AGENDA

Potentially Transformative Research: Social and Ethical Implications

March 8-9, 2012 @ NSF
Stafford II Room 555

NSF Headquarters, Arlington, VA

Thursday, March 8	Friday, March 9
8:00: Coffee and bagels 8:30: Welcome/introduction/charge to the group 9:00-11:30: 5-8 min summary, 15 min discussion of individual one pagers 11:30-12:10: Discussion 12:10-1:00: Lunch 1:00-4:10: 5-8 min summary, 15 min discussion of individual one pagers 4:10-5:00: discussion 7:00: dinner	8:00: Coffee and bagels 8:30-11:10: 5-8 min summary, 15 min discussion of individual one pagers 11:10-1:00: Discussion and next steps. End at 1 pm

Thursday, March 8

8:00: Coffee and bagels

8:30: Welcome — **Cora Marrett**, Deputy Director, NSF

8:45: Workshop Introduction — **Robert Frodeman**, Univ of North Texas

9:00: A Colon is More than a Matter of Punctuation: An Analytical Riff on the Workshop's Title
Irwin Feller, Penn State/AAAS

9:20: Can Peer Review Promote Transformative Research?
Edward Hackett, Arizona State University

9:40: Transformative Research
Robert Hull, Rensselaer Polytechnic Institute

10:00: Transformative Research — Beyond Silos, Unexpected Results and Invention
Roop Mahajan, Virginia Tech

10:20: **Morning break**

10:30: NSF Transformative R&D Workshop Comments
Ned Woodhouse, Rensselaer Polytechnic Institute

10:50: Transformative Research and Education then and now: Are we never happy with where we are?
Jane Maienschein, Arizona State University

11:10: Stable Scientific Strategies and the Unexamined Frontier of Knowledge
James Evans, University of Chicago

11:30: Common discussion

12:10: **Lunch**

1:00: Bridging the Ideal and the Policy Senses of Transformative Research
James Collins, Arizona State University

1:20: What is Transformative Research? How Does it Relate to Responsible Innovation?
Michael Gorman, University of Virginia

1:40: An Early Examination of Peer Review, Innovative Research, and Grant-Giving
Marc Rothenberg, NSF

2:00: Transformative Research: Social and Ethical Implications – Some Thinking Points
Luis Amaral, Northwestern University

2:20: **Afternoon break**

2:30: Transformative Research: Inchoate Thoughts about an Incoherent Concept
Dan Sarewitz, Arizona State University

2:50: Normal Science and Innovation
Benoît Godin, INRS University (Quebec)

3:10: Identifying Potentially Transformative Research
Gregory Feist, San Jose State University

3:30: A Crucial Issue in the Discussion of Transformative Research
Juan Rogers, Georgia Tech

3:50: Concluding discussion

5:00: **End Day 1**

7:00: **Dinner at Bangkok Bistro**: See website for directions and menu details.

Friday, March 9: Next Steps

8:00: Coffee and bagels

8:30: Summary of the questions before us

Robert Frodeman, Univ. of North Texas
J. Britt Holbrook, Univ. of North Texas

8:40: The Necessarily Proactionary Nature of Transformative Research

Steve Fuller, University of Warwick

9:00: Transformative Research Can Have Transformative Broader Impacts

Mark Frankel, AAAS

9:20: Linking Transformative Research to Broader Policy Goals in the EU Context

William Cannell, European Commission

9:40: Transformative Research: Chinese Perspectives

Wang Nan, Colorado School of Mines

10:00: **Morning break**

10:10: Transformative Governance: Is It Possible?

David Rejeski, Wilson Center

10:30: Transformative Change as a Qualified Good

Carl Mitcham, Colorado School of Mines

10:50: Identifying Potentially Transformative Research: Peer Review and its Alternatives

J. Britt Holbrook, University of North Texas

11:10: Discussion about workshop report, dissemination of report findings, coordinating activities, future research, etc.

1:00: **End of workshop**

This Workshop is supported by the National Science Foundation under Grant # ***1129067***. *Any opinions, findings, and conclusions or recommendations expressed in this material are those of the auhtor(s) and do not necessarily reflect the views of the National Science Foundation (NSF).*

A Colon is More than a Matter of Punctuation: An Analytical Riff on the Workshop's Title

Irwin Feller
AAAS, The Pennsylvania State University

Analytically, historically, and potentially of considerable programmatic importance, the colon that separates the two phrases in the workshop's title constitutes more than a transition from the general to the specific, a mainstream grammatical use of the colon. Rather, for all intents and purposes the colon symbolizes the bifurcated nature of recent discourse about the characteristics of, needs/opportunities for, and programmatic initiatives directed at fostering transformative research, on the one hand, and discourse about the need for or impacts of formal, systematic incorporation of public values and ethics into the articulation of agency programmatic priorities, the criteria for selecting proposals to be funding, and the criteria by which agency research programs and research projects are to be evaluated, on the other.

First, evidence that the bifurcation exists. The evidence is of the dogs that don't bark type. It is found in the reciprocal scant attention in recent seminal NSF documents and related literatures that address transformative research and broader impacts (which are treated here as a proxy for social and ethical values.) The National Science Board 2007 report, Enhancing Support of Transformative Research at the National Science Foundation, for example, is "science-driven." Its call for increased support for "ideas that have the potential to radically change our understanding of an important scientific or engineering concept" is essentially a critique of the (acknowledged) risk-averse selection of research projects caused by NSF's (and NIH's) divisional structure and the disciplinary-based conservatism (with respect to Criteria #1) of peer review panels. The report though is silent about social and ethical implications. Essentially absent too, with a few exceptions, was discussion of such implications at the NSB workshops that preceded preparation of the report.

Conversely, the recent surge of studies on NSF's Criteria # 2, Broader Impacts, such as Rothenberg's informative history on the origins of NSF's merit review criteria, the articles in Social Epistemology's 2009 special issue, or the broader Bozeman-Sarewitz brief on behalf of a public values framework to setting research priorities and related ongoing research projects, are replete with analytical insights and normative arguments calling for increased influence on "non-scientific" evaluative criteria. This literature, though, seldom makes specific reference to transformative research, at least in terms of its NSB's articulation (or indeed of recent NSF programmatic initiatives).

Accepting that such a bifurcation exists raises a variety of questions (or opportunities/challenges) about the contents, objectives and (likely) impacts of this workshop:

1) What, if any, differences exist in consideration of the role of ethics and values as proposal selection or output assessment criteria between transformative and "normal" (non-transformative) NSF research? One might argue by inference that transformative

research is sui generis to all NSF (NIH) funded research, so that no specific mention or consideration is required. Here indeed a colon is appropriate. But if so, what is the intended/realized value added of this workshop above and beyond ongoing, indeed intensifying soul-searching within several disciplines about their community's collective prioritization of research questions (and methodologies), research agendas in turn that condition the priorities and decisions of funding agencies. The question arises in part from observing the angst now evident in political science and economics that current definitions of what constitutes "important " research questions (shaped in turn by dominant theoretical paradigms and methodologies) have marginalized many issues (and researchers). As expressed by the Task Force of Political Science in the 21st Century, "Political science is often ill-equipped to address in a sustained why many of the most marginal members of political communities around the world are often unable to have the needs effectively addressed by governments." Similar intellectual unrest exists in economics about the dominance of propositions that apotheosize the efficiency of competitive markets to the marginalization of attention to issues of income distribution, poverty, or links between the concentration of economic and political power ("Economists Push for a Broader Range of Viewpoints in Their Field," Chronicle of Higher Education, January 6, 2012, A11ff.). These examples in part raise anew recurrent semantic issues about when a substantive reorientation of a field's prioritization of topics and methodologies constitutes a transformative change as contrasted with a return to and renewed acceptance of longstanding but now heterodox approaches. More substantively though it hones the question of what is new or different, if anything, in consideration of ethics or values for transformative research that does not apply to all research?

2) Rooted in the scientific, technical, and economic histories of major discoveries, this question is quite different: Is it possible within some reasonable degrees of confidence to predict the ethical or value impacts, positive or negative, of paradigm changing discoveries in science and technology? A cottage industry exists with abundant evidence about the presence of Type 1 and Type 2 errors in predicting how, when, who, and under what conditions major scientific or technological advances will have their impacts-economic, political, societal, and ethical. Adding further complexity to how one might answer this question is that even if one was accurate in predicting implications/impacts, it might simply shift the debate from predictive accuracy to normative criteria. Deep ethical cleavages can and do exist both about the manifest impacts of widely adopted scientific advances-birth control pills- or of the potential for work in progress-stem cell research. Given uncertainty about impacts and disagreements about the societal desirability of the sought after impacts, how does one transform consideration (concern) for ethics and values into the selection of research projects?

3) This is the most programmatically oriented question. In the short turn, it is the one for which answers are most latent with implications, many of them worrisome, for NSF's behavior: What consideration/weight should be accorded to ethics and values in reviewing transformative research proposal? Are ethics and values to be specifically highlighted in articulation of Criteria #2? Who are the experts/peers qualified to make predictions about ethical or normative impacts for paradigm changing undertakings? What is the historical or analytical basis for answering these question in light of the

evidence forthcoming under #2, or the Bozeman-Boardman argument that, "Retooling or refining the broader impacts criterion does not alter the fact that conventional peer review, based on specialized scientific and technical expertise, is not up to the task of ensuring adequate judgments about social impact"?

Can Peer Review Promote Transformative Research?

