are deemed competent to judge the cogency and quality of a submitter’s proposal. But in so far as a proposal—or more generally, the societal
challenges we face, or the nature of the world out there—are interdisciplinary and transdisciplinary in nature, the belief in a clearly identifiable set of peers to judge the intellectual merit of a proposal loses its justification.

Daniel Sarewitz highlighted this dilemma in 2000, in an article titled “Science and Environmental Policy: An Excess of Objectivity”. Sarewitz recounts an email exchange between climate scientists over who is competent to speak to the public on the subject of climate change. All seven of the email correspondents were acknowledged experts in one or another aspect of climate change—in fields such as marine ecology, atmospheric chemistry, biology, and soil science, with the group including a Nobel Prize winner. Nonetheless, the email exchange turned acrimonious, as the qualifications of these experts was challenged by a prominent climate modeller who doubted that the others possessed relevant expertise (Sarewitz 2000).

While the debate was conducted in terms of the epistemological *bona fides* of the correspondents, the underlying issue was really one of ontology, or where one draws proper boundaries between disciplines. The discussion highlighted the fact that there are no clear and unequivocal disciplinary boundaries to be drawn simply on the basis of epistemology. The world has problems, not disciplines; disciplines are useful fictions rather than reflections of the nature of reality. Relevant expertise for addressing a problem expands or contracts depending on needs and circumstances. Expertise is as much a political as an epistemological category.

Intellectual merit thus depends on a dubious ontological belief, the faith in clear and unequivocal disciplinary boundaries matching up with equally clear divisions in the world. Granted, such boundaries have a pragmatic function—they make it easier, for instance, to evaluate faculty for tenure and promotion, in that committees are able to compare apples with apples. The same is true with the review of grant proposals. But this convenience comes at a cost: we have defined the world according to our convenience rather than in terms of its own (promiscuous) nature.

“Intellectual merit”, then, can be seen as a species of “broader impacts”—the evaluation of the narrower or disciplinary impacts of a research proposal, whether on the scientific community or even only on a particular disciplinary problematic. When researchers speak of the potential “significance” of research, this is but a placeholder for the impact or influence the research is likely to have within their own community. “Broader impacts”, then, is the wider or interdisciplinary and transdisciplinary accounting of significance. But since the distinction between narrow and wide (or disciplinary, interdisciplinary, and transdisciplinary) is merely proximate, there is in effect only one category—that of “impacts”, narrow and wide.

This is not a call for refashioning the criteria. In the end, it little matters whether the criteria are left as are, or are reduced to one. What is important is for researchers and reviewers alike to recognize the complexities tied to the evaluation of proposals—and to avoid the more simple-minded accounts of the nature of peer review (Holbrook 2010).

The entire subject of expertise and peer review deserves a thorough review of its own. The literature on expertise has grown significantly in recent years (for example, Collins and Evans 2007; Crease and Selinger 2006; Ericsson et al. 2006). Peer review has gotten less attention, although Holbrook (2010) offers a summary of the literature. In
addition, the authors of this essay, along with some other colleagues, are currently in
the midst of a three-year research project funded by NSF called CAPR, the Compara-
tive Assessment of Peer Review. This study is examining how six different public
science agencies in three different countries conduct the peer review process, with
particular attention to how questions of “broader impacts” are addressed.

This research will be published beginning in 2011, but it is already clear that every
agency is struggling with overcoming the 20th-century legacy of the philosophy of
science, where science was considered both objective and utterly distinct from cultural
affairs. This constitutes a cautionary tale to those who believe that the reflections of
philosophers are irrelevant to practical affairs.

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Notes

[1] Numerous exceptions to this statement can be found, from Toulmin (1961) to Kitcher
(2001). But to a first approximation the statement holds.
This and other similar accounts are available from www.nsf.gov/pubs/2009/nsf0903/
nsf0903.pdf; INTERNET.
[3] A summary of that meeting, whose products include the present issue, is available from
http://www.ndsciencehumanitiespolicy.org/workshop/; INTERNET.

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