Edward J. Hackett
Arizona State University

In recent years there have been calls from various quarters for policies to promote greater boldness and originality in US scientific research. Radically new science is invoked as an engine of innovation in the National Research Council report "Rising above the Gathering Storm" (NRC, 2007), in initiatives to create a "science of science and innovation policy" (Marburger, 2005), and in recent *Science* editorials (Leshner, 27 May 2011; Rosbash, 8 July 2011) that lament the eroding US research climate. The National Science Board called bold and original research "potentially transformative" because it is "driven by ideas that have the potential to radically change our understanding of an important existing scientific or engineering concept or leading to the creation of a new paradigm or field of science or engineering" (NSB, 2007: 10) and instructed NSF to devise programs and procedures to promote such research.[1]

NSF complied, revising and enlarging funding instruments capable of supporting urgent or speculative research with a brief proposal and minimal review (Rapid and EAGER (Early-concept Grants for Exploratory Research), see NSF 2011). NSF also altered its review criteria to mention transformative research, and instructed panelists to take this into account in their reviews and program officers to do so with their recommendations. But, as one NSB member at the time has since observed, "urging peer reviewers and funders to support more high-risk but also potentially high-payoff or transformative research, which can revolutionize fields, has not worked well, at least not in the United States" (Leshner, 2011: 1009). Why?

To answer I will draw upon the history, philosophy, and social studies of science to reframe the challenge of encouraging potentially transformative research as a matter for broadly-based investment and innovation, rather than a narrower matter of recalibrating peer review criteria or inventing new funding programs. In particular, I would broaden the definition of transformative research to include more varied forms of deeply original

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[1] An independent but similar line of argument led NIH to devise Pioneer, a research award program that offers unparalleled freedom of inquiry, and a "Roadmap" process that supports large-scale centers to promote interdisciplinary integration.
[2] Although the NSF, as organization, has been active on studying innovation from its very beginning (see Appendix).
[3] In a large sense: a good, a method, a protocol, a policy or law, a service; briefly stated anything that is 'useful' to

science and, by implication, include also the policies and practices that would stimulate such work. The implicit concern of the NSB and others to select individual projects and investigators that might make "particulate" transformative discoveries overlooks the social processes and temporal dimensions of science which embed and give meaning to research results and theoretical ideas. Science is inherently conservative and incremental, critically challenging new results and ideas before accepting and building upon them, and therefore policies to encourage potentially transformative research must accelerate the systemic processes through which research results are communicated, evaluated, and pursued. The policy emphasis on revamping peer review is grounded in the mistaken view that sets peer review apart from the research process—an antecedent of receiving a grant or publishing a paper. To the contrary, peer review is among the core systemic processes of science and an integral part of the conduct of research, and for that reason it embodies and reflects the fundamental conservatism of science.

Transformative Science and the Research Process

Research may be "transformative" in a variety of ways, not all of them direct confrontations of received understandings. For example, transformative research may synthesize diverse data and results into a more comprehensive or integrative explanation, demonstrating that more general or fundamental processes underlie a spectrum of apparently inconsistent phenomena. Or it may open a new sphere of research by raising novel questions, devising new research technologies, or uncovering new phenomena to study. The defining characteristic of path-breaking research, however it is accomplished, is that other researchers follow the path, and so research of this derivative, incremental nature is an essential complement to bold innovation. In fact, it is the purpose of breaking a path is to ease the passage of others.

> Paths may be broken in many ways: through new technologies (PCR, radio astro, which was resisted and ignored), through new ideas (plate tectonics, nano-scale S&E (Feynman), viral oncology→oncogenes), engagement with real-world problems (Pasteur; Stokes's "Pasteur's Quadrant").

The generally incremental character of research was observed and explained decades ago—the core ideas of Thomas Kuhn's <u>Structure of Scientific Revolutions</u> (1962) were proposed in the late 1950s, including the concepts of normal and revolutionary science. In Kuhn's view research is performed in dynamic tension between the inconsistent demands to say something new and to build upon the extant body of knowledge. Original ideas and results, while highly valued, are correspondingly strongly questioned: it is of the essence of science to seek originality and mistrust it (through organized skepticism, for example, as exercised by individual self-criticism and through the peer review system). The practice may become trying for, as François Jacob noted, science presents a "universe of limitless imagination and endless criticism" (1988: 8). Here are some of the things we know and what they mean for ways to promote transformative, pioneering research.

Discovery is a social process: discovery takes time and occurs within a community of scientists. Major discoveries are often multiple discoveries, occurring after a succession of false starts and incomplete efforts, and seldom the work of a lone scientist completed in a single research act (Merton, 1973: 343-382). To emphasize the point Merton was fond of quoting Alfred North Whitehead: "But to come very near to a true theory, and to grasp its precise application, are two very different things, as the history of science teaches us. Everything of importance has been said before by somebody who did not discover it" (Whitehead, 1961). Discoveries may be initiated by one scientist and completed by others some years later. For example, the meaning and transformative force of Mendel's results resided in the inferences drawn and uses made of that research by scientists working several decades later, applying concepts and theories unknown to Mendel (Holmes, in Hook: 164-174). Transformative research—discoveries—emerge through a social process that occurs within the scientific community.

On this argument, the key to accelerating transformative research lies in accelerating the process of transformation, which begins with new ideas or findings, but continues to include critical review, response, restatement, incorporation, adoption, replication, and exploration. Science is organized to express skepticism in various ways (through peer review, discussion boards, dissertation defenses, open publication and review), and scientists' critical faculties are honed in graduate seminars, laboratory meetings, and the internal dynamics of collaborative groups (Owen-Smith, 2001; Hackett et al., 2008). Skepticism is applied sequentially within the scientific research group, specialty, and community, where it is guided by the judgment and research decisions of the core set of scientists most expert and involved in a particular sphere of inquiry. Accelerating this process would require increased efficiencies, investments, and innovations in the conduct of science, which includes the review processes that occur within the expert scientific community.

Scientists deeply experienced with a line of inquiry, however, are also deeply vested in their own ideas and practices, and may resist potentially transformative ideas, as the historical record shows. Some resistance is grounded in competing commitments to ideas which impair the ability to comprehend and deploy new ideas (e.g., evidence deduced by Avery and colleagues that DNA, not protein, carried biological information; Kay, 2000: 55-57). Other discoveries may be resisted because they are "premature," their plausibility foundering on expert knowledge that renders them unlikely or unproductive, their acceptance awaiting ideas or findings that fill in details, propose workable mechanisms, shed doubt on competing explanations (such as Wegener's theory of continental drift; Oreskes, 1999; even the idea of prematurity itself was premature; Hook, 2002). Whatever the reason for resistance, its presence implies that accelerating transformative research will require more than placing winning bets on particular individuals or proposals. It is vital to understand that path-breaking research and path-following research are not opposites but complements: a discovery becomes transformative by virtue of the volume of productive research that follows--the deflection of the stream of research along a new course is the transformation.

Peer Review and the Process of Science

Accompanying calls for greater investment in transformative research are claims that the peer review system is responsible for conservative funding decisions and recommendations for improved programs, policies, and procedures to remedy this flaw. For example, NSF responded to the NSB report on transformative research by rewording its merit review criteria, reshaping its discretionary funding mechanisms, and re-emphasizing program officers' judgment in the decision making process. Why do such reasonable changes in programs and procedures fail to have the desired effect? The reason, in part, is that they misconstrue the nature and purposes of peer review.

Peer review is not a selection process that precedes or follows the conduct of research but is instead an intrinsic part of the practice of science that embodies and reflects the constituent elements of science. For this reason peer review must not be regarded as an impediment en route to the registration of a transformative discovery but instead as an intrinsic part of the transformative process. It is how scientists learn and incorporate a new idea or result, and how, through criticism and emendation, a new result is shaped. For this reason, peer review is central to the process of science and therefore balanced among a set of inconsistent purposes and competing values, a position that entails inherent tradeoffs between desirable qualities. This argument has been developed elsewhere, so I will briefly summarize the basic idea here (Chubin and Hackett 1990),

Among the purposes of peer review are:

Evaluate proposals and manuscripts: rating and ranking proposals for award or decline, and manuscripts for publication, revision, or rejection;

Advise scientists and decision makers: commenting on substantive aspects of scientific work, for the benefit of the author, editor, science agency, and wider publics;

Impart inertia: sustain the velocity of research in a field of science, helping it navigate around fads, foibles, and flops;

Communicate: circulate ideas and plans among scientists working at the research front;

Exercise professional authority: apply the standards and principles of scientific expertise in ways that distinguish science from other endeavors;

Guarantee accountability: embed science within society through structured and limited modes of formal responsibility.

Accompanying these diverse purposes are a set of value dimensions, with tensions within and across pairs. Among the most salient value tensions for the evaluation of potentially transformative research are:

Originality-Tradition: this is the "essential tension" of science (Kuhn, 1977 [1957]): to support new ideas, approaches, and topics ***yet*** sustain the research trajectories of scientific fields (ironically, the most original work opens spheres of inquiry that provide ample opportunity for follow-on work and the luxury of time to do it).

Selectivity-Sensitivity: exclude unsound ideas, weak designs, fishing expeditions, "flyers," and fads (or risk winning the contemporary equivalent of a "golden fleece") *yet* remain sensitive to imaginative ideas, novel approaches, and new topics (something like the tradeoff between noise and low-light imaging in a digital camera).

Effectiveness-Efficiency: provide thorough and expert review to identify the best research for publication or funding support *yet* do so at the lowest cost and least burden to the review community.

Responsiveness-Rigor: address the urgent, emergent research issues of the day *yet* uphold the highest standards of methodological rigor.

Validity-Reliability: adequately evaluate all facets of a manuscript or proposal (which may require reviewers with varied expertise who attend to different parts of the work with different degrees of attention) *yet* insure that reviews agree with one another (or they will appear unreasonable, illegitimate, and perhaps silly).

The desirable qualities of sensitivity and selectivity, which are germane to the selection of potentially transformative research and to insuring that resources are allocated efficiently, are in tension: a selection process designed to be sensitive to any scientific merit in a paper or proposal will likely accept a certain number of overly speculative works, while one designed to select only work that is utterly sound must accept that some good ideas will be declined and discouraged. Increasing the amount of transformative research in a field would require increasing sensitivity and reducing selectivity, which may raise concerns about risk or waste. Similarly, to review potentially transformative proposals with sufficient care to be effective may raise concerns about efficiency. The NIH Director's Pioneer Award Program, an exemplary effort to elicit, evaluate, and support transformative ideas, promises to make at least 7 awards in the fiscal year 2011 competition, having made 17 in the previous year (https://commonfund.nih.gov/pioneer/; Rosbash, 2011).

Reasonably enough, strategies to encourage transformative scientific research tend to focus on individual scientists and their proposals. Specific ideas include creating new programs devoted to high-risk/high-reward research, allowing more time for an idea to bear fruit, and supporting greater numbers of young or female or minority or disabled investigators (Leshner, 2011). Stimulating potentially transformative research by choosing individual projects or investigators in a national competition, even one specifically tailored for the purpose, is not likely to succeed. Discoveries are not particulate but result from a process that involves critical evaluation and emendation at every stage. To accelerate transformative discovery it is necessary to accelerate a field of research, and the breadth of the field widens as interdisciplinary connections increase. Path-breaking research acquires meaning and impact from those who follow the path, which is enabled by the commitments of scientists and investments of funding agencies. Peer review is an intrinsic part of the process of evaluating and recognizing new ideas and results, not a preliminary hurdle to leap or impediment to remove.

What to do?

Science is a social process, so a strategy with greater likelihood of success would support organizations, collaboratories, contexts, and technologies that might catalyze transformative research. Such a strategy would alter the ecology of research in ways that would increase the rate at which sound new ideas produced, evaluated, selected, and incorporated into future research. The aim of this strategy would be to accelerate the evolution of scientific ideas and results.

Discretionary resources of time and research material are a well-documented source of transformative science (Hackett, 2005; Heinze et al. 2009). Serendipitous discoveries, which are fortuitous but unpredictable, occur when there is sufficient discretionary time and other resources to allow exploration of unexpected occurrences (Barber and Fox, in Barber, 1990: 83-95; Merton and Barber, 2004; Hackett, et al., 2008).

But academic capitalism and its attendant demands for greater efficiency, accountability, and measurable (countable) performance, complemented by similar drives for greater efficiency and accountability in government funding agencies, have wrung discretionary time and resources from the research system (Hackett, 1990; Slaughter and Leslie, 1999). Restoring flexibility and discretionary time and resources is one strategy for encouraging transformative research.

Young scientists with fresh ideas, the latest techniques, high ambitions and unbounded energy are potential sources of transformative science (Leshner, 2011), but declining numbers of secure jobs, coupled with the resource pinch described above, diminish their ability to undertake high-risk research. They may also lack the perspective to frame a major research challenge, the context to recognize a powerful result, or the patience to pursue a promising but difficult line of investigation (Hackett, 2005). But mature scientists, in contrast, can provide such guidance, perceptiveness, and persistence, and so structures that bring such qualities into collaboration hold promise for accelerating the pace of transformative discovery.

Diversity of various sorts stimulates original thinking, and so creating places where diverse ideas meet, mix and may be synthesized is a promising strategy for stimulating transformative science (Carpenter, et al., 2009; Page, 2007; Leshner, 2011). Synthesis centers have been built or are proposed in a wide gamut of fields, beginning with ecology (National Center for Ecological Analysis and Synthesis) and extending through the life sciences (National Evolutionary Synthesis Center; iPLANT, National Institute for Mathematical and Biological Synthesis), and into engineering, energy, and building technologies (Energy Efficient Building Systems Regional Innovation Cluster in Efficient Building Systems Design, Philadelphia). The ecology center is the longest-lived and best known of these, and its research output and pattern of organization have transformed the field of ecology (Hackett et al., 2008). It remains to be seen if this is a portable model.

Emotional energy drives bold scientific thinking, and organizations that generate and concentrate emotional energy, even episodically, can transform fields of science (Parker and Hackett, under review). Such places create conditions that engage the whole scientist, creating “hot spots and hot moments” in which imagination is freed and a sort of intellectual fusion takes place.

We need to understand more about the nature and varieties of risk and failure in science and their consequences. Risk, in one form or another, is present in every sort of research: even the most conventional experiment runs the risk of triviality and being ignored, and virtually every scientist I ever interviewed had a research portfolio with a mix of more and less risky projects (Hackett, 2005). Failure is the unmentioned accomplice of transformative research, and until we accept that bold science invites failure or, stated more congenially, delayed success, we are unlikely to advance very far in devising strategies to encourage path breaking work. Much of what scientists attempt does not succeed, so recognizing and coping with failure (at many levels—the experiment, the research theme) is an essential but under-examined quality of the scientific mind. Even the basic decision to persist or desist in the face of failure conceals a wealth of complexity, as there is wisdom in cutting losses and there is honor in perseverance. I know of no research on this crucial aspect of research judgment.

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Transformative Research

Robert Hull
Rensselaer Polytechnic Institute

I approach this issue from the perspective of a researcher and educator in the field of materials science and engineering (MSE).

I perceive two broad classes of "Transformative Research" in MSE and related disciplines. The first relates to the development of new languages and new intellectual infrastructure to help define new fields of research. A classic example is the creation of the periodic table, which helped establish the modern framework for physical chemistry. A more recent example is the development of structure – property maps by Ashby and co-workers, which has transformed the field of materials selection for engineering applications. Key questions: do the current scholarly / academic (e.g. journal policies, tenure processes) and funding structures adequately support and encourage the sustained intellectual focus required to enable such developments?

The second broad class is the "transformative discovery" of new classes of materials. There have been several such examples in recent years, e.g. graphene, fullerenes, carbon nanotubes, quasi-crystals, high temperature superconductors, etc. It is interesting to note that while some of these discoveries were accepted pretty well immediately, some were only fully accepted after several months or even years of controversy. The differences in the speed of acceptance by the community can be understood at least partially on the basis of logical factors such as the ability to rapidly reproduce results, the existence of different theories regarding the nature of the new materials. Another issue with respects to transformative discoveries of course is how correct and complete the original report and interpretation are, and of course a fundamental skepticism regarding new breakthroughs is important to the scientific community in avoiding too much momentum developing for "discoveries" that prove to be incorrect. But the initial transient in gaining the acceptance of the community can prove to be very trying and stressful for those making the discovery, and lead in some cases to quite acrimonious debate, whatever the ultimate recognition or reward. Key questions: What factors differentiate the length of the acceptance time for *bona fide* new discoveries? How might this process be accelerated, while maintaining sufficient judicious review to identify false discoveries?

Finally, do our methods to teach and train students and young scientists / engineers stifle or encourage the ability to make, recognize or accept a transformative discovery?

Transformative Research – Beyond Silos, Unexpected Results and Invention

Roop L. Mahajan
Virginia Tech

1. Interdisciplinary approach is critical to Transformative Research (TR).

At a 2003 Energy & Nanotechnology Conference at Rice University, noted scientist and Nobel Prize winner R.E. Smalley presented the following list of the top 10 problems of humanity for next 50 years: 1) Energy, 2) Water, 3) Food, 4) Environment, 5) Poverty, 6) Terrorism and War, 7) Disease, 8) Education, 9) Democracy and 10) Population. These problems have a few characteristics in common. They are challenging and complex, are interconnected, have a high degree of uncertainty, and are global in scope. For example, alleviating poverty and providing safe drinking water to fight waterborne diseases for the growing world population, especially in developing countries, will produce a significantly higher demand in energy. Meeting this escalating demand, without adversely affecting the environment, is a challenging task that will require transformative research beyond the reach of a single discipline. Although creativity can, and does, arise spontaneously from individual talent (Max Perutz, 1998, in his book "I wish I'd Made You Angry Earlier"), I believe that in our fast-changing world, many of the creative transformative solutions will arise at the intersections. While there is an increasing appreciation of the need and power of interdisciplinary research, the discovery/pursuit of inquiry at most academic and research institutions is still single-investigator focused. Reward systems including promotion and tenure are highly discipline-skewed. *For advancing TR, it is imperative that promoting and rewarding collaborative interdisciplinary research become a strategic initiative of academic and research institutions.*

2. Transformative research beyond unexpected results.

Since transformative research often leads to or, in some cases, arises from unexpected findings, there is a temptation to believe that high impact, paradigm shifting technologies cannot be systematically investigated. For example, in his New York Times best seller, "The Black Swan", author Nassim Nicholas Taleb notes that the three recently implemented technologies that most impact our world today – the Internet, the computer, and the laser – were all unplanned, unpredicted, and unappreciated upon their discovery, and remained unappreciated well after initial use. He calls such events and technologies Black Swans and maintains that these are unpredictable. However, it is my contention that we can build an environment and put processes in place to create a breeding ground for future Black Swans. For example, at the Virginia Tech's Institute for Critical Technology and Applied Science (ICTAS), we hold a monthly "Black Swan" Seminar in which engineers, scientists and humanists come together to explore the next potential disruptive or transformative technologies. Triggered by a question, "What technology/innovation/idea will make your field irrelevant in seven years," a few cygnets are hatched which are then nurtured with the hope that one or more of these will develop into the next transformative technology. For a more systematic pursuit of TR, *there is a need to develop and promote similar mechanisms with an emphasis on unencumbered, high-risk, high reward discovery.*

3. Transformative research through integration and innovation.

The classical domain of TR still belongs, in many circles, to new inventions. However, many of the inventions never translate into innovations. On the other hand, transformative impact may arise from the innovative way many existing or current technological concepts are deployed. I consider such innovation to be in the realm of TR. For example, in making a remote rural community self-reliant through sustainable technologies, TR could simply be developing solutions based on known technologies to provide sustainable energy, safe drinking water, sustainable agriculture, and basic animal and human health care. Similarly, achieving sustainable environmental development in cities through the integration of existing technologies and practices such as those for water conservation and reuse, green buildings and transportation, and smart grid infrastructure can be transformative. Transformative research, as viewed from the classical prism of invention may also arise, but I submit that *TR should also include Innovative and integrative way of using existing technologies with potentially transformative impact on society.*

NSF Transformative R&D Workshop Comments

Ned Woodhouse
Rensselaer Polytechnic Institute

In an advanced democracy, technological transformation and the research leading to it would be undertaken from the outset with the informed consent of affected interest, would proceed at a pace commensurate with needs and contexts, would contain built-in strategies for guarding against unintended consequences, and would be accompanied by incentives for learning rapidly from experience. Most of all, decisions would be real choices – meaning that "no" and "not now" would be within the standard range of options. Present behavior in and out of NSF approximates none of these criteria.

1. Do potentially affected interests have influential participation in decision making? Those who endorse a model of very "thin democracy" might believe that conventional congressional, parliamentary, or EU oversight fulfills this criterion. However, conventional political science and public opinion research document that existing systems are only weakly representative within their own boundaries – with the U.S. among the worst. More grossly, decision making about transformative research systematically excludes the majority of humanity living outside the science-dominant countries.

2. a) Informed? Scholarship on public understanding of science sometimes valorizes "lay knowledge," but the modal citizen obviously lack sufficient understanding of technoscientific issues to pass a bioethics panel review, partly because mass media coverage is extremely thin except for a few big controversies (e.g., civilian nuclear power in its heyday). Many forces intertwine to keep emerging technoscience from becoming more salient, but the potential change agents I blame the most are the NGOs that could

be serving as sources for journalists and as alternative routes to public representation. Selected exceptions such as etc Group's early warnings on nano health risks highlight how low transformative research usually is on most NGO agendas.

b) Nor is any existing electoral system organized systematically to select officeholders with the requisite qualifications to oversee complex technological phenomena. At last count, for example, there was one person with a reasonable grasp of chemistry on the House Science Committee. This competence gap combines with organizational factors to assure that elected officials have limited capacity to oversee the bureaucratic agencies responsible for science funding and for technological promotion/regulation. Agency and ministry staffs are more competent technscientifically, but with certain exceptions (Dutch dikes?, French nuclear power?, U.S. pharmaceuticals?), the bureaucracy is not granted sufficient authority for genuine oversight.

3. How timely is the decision making? Do non-scientists get to deliberate early enough to qualify as *choosing* or *authorizing* the transformative research – or does momentum become hard to reverse prior to real scrutiny? At least limited nanoparticle debate came sooner than for GMOs, which came sooner than with nuclear power, which came sooner than for robotics; except perhaps for early rDNA research, in no case of transformative research has real debate come soon enough.

4. Options? Is declining to perform transformative research a viable option? Is <u>anyone</u> *choosing, deciding*, or *directing* transformative research – or is *non-decision* a better description? NSF officials of course establish and seek funds for cross-directorate initiatives: But are they dreamers conceptualizing new endeavors, or interpreters-mediators responding to emerging technoscientific forefronts, or responders cued by researchers and their corporate, Defense Department, and other rallies/sponsors/clients?

   Consider, for example, a hypothetical case in which NSF declined to sponsor a line of transformative research. Would it not get picked up anyway by DARPA, DOD, or EU science agencies? Would not U.S. scientists then propose specific projects to their regular NSF sections to keep up with global competition? If emerging niches fill via such end-around moves, then arguably the only real choice is *when* to sponsor research, not *whether* to do so. This obviously is a soft variant of technological determinism, a notion anathema to many; but a social thinker still must ask: Are actual choices being made, or is transformative research closer to *happening* or *emerging as a vector outcome?*

5. What precautionary strategies and tactics are being built into transformative research and what additional ones might be? Nearly a century has elapsed since Capek's play about robotics, a billion people have seen a "Terminator" film, and robotics icon Isaac Asimov warned in a non-fiction source in 1974 that requisite technical capacities were developing in ways he had never believed possible – and that great care should be taken. Popular culture might be dismissed, but a highly qualified British roboticist, Kevin Warwick, argued systematically in *March of the Machines* (1997) that humanity has no realistic chance of remaining the dominant species after the advent of fully intelligent

robots. Carnegie Mellon's Hans Moravec agrees. And yet intelligent machinery both civilian and military continues to develop rapidly with few precautions (except, say, systems to prevent driverless forklifts from running over warehouse workers). Warwick's proposal for a non-proliferation treaty gained zero traction. Roughly the same naïve trial and error characterizes most of nanotechnology, synthetic biology, radical human enhancement, surveillance and data banks, and essentially every other transformative technology that I know about.

In sum, my view is that even in cultures and political systems somewhat more enlightened and workable than that of the U.S., transformative research is debated too late if at all; it is overseen by persons and organizations without the expertise or authority to intervene effectively; is driven by insiders whose self interest lies on the side of doing rather than not doing; contains few built-in precautions against unintended social consequences; does not create incentives for rapid learning except of a technosicentific sort; and is being mounted without the informed consent of 80-99+% of humanity. In other words, NSF officials responsible for transformative research by and large are acting irresponsibly and illegitimately; but they have plenty of company.

Transformative Research and Education Then and Now: Are We Never Happy with Where We Are?

Jane Maienschein
Arizona State University, Woods Hole Marine Biological Laboratory

I will comment very briefly on three relevant features:

1. My historical research on *Transforming Traditions in American Biology, 1880-1915* (Johns Hopkins University Press, 1991), which focuses on shifts in the life sciences that led to specialization and diversification. Technological and conceptual innovation went along with social, cultural, and economic changes within a rapidly expanding university system that made true transformation possible. Yet calls for innovation went along with emphasis on being grounded in tradition. Transformation did not mean revolution or rejection of established practices or ideas – hence the idea of transforming, but without losing the traditions. Is this a good thing?

2. At Arizona State, we have had ongoing lively discussions about what the "New American University" needs to transform in order to have become truly "transformative." The answer is not clear, and it is also not clear that change for its own sake actually transforms what we care about or how we implement change effectively when we do want it. I will offer some suggestions following on meetings during the next month.

3. We are developing new approaches to education that do attempt to change the way we teach and the way students learn. The goal is to add to the ways that research is done by putting students to doing real work rather than just class assignments that go nowhere. The NSF-funded Embryo Project has led to an online encyclopedia, style manuals, working seminars, and a model that we are expanding to other areas. Yet while we are trying to build on rich traditional values in education, and in seeking to transform the way the classroom looks, we nonetheless are promoting fundamentally traditional enlightenment approaches to understanding science in its social context.

What does this all mean, and is it good that we are stuck into boxes that emphasize what is thought, through some unspecified process, to be transformative?

Stable Scientific Strategies and the Unexamined Frontier of Knowledge

James A. Evans
University of Chicago

Scientific advance is profoundly influenced by scientists' choice of research problems. But how do scientists choose a research problem? And how do they select the elements they will assemble to solve it? Many factors influence these decisions, from past interests and training to serendipitous encounters with salient expertise and information. Intensifying this choice are professional pressures to make important, original contributions and to remain visibly productive. These conflicting demands create a tension. Scientists can choose to extend known scientific relationships with probable success but little surprise, achieving publication but not recognition. Alternatively, they can choose to explore novel, unexpected relationships. Most attempts will fail with no demonstration of productivity. When surprising relationships bear out, however, they often have profound impact within the scientific community. These choices mirror well-known dichotomies between "transformational" and "incremental" research, "succession" versus "subversion strategies" in the sociology of science and "exploitation" versus "exploration" strategies in the study of innovation.

My current research, in collaboration with computational biologist Andrey Rzhetsky and physicist Jacob Foster, uses a complex networks approach to consider strategic choices in the contexts of chemistry, medicine, computing, sociology and other areas. To what degree do scientists introduce novel compounds and novel relationships or repeat those defined previously? To what degree does their work consolidate existing subfields and chemical components or bridge distant ones? What is the frequency with which different investigators and different fields engage in transformational, high-risk research? How efficient are existing scientific strategies for discovering all that has been or could be discovered?

In the context of medicinal chemistry, our findings show that even as the network of chemical knowledge grows dramatically, the distribution of strategies remains remarkably stable: scientists focus narrowly on established knowledge and work within established subfields rather than on the increasing opportunities to link between them. It could be that scientists face difficulty in following or responding to the rapidly expanding knowledge horizon. As a result, despite exponentially growing opportunities to consider high-risk and potentially high-impact topics that bridge distinct areas, scientists demonstrate a persistent *preference* for local low-risk, low-impact information. Higher risk strategies involving the exploration of novel compounds or chemical relationships are less prevalent in the scientific literature, produce more unexpected findings, and have a greater risk of being ignored—but also a greater likelihood of achieving scientific appreciation and importance, as indicated by both citations and prizes. Moreover, researchers crowd around popular and important compounds and very rarely connect ones that are distant or entirely disconnected in the network of previous research. While this strategy may be productive for uncovering early connections in the network of knowledge, our work suggests that more individually uncertain approaches, which connect disparate components in the network of chemistry would provide greater total benefits for science.

This research suggests why unexpected findings that change the landscape of science are so infrequent: they involve substantial risks that scientists may not be able to afford. More should be done to encourage scientists to investigate new entities and new relationships that have transformative potential. Such work is costly to researchers. It is harder to identify a new association than to dig more deeply into a known one. Identifying new relationships often requires multiple attempts before success. Even when discovered, the reward for surprising findings can be lower than for expected ones. These risks may not currently be balanced by individual rewards, even if the overall benefits to science outweigh the costs. My discussion will consider these and other findings in light of possible institutional barriers to the performance of "transformational science," families of alternative research strategies, and some of the benefits that could be reaped if barriers were overcome.

Bridging the ideal and the policy senses of transformative research

James P. Collins
Arizona State University

Transformative research (TR) is currently used in two ways: an ideal or basic sense referring to how science is practiced or the fruits of that practice, and a policy sense in which proponents call for studies fundamentally different from preceding efforts and therefore deemed worth investing in as high-risk, potentially high-reward efforts. Arguments regarding how science is practiced, explained, and supported will likely include both senses of TR for the immediate future, suggesting the need for a vision and means for bridging the two ways in which the term is used.

Questions at the center of this workshop reflect mainly TR's ideal or basic sense; for example: What counts as TR? Is it possible to identify TR metrics? Alternatively, policymakers use the term in a more practical sense as part of an argument exhorting agencies to take more risks in choosing potentially transformative research proposals to fund. It is argued that failing to take the risk associated with funding such proposals means that some of the very best, "transformative" ideas go unsupported.

A bridging argument between the two senses could succeed if centered on conditions likely to increase the probability of yielding transformative results as opposed to a focus mainly on outcomes. Three elements of such an argument could include the following. 1) How transformative science might be practiced. As one example, we are coming to understand how modern social media can overcome the limiting features of a particular research environment and provide a means for investigators to escape a failure of innovation trap by embracing much wider communities of practice. 2) How transformative institutions are designed. Places likely to yield TR will have qualities such as a low barrier to movement of ideas and methods across areas of investigation, along with a physical space designed to increase the likelihood of creative contacts. 3) How transformative funding organizations are managed. Funding agencies typically want a return on investment, but it is widely recognized that research often progresses under conditions that allow and even accelerate the rate of failure. That is not to say investigators set out to fail; rather, multiple avenues are tried before achieving success. Funding organizations must accept that not everything will work; failure and moving past it is an integral part of a process that might ultimately yield a transformative breakthrough.

A counterargument to either sense of TR is that the process of discovery is an exercise in the careful execution of many well planned steps that over time eventually yield breakthroughs. There is a danger that the rhetoric of TR devalues this approach. It is here, however, that the uncertainty attendant to a precise TR definition can be beneficial as it places the burden of identifying leading edge research on skilled science managers who can discriminate among proposed TR that is unlikely to succeed, accumulative research that is just plowing the same old ground, research presented as TR that is likely to succeed, and research reflecting continuous progress on a tough problem requiring years of work to yield a transformative breakthrough.

A bridge between the two senses of TR could succeed with elements that reorient the focus of the discussion from a particular outcome that is a fundamental advance to features of how science is practiced, research institutions developed, and the scientific enterprise managed. In other words, on conditions most likely to enhance the process of discovery in ways that increase the likelihood of transformative results as opposed to just a concern about whether the outcomes of a particular project will be transformative or not.

What is transformative research? How does it relate to responsible innovation?

Michael E. Gorman
University of Virginia

Transformative research in science is revolutionary, in Kuhn's sense – which means it transforms current research paradigms. Transformative research is not merely at variance with such paradigms because all sorts of idiotic ideas are also at variance. Transformative research not only accounts for what is known or can be done in an area of science and/or engineering, it identifies and solves new problems that cannot be handled effectively by known concepts and practices. As it says on the NSF web-site: *Transformative research involves ideas, discoveries, or tools that radically change our understanding of an important existing scientific or engineering concept or educational practice or leads to the creation of a new paradigm or field of science, engineering, or education. Such research challenges current understanding or provides pathways to new frontiers.*

The NSF also has criteria for broader impacts that, it is now proposed, should be shifted to more economic criteria rather than the original list. This kind of work that benefits society relates to the idea of responsible innovation which involves not only being safe and ethical within known parameters but also thinking about the futures we might evolve as we push scientific and technological frontiers. How can we maximize benefit and minimize harm for future generations?

Responsible innovation of this future-oriented sort will require transformative research and thinking. Consider, for example, bio, info, nano, cognitive and robotics technologies that could transform what it means to be human. We need transformative research on how we can work together to imagine these futures and collectively manage their possibilities. Elsewhere I and others have outlined mechanisms for doing this (see Gorman, 2010; Allenby & Sarewitz, 2011).

Bottom-line: we need not only transformative research in science and engineering; we also need the reflexive capability to manage transformative research without killing creativity.

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An Early Examination of Peer Review, Innovative Research, and Grant-Giving

Mark Rothenberg
NSF

In November 1977 the National Science Board, in response to a Congressional recommendation, conducted a study on peer review procedures at the NSF. Among the issues considered was NSF support of innovative research. Congress had challenged the NSB to evaluate whether the NSF's review process ensured the funding of innovation research. However, the term "innovative research" was never defined by Congress or the NSB. The list of innovations the NSB developed was based on a survey of scientists in each discipline, with the definition of the term left to the scientists to decide for themselves, but in the context of the discussion (and given the list developed of innovative discoveries) innovative research was clearly analogous to what observers today would identify as transformative research.

The NSB came to a number of very important conclusions about the nature of innovative research. Over half of the innovations were unanticipated by the granting agency funding the research. The breakthroughs were either completely serendipitous or were not part of the research being funded. To the extent researchers did claim that their research would be innovative, peer review was very good at anticipating the probability of the success of a proposal. Only in one instance was a research proposal rejected which later turned out to lead to a significant advance, and in that case it was an issue of methodology that led to the decline of the proposal. The NSB also pointed out that only a small percentage of all research activity truly results in an innovation. Given that the overwhelming majority of all research did not lead to innovation and the difficulty of anticipating where innovations would come from, the NSB was reluctant to recommend changes in the review system that might possibly encourage innovation. Instead, it called for the continuing support of "good" research, and argued that lowering the rejection rate would be an important factor in continuing innovation. The NSB concluded that the best way to ensure innovation was having a large and thriving scientific community, not to attempt to identify and target proposals which might lead to innovation.

Some almost four decades later, the NSB has taken a much different tack. It has asked the NSF to aggressively identify and fund potentially transformative research. And the NSF responded by modifying the merit review criteria. The issue I would like to raise is whether there has been any empirical evidence gathered over the last four decades that demonstrates that the conclusions the NSB drew in the 1970s is no longer valid. Can the NSF merit review system successfully identify potentially transformative research? Also, the NSF now claims that its support "commonly results in transformative advances within fields of science or engineering." Four decades ago the conclusion was that this research as rare. Has transformative research become so common? Or has this become a situation in which everyone is above average? Will the modification in the language for the merit review criteria truly lead to the funding of research which will lead to transformations, or will the change lead to changes in the language of proposals and reviews, but little else?

Transformative Research: Social and Ethical Implications

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HHMI and Northwestern University

Some thinking points:

1) Exploration versus exploitation.
Potentially transformative research (PTR) is clearly an explorative type of activity. However, in spite of their best efforts, federal funding agencies are still structured for the evaluation of exploitative types of activities.

2) Low success rates of funding.
The low success rate, especially for young investigators, of grant applications means that they need to focus almost entirely on exploitative-type activities, thus limiting their ability to engage in PTR.

3) Collaborations: "Parallel playing" versus "playing together"
It is widely accepted that collaboration is an effective route for more explorative types of research. Funding agencies have in fact been very supportive of (some would even say pushy about) collaborative research, especially interdisciplinary collaborations. However, much of those collaborations are not truly PTR because the work being done is more like an assembly line: I do this, then you do something else, then I do something else, etc., than like an attempt at true discovery of what the collaborators can do together.

The challenge of course is that discovering what we can do together is quite difficult and time consuming. In my own experience it requires a sort of "parallel playing" period before true discovery can occur. This process can take years to come to fruition and it is not something for which you could even apply for funding; at the start, you do not know what you are going to be doing.

Transformative Research: Inchoate Thoughts about an Incoherent Concept

Dan Sarewitz
Arizona State University

1. So I'll stipulate (perhaps controversially) that funding billions of dollars of research that is uncreative, uninteresting, unoriginal, and offers little if any prospect for actually adding anything worth knowing to the knowledge corpus is a waste of money, time, and effort. Does that mean that we need more "transformative" research? Is "transformative" research something that's actually possible to do consciously and explicitly? Our heroic myths of Einstein, Faraday, Newton, Copernicus, etc., notwithstanding, we know that a lot of what makes change possible in science is the presence of an increasingly unsatisfying

body of theory and explanation that actually opens up space for science to begin to move in new directions; thus making it possible for Einsteins, Faradays, and others of that incredibly rarified crowd to do their thing (I guess). So "transformation" may be as much an emergent quality as one that can be consciously cultivated. But it's also worth noting that among other complaints about the state of science (that is, other than "it's not transformative enough") are also complaints that it's not conventionally "scientific" enough, especially that there's not enough science aimed at reproducing existing results, and that there's not enough reporting of science that fails to confirm hypotheses, because neither of those are considered worth publishing or granting tenure for or building careers on. Yet the concepts of "confirmative" and "refutative" science are sort of the opposite of "transformative," aren't they?

2. The thing about "transformation" is that organized systems are organized to resist it, whether it's a knowledge system, a technological system, a political or cultural system. Transformations may come when the logic of the current organization is no longer supportable (Kuhn and paradigm shifts, e.g., etc.), or when a change, or novel new opportunity, comes from the outside that the system cannot respond/adjust to. The energy system is hard to change because the electrons delivered to my desk by carbon-belching power plants are just as good as the ones delivered by much more expensive clean solar panels. Personal computers, the internet, GPS, cell phones, automobiles, steam engines, stirrups, blah-blah-blah, were transformative because they were so much better than what the existing system had to offer; or because they offered something of which the existing system hadn't even thought.

3. Why the obsession with transformation? Not to oversimplify or anything, but I take it that one reason is our belief that "transformative" science will lead to "transformative" technology (cheap solar panels?) and thus create our next "economic" or "industrial" or "technological" revolution, which we need because otherwise China will eat our lunch. (As a matter of technological history, the link between transformative science and transformative technology seems mostly wrong to me, or at least fabulously incomplete – but that's a different point.) It seems both interesting and obvious that the commitment to radical technological transformation of society that has been central to the logic of market economies for the past couple of centuries (and seems somehow connected to the quest for "transformational" science, and to the overheated rhetoric of scientific hype in the past 50 years or so) is also a commitment to radical and wrenching social transformation – transformation that leads not just to wealth creation and marvelous new conveniences, but to the destruction of entire sectors of the economy and entire ways of life. If you're dug into the current system, then transformation is often bad for you. This, by the way, is not necessarily a minority position. You can't isolate the incredible concentration of wealth in this country over the past 50 years and the hollowing out of the middle class from technological change. Perhaps with a different set of social and economic policies we could have had technological and economic transformations and also maintained jobs and better wealth equity and health access etc., but that's a trick no political economy seems ever to have figured out how to pull off since the Luddites first smashed the power looms. But in any case, I wonder if talking about transformation brings with it an ethical obligation to

talk as well about managing the consequences of transformation in ways that are just, equitable, and wise.

Normal Science and Innovation

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According to the National Science Board (NSB) of the National Science Foundation (NSF), transformative research is "research that has the capacity to revolutionize existing fields, create new subfields, cause paradigm shifts, support discovery, and lead to radically new technologies" (National Science Board, 2007). Transformative is one of the new terms invented in the last few decades to get away from 'pure' science (and its variants: fundamental, basic), a category no longer used because few people believe in its existence or relevance (Godin, 2003):

Pure, fundamental, basic → (mission-) oriented, strategic → transformative

Today, every organization has its own similar label. The National Institute of Health (NIH) talks of "translational" research, the OECD of "blue sky" research, the European Research Council of "frontier" research. A new label is essentially a semantic innovation introduced to emphasize a new idea and catch the attention. Semantic innovation is not limited to public organizations. Social researchers have their own labels too: "mode 2" is certainly the most popular label invented in recent years to name a (supposedly) new mode of knowledge production.

If one thinks a bit, he will observe that transformative research is **nothing else than innovation** (innovative research). Why not simply use that word? It may have to do with the fact that innovation has had, for most of its history, a pejorative meaning and that today it is industrially connoted (Godin, 2011). The technological and commercial representation – a spontaneous representation because hegemonic – involves a 'bias' that most academic researchers do not accept – at least publicly.[2] Yet, for 2,500 years innovation covered anything which is new, and etymologically innovation is precisely what transformative research is.

Innovation is a word of Greek origin (καινοτομία), used in Antiquity for talking about changes in the political and constitutional cycles. When the word got into our everyday vocabulary, namely after the Reformation, it meant 'introducing change to the established order' (religious and political). Such a meaning was pejoratively connoted. After the English

[2] Although the NSF, as organization, has been active on studying innovation from its very beginning (see Appendix).

revolution of 1649, then the French revolution of 1789, innovation got still more negatively connoted when it got associated to revolution: revolution became the emblematic example of innovation. What is a revolution? A revolution is a radical and disruptive change – a 'transformative' change! Innovation still has this revolutionary meaning today, but in a positive sense. To the theorists and the statistical mind a technological innovation is necessarily revolutionary (for its (measurable) impacts on the economy) – although incremental changes are increasingly admitted as innovation too.

In this context (and semantics), what is innovative research? Innovative research is research which is radically new on the following elements:

- Object
- Hypothesis
- Framework
- Method
- Approach (like multidisciplinarity and reflexivity in social sciences and humanities)
- Impacts (scientific and socio-economic)

What are the implications of innovative research so defined? Let's limit the discussion to policy (there are more implications discussed in Godin and Lane, 201?). Policy needs categories for action. I suggest that, in place of the previous categories (basic and applied) we shift to the following two: normal science and innovation. Researchers would have to decide to which category they submit their proposal. But beware: normal science would have a very small pot of money and innovation would have high criteria: if the NSF is serious about transformative research, it should fund projects that are innovative on ALL of the elements above. This is certainly a huge demand put on the researchers (but possible, believe me). Yet, it is certainly a way to 'clean' the publications market and reduce the (voluminous) number of minor works no one reads. To be sure, normal science has a place in the research system, but not most of the place as it actually has. If research is to be transformative, it has to be innovative – innovative on all fronts: scientific, technological [3] and socio-economic, and the latter should have equal weight to the other five together. In order to meet the socio-economic objective the researcher would have to include a specific and concrete plan for development or application in his proposal – depending of course on the stage of development of the research.

It remains the question of who evaluates the proposals in order that the NSF get real innovation research. Given the conservatism of the peer review system, one needs an appropriate mechanism. I suggest that, as a counterpart to getting a large grant, the

[3] In a large sense: a good, a method, a protocol, a policy or law, a service; briefly stated anything that is 'useful' to society.

innovation grantees should be asked to evaluate the proposals during the time of their funded project.

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Identifying Potentially Transformative Research

Gregory J. Feist
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Understanding how transformative science has been funded can help inform and improve future funding decisions. The stakeholders are not only those who desire a good return on their investment in science, but every person who lives in a world that can be constantly changed by the next great idea.

Sometimes a scientific work is important because it provides new methods, or tools, or techniques, sometimes because it is the necessary logical extension of what came before, and sometimes because it can serve some real and immediate good. There is also that science which is important because it is revolutionary, because it fundamentally transforms an existing field or serves as the foundation for an entirely new one. It is this transformative science that is our present focus.

Identifying transformative work is a significant challenge. Even an expert may not be able to immediately identify important work without the benefit of historical context. While this would appear to argue for only considering older work which already has a well established place in the history of science, that advantage has to be weighed against the benefit of providing more current information. Presumably information about work that is closer to the present day would be more relevant and useful to a contemporary decision maker. For this reason we will choose to rely on imperfect metrics to provide us with something akin to a first draft of the history of science.

One way to approach the problem of transformative research and whether funding agencies have been able to identify these ideas a priori is to examine whether very high impact papers have received federal funding, private funding, or no funding. A graduate student (Barrett Anderson) and I are just beginning a study in which we will be looking at the most highly cited papers that have been published in the last five years. Specifically we will be looking at papers that are more highly cited than their peers, defining peers as other papers published in the same field, at or around the same time and then code these papers on the type of funding they received. We would limit this question to U.S. papers and U.S. funding agencies.

A Crucial Issue in the Discussion of Transformative Research

Juan Rogers
Georgia Tech

The crucial issue on "transformative research" (TR) is whether such a thing can be said to exist in any recognizable way. It seems that the main motivation for focusing the peer review of proposals with such a category is a perception that panels of peer experts tend to be conservative in their willingness to rate highly project proposals with novel ideas that will inherently come with high uncertainty on what they will deliver over the period of the grant. TR is then not so much an intrinsic attribute of a certain type of research, but a label for a subset of the proposals that would come in for review under the normal business of grant cycles that may be re-classified for funding if the peer review panels were able to better recognize the novelty of the ideas in them and the potential for them to make a difference if funded by improving on their ability to reduce uncertainty about their results. If this is true, it has more to do with diagnosing the ability of peer review panels to recognize novelty and potential for change in the face of high uncertainty.

The argument is sometimes made that the known inclination of peer review panels to favor, so called, incremental research, which by implication does not include much novelty and only improves upon past science in small ways, discourages researchers from even proposing projects with novel ideas with high realization uncertain. So the mere fact of brining attention to this problem and requiring panels to include a TR criterion in their judgment of proposals will encourage more submissions of so called "high-risk, high-reward" research proposals.

But, why haven't panels been able to recognize this species of project proposals before? The commonplace answer is that they are protecting the scarce resources of public funding for research and do not want to expose the funding sources to the risk of losses. But then the ability to recognize novelty itself is not really called into question. The willingness to accept risk is. Unless we come up with a way to reduce the inherent uncertainty of carrying out projects based on highly novel ideas, the level of risk itself is not going to change. And if the level of risk doesn't change, given a certain judgment of novelty by panels with essentially the same abilities they have now, it all boils down to committing funding accepting a higher rate of failure in the expectation that the fewer successes will be so much more rewarding that they outweigh the losses. If the higher risk is real, the higher rate of failure must follow.

If this is not the case, then what is being talked about is to reduce the uncertainty somehow without reducing the value of the research results. But this is impossible without reducing the novelty too. It seems that there is faulty logic in the formulation of the problem of TR and lack of clarity on what the target of this category might be.

The Necessarily Proactionary Nature of Transformative Research

Steve Fuller
University of Warwick

TR originated as a response by elite scientists who claimed that they were unable to get exploratory research funded at a time when the NSF was stressing 'broader impacts', which seemed to bias research evaluation towards projects capable of showing social benefits in the relative short term. One way out of this problem is to treat TR as an application of the 'proactionary principle', which stresses the need not to miss opportunities over the need to avoid error or harm (as in its evil twin, the 'precautionary principle'). However, to be truly proactionary, TR must not only be opening up epistemic frontiers that might otherwise remain closed but it must also open up the minds of the public to such frontier-seeking work. In this respect, the prospect of new knowledge requires the prospect of new knowers. (A statement of proactionary principle may be found here: http://www.maxmore.com/proactionary.htm. It is the subject of a book I'm writing with Veronika Lipinska for Palgrave Macmillan.)

At the most basic level, this may mean that TR proposals should include provision for the deployment of focus groups, scenarios and wiki-media on the public to trail potential benefits and inoculate against potential harms (what the NSF's Nanotechnology and Society Program sometimes calls 'anticipatory governance'). However, one might also encourage a more thorough enfranchisement of the public in TR as subjects, data collectors, self-experimenters and even collaborative theorists. The public is more likely to embrace high-risk/high-reward research if their fates appear to be joined with those of the scientists seeking their funding. (This could be part of a larger political strategy to inculcate scientific citizenship on the model of national service.)

Perhaps the biggest challenge facing a fully proactionary sense of TR is getting people to accept failure and harm as a short-to-medium term cost for substantial long term benefit. At the very least, a re-invigorated welfare state would be needed to insure the public against the risks that they would now be encouraged to take. In addition, an education and media – some might say ‘propaganda’ -- infrastructure needs to be set in place to integrate this objective into people’s ordinary self-understanding. Here some useful lessons can be learned about what (not) to do from the Soviet Union, which harnessed the fate of its entire society to a particular version of TR. The main lesson for TR from this experience is less that people should not be sold risky, speculative research (e.g. Lysenkoism) on a dodgy empirical basis than that mechanisms need to be in place to catch error and failure before they contaminate the entire knowledge system – and destroy even more people’s lives. (One area from the workshop where this idea might be piloted is ‘edge governance’ of DIY Biology, as presented by David Rejeski.)

Transformative Research Can Have Transformative Broader Impacts

Mark S. Frankel
AAAS

NSF observes that “History shows that it is difficult to predict which research projects will result in transformative results before the research is conducted and the scientific community has assimilated its findings.”
(http://nsf.gov/about/transformative_research/challenges.jsp)

One can extrapolate from this to take a position that identifying broader impacts based on such research may be even harder to predict! This raises a number of challenging issues for the research community and policy makers, among which are the following:

1. How should we think about the broader impacts of transformative research, especially since its “transformative nature and utility might not be recognized until years later”?
2. What methods and strategies can help to anticipate, characterize and assess the impacts of such research?
3. What are the value assumptions underlying transformative research, and how do they affect choices of methods, perspectives included, and the interpretation of findings?
4. What does transformative research have to say about the role of science and scientists as change agents, especially with regard to social responsibilities?
5. If “NSF supports and encourages” transformative research that involves “high risks,” what expectations should the public have about the responsibilities of researchers?

This is a subset of what I expect will be a longer list of issues considered at the workshop. Thinking about how to answer them forces us to consider perspective, which is critical to how people interpret and evaluate data, context, and “findings,” and what informs people’s opinions about problems, and their willingness to consider and assess alternative solutions. The judgments that people make about how science should be defined, practiced and applied are influenced by their perspectives. This poses a challenge for any effort to consider the social and ethical implications of transformative research—how to capture the rich experience, knowledge, fears and hopes that diverse perspectives would bring to our deliberations.

Confronting this challenge is critical for at least two reasons. First, social justice requires that, in light of the potential for *transformative* change, the views and interests of all stakeholders must be taken into account. Second, a valuable contribution of multiple perspectives is the "correction" it can make to our science, in that it can help minimize distortions caused by too narrow a perception of the problem, its causes, or of the range of possible solutions. We must take roll at the workshop, not merely of individuals present, but, more importantly, of the different perspectives that will influence our work, either by their presence or absence.

Linking transformative research to broader policy goals in the EU context

William Cannell
European Commission

The European Union has become an increasingly significant player in European research funding over the last decades. The most noticeable recent development has been a rapid increase in funding for investigator-driven frontier research, which has grown from virtually zero in the year 2000 to around €1.5bn pa in 2012 via programmes such as New and Emerging Science and Technology (NEST) and latterly the European Research Council (ERC). This disproportionate growth, as compared to more targeted and strategic (society or industry-driven) funding, is set to continue in the next budgetary period, up to 2020.

Various ideas and mechanisms associated with what the EU calls "frontier" research are strongly redolent of NSF's concept of "transformational" research. Policy rationales appeal to the need to anticipate and catch the waves of radical technology change that will form the markets of tomorrow; programme objectives emphasise high gain/high risk research at the interface between disciplines; peer review mechanisms are explicitly designed to favour such research in a context of very high demand for funds and correspondingly low success rates.
The salience of these ideas in EU research and innovation policy is linked to a number of broader dimensions of the policy discourse which in some respects are peculiarly European, for example:

- One of the core economic justifications for EU research investment, which is the relative weaknesses in Europe's science base as compared with the apparently superior capacity of the United States, particularly in emerging, fast-moving and high-impact research. This "weak science" rationale for European economic performance has supplanted the earlier, and almost polar opposite, argument of the "European paradox" – that Europe was unable to translate its good science into innovation.
- The perceived need to unblock rigidities – bureaucratic and cultural - in Europe's national research systems, which in many cases are organised on highly traditional

and hierarchical principles. EU research programmes are designed to add value to, and therefore differentiate themselves from, national programmes which operate in parallel. The "transformational" attribute of EU programmes is one such declared differentiator, and in such environments, they are seen as offering an alternative pathway for highly talented younger researchers to bypass career blockages and thereby also impose strong pressure towards structural reform.

- The "federating" role of EU research, vis-à-vis national programmes. An important idea here is that, by comparison to the fragmented national programmes, EU research funding – in particular via the ERC – has a greater "liquidity", which enables the rapid build-up of support to promising new (and "hot") areas through "bottom-up", investigator-driven research projects, selected on the basis of their potential scientific impact via high quality and risk-tolerant peer review.
- A further appeal to the transformational character of frontier research in the European context is what one could call the "transformational demand". The EU's sectoral policy objectives are presented increasingly in terms of large-scale "societal challenges" – like food security, climate change and healthy aging – which, in their complex and interlinked character somehow mirror the appeal for multidisciplinarity in research policy. Such challenges are seen as so acute and intractable that "transformations" – for example of institutions, markets, technologies and behaviour patterns – will be needed to resolve them. In turn, this imposes a demand for "transformational" research.

A conclusion could be offered that a certain construction of frontier research, corresponding at least in part to the "transformational" research paradigm, has helped to form – perhaps in co-evolutionary mode – and is intrinsic to, a broader dynamic of research policy and indeed of policy more generally in the EU. The various ideas associated with "transformational" research in the EU context do not necessarily imply a well-constructed or consistent philosophy. However, they do signal both a strong interest in improving the productivity of research, in the sense of the creation of novelty, and important connections between the concept of transformational research and the desire to foster broader structural changes in the EU economic and social landscape. In both cases, the concern for promoting transformational research seems to be based on an assumed link to radical innovation.

Outside the realm of the EU, these observations suggest a number of conceptual and practical questions about the transformational research paradigm, for example:

- To what extent has the notion of "transformational" research entered the mainstream, i.e., become "the new normal"?
- If the language of transformation permeates research funding programmes across the board is this simply a rhetorical device, or does it suggest a conviction that (e.g.) high risk-high gain funding is a generally applicable route to greater research productivity and impact?
- If, on the other hand, "transformational" research is considered as necessarily a niche venture, does that mean it is inevitably defined in terms of its distance from the norm (the parameters of which may themselves be changing over time)?

- In that case, what is the correct "dosage" of transformational and "normal" research in a funding system?

Transformative Research: Chinese Perspectives

Nan WANG
Graduate University of Chinese Academy of Sciences

As an emerging agenda in last decade, transformative research has come to play an increasingly significant role in policy and practice in the Chinese development of science & technology. Recent actions by National Natural Science Foundation of China (NSFC) and Chinese Academy of Sciences (CAS) illustrate the point:

- The NSFC put forward the concept of "non-common understanding project" in 1993. *Tonghang pingyi fangfalun* [The methodology of peer review], the first to use the phrase "peer review" as its title in China, was published three years later in 1996 after a special NSFC research project as initial step in transformative research. The suggestions on non-common understanding project in this book were implemented at the beginning of the 21st century. (The term *pinyin* is difficult to render into English. "Non-common understanding" refers to projects that do not have strong peer review support but are judged by one or more program officers to nevertheless be worthy of support. In English such projects might be described as low probability of success but of potentially high benefit.)
- In a 2000 document on "Regulations on the National Natural Science Funds" NSFC specified a regulation on "real-name recommendation system" as regards to projects on which most evaluation experts argue against funding. But this regulation involves many innovations. For instance, a meeting-based evaluation may be performed if two evaluation experts that participate in the meeting-based evaluation have signed a recommendation for it.
- One year later NSFC created a "Small and Exploratory/Developmental Research Grants" program, which is a one-year suggestive small fund for a high exploratory and risky application. It is designed for new ideas have never been approved before or the probing of newly developing cross-field subjects, for young applicants with new ideas who lack funds to initiate their research, or for applicants who want to change their direction or have a pressing need for funds.
- In 2011 NSFC struggled to implement experimentally the "Major Non-common Understanding project" in some subject areas. It also adopted a series of special policies on transformative research in its 12th Five-year Program (2011-2015). This document states that NSFC will seriously support transformative research, encourage scientists to create ideas and practice transformative innovation. The fund size and intensity will be determined according to the situation of applications and approved projects, making the best use of academic judgments from the

relevant scientific departments and gradually establishing a special mechanism of review and management for high risk, exploratory projects.

- CAS also places great importance to transformative research. Bai Chunli (CAS President since March 2011) gave a number of public talks on transformative research after assuming office. According to his analysis of developments and the tendencies in modern science & technology, material science research will be the leading edge and full of opportunities for innovation. Transformative breakthroughs in this field will play an important role in scientific, technological, and economic development. Based on a historical view of transformative breakthroughs in materials science research, Bai identified four areas for future CAS support: quantum size, nano scale, macro scale, and unknown scale.

Basically, transformative research has been judged important in the policy and academic fields. Both NSFC and CAS have worked to create relevant policies to support transformative research and turn it into realistic practice in scientific and technological research. But there is a lot of responsibility and a long way to go in this regard. It is necessary for China in the future to further clearly defined the concept of transformative research for the public, set up a advantageous environment for scientists to work, develop more measures to encourage scientists to submit innovative ideas, and adopt more flexible mechanisms for the support of transformative research. Discussions from this workshop may contribute to a mutually beneficial dialogue on these and related issues.

Transformative Governance: Is It Possible?

David Rejeski
Woodrow Wilson Center

Sometimes the results of transformative research hit us between the eyes, but not often. Here is why. When disruptive technologies appear, they often perform at a level that is actually below what is already on the market. This is exactly what makes it difficult to perceive their potential. Think about digital photography versus film; e-commerce versus bricks-and-mortar retailing; or classroom education versus internet-based, distance learning – all greeted with yawns and skepticism. But these disruptive technologies created new market opportunities, especially for people focused on higher performance options, and that is what drove their adoption. Sometimes disruption is subtle because it enables indirect changes in other technologies (like the 3 ½" floppy drive enabled laptop computing) or because the technology changes who has access to innovative capacity (like 3-D printing and open-source hardware puts manufacturing capacity on a desktop just about anywhere).

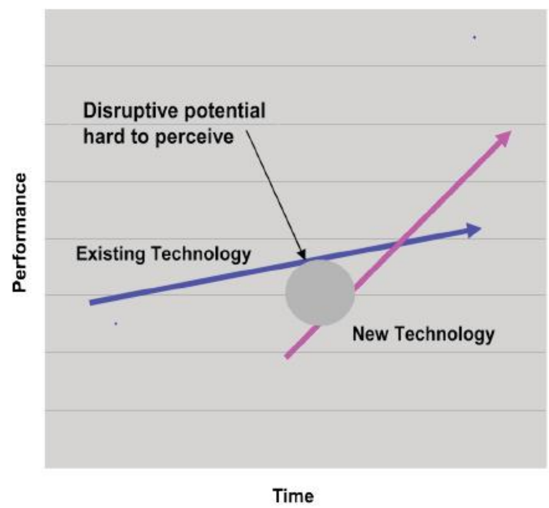

Figure 1

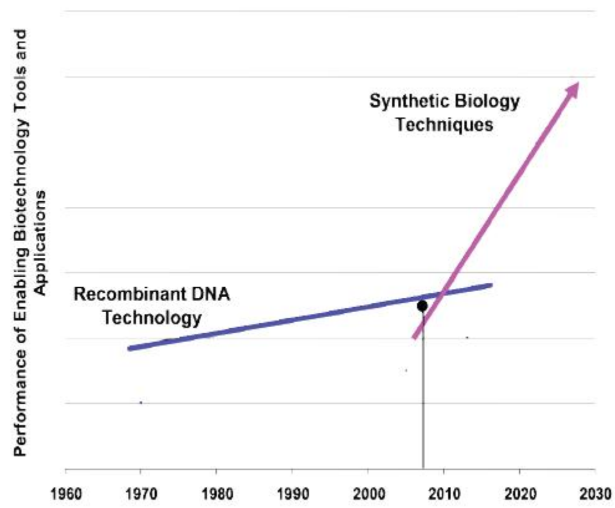

Figure 2

The strategic inflection point occurs sometime after the introduction of the new idea but before its advantages are obvious or market-tested – often upstream in the research phase. The new technology does not replace the old; it provides new capabilities. Schematically, this is represented in Figure 1 (based on the work of Clayton Christensen at Harvard).[4] Figure 2 appeared in a 2007 Department of Energy-supported study on synthetic biology and shows the anticipated performance increase of the enabling tools of synthetic biology compared to traditional recombinant DNA techniques.[5] Today, it has become more obvious that synthetic biology is a game-changer, but it wasn't that obvious in 2007, and few people were thinking about the social, ethical, environmental, or legal challenges of synthetically-engineered life forms.

All of this may look like interesting management theory, but disruptive shifts in technologies can have larger implications for governance. Rapid technological change often leaves risk assessment catching up with the risks, outstrips the ability of governments to provide adequate oversight, an dleaves little time for democratic deliberation and public dialogue about emerging ethical an dsocial issues. As Charles Fine at MIT's Sloan School has pointed out, when the "clockspeed" of government falls far behind the research and technology curves, public policies can either become irrelevant or badly designed as policymakers rush to close the governance gap.[6] Andy Grove, the former CEO of Intel, put it this way: "High tech runs three times faster than normal businesses. And the government runs three times slower than normal businesses. So we have a nine

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[4] Christensen, Clayton. 1997. *The Innovator's Dilemma.* New York, NY: Harper Business.
[5] Bio-ERA. 2007. "Genome Synthesis and Design Futures: Implications for the U.S. Economy." Cambridge, MA: Bio Economic Research Associates, p. 38.
[6] Fine, Charles. 1998. *Clockspeed: Winning Industry Control in the Age of Temporary Advantage*. New York, NY: Perseus Books.

times gap."[7] Closing a gap that large is likely impossible, so it raises the question of whether disruptive innovation is possible in governance systems, not just technological ones, and, if so, what that might look like and who might be responsible for designing and implementing such changes.

One strategy would be what biologists call *persistent co-evolution,* in which the players in the policy system become part of a diverse, complex, and dynamic innovation ecosystem, not isolated observers sitting on some external bureaucratic perch. The goal is to prevent risks, not just study them; to encourage innovation, not just write about it; and to accelerate the introduction of new technologies into the marketplace, not to hinder it. This would require adaptive learning on the fly and continual experimentation with innovative governance approaches and organizational structures.[8]

Sadly, no one in government is responsible for transformational governance. It would require a type of *DARPA-for-governance* model, and we are far from that.

Transformative Change as a Qualified Good

Carl Mitcham
Colorado School of Mines

Since January 2008 the NSF merit review criterion has asked "To what extent does the proposed activity suggest and explore creative, original, or potentially transformative concepts?" To be transformative is assumed to be a plus for research proposals; there is no apparent qualification of this element in the criterion. In its 2007 report, *Enhancing Support of Transformative Research at the National Science Foundation*, the National Science Board had previously defined transformative research in a similar unqualified manner as that which

> involves ideas, discoveries, or tools that radically change our understanding of an important existing scientific or engineering concept or educational practice or leads to the creation of a new paradigm or field of science, engineering, or education. Such research challenges current understanding or provides pathways to new frontiers.

Despite the implicit assumption (if not assertion) regarding transformative research as an unqualified good, it is reasonable to ask to what extent scientific transformations are in reality always good for science or for society.

Consider first the case of science. Although discovery and change is regularly praised and prized in research, any transformation necessarily takes place against some stable backdrop of normal science. According to Thomas Kuhn's analysis of scientific revolutions, for instance, a good part of the goodness of revolutions in paradigms is that they establish

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[7] Quoted in Cunningham, L. 2011. "Google's Schmidt Expounds on his Senate Testimony." *The Washington Post*, October 1.
[8] See Hoffman, A. 1991. "Testing the Red Queen Hypothesis." *Journal of Evolutionary Biology*, Volume 4, pp. 1-7.

new normalities in science, which can then be pursued in a non-transformative manner. If science were nothing but transformations it would be chaotic.

There have also clearly been instances in which desires to produce transformation have taken science off on the wrong track. Examples would include Martin Fleischmann and Stanley Pons and their claims to cold fusion (1989), Jan Hendrik Schön's semi-conductor research and Victor Ninov's announcement of element 118 (both in 1999), and Hwang Woo-suk's stem cell research (2004). The fact that these false claims in chemistry, physics, and biology to having produced ideas and discoveries that radically change our understanding of important existing scientific concepts in ways that provide pathways to new frontiers were initially so readily accepted by the scientific community should raise cautionary concerns about an over emphasis on the value of transformative research. Could it be that too strong a dedication to transformation has led both scientists and scientific journals to be too quick to cut corners and contributed to misconduct in science?

Rapid or radical change in science may also be questioned insofar as it tends to intensify inequities in science and/or in the social worlds in which science is embedded. As has often been observed, leading research institutions such as MIT tend to win a disproportionate share of grant proposals, a disproportion that can only be expected to increase under guidance from the transformational research criterion. Additionally, transformative research is often presented as way for the United States to maintain competitive advantage in the global scientific community. Clearly transformational research has the potential to increase gaps between the scientific haves and have nots, between the scientifically rich and poor.

Insofar as we recognize that transformation is not an unqualified good in science, it also becomes incumbent to inquire whether there are ways to distinguish good transformations from non-good transformations. This is a challenge that has so far been largely neglected in all the praise for transformative research.

Consider second the case of society. In society even more than science inequity presents a fundamental challenge and stability is conceived as a fundamental good. The social challenge of the pursuit of transformational science is suggestively hinted at in Daniel Bell's analysis of *The Cultural Contradictions of Capitalism* (1976; 20th anniversary edition 1996). Although Bell does not directly address the relation between science and society, it is easy to draw related implications from his effort to call attention to social contradictions in the relationships between economics, politics, and culture. As Bell summarizes his problematic position in a new preface to the paperback edition, he is "a socialist in economics, a liberal in politics, and a conservative in culture." Elaborating, he writes:
(1) I am a socialist in economics. For me, socialism is not statism, or the collective ownership of the means of production. It is a judgment on the priorities of economic policy. I believe that in this realm, the community takes precedence over the individual. (2) I am a liberal in politics — defining both terms in the Kantian sense. I am a liberal in that, within the polity, I believe the individual should be the primary actor, not the group. And the polity has to maintain the distinction between the public and the private. (3) I am a conservative in culture because I respect tradition; I believe in reasoned judgments of good

and bad about the qualities of a work of art. I use the term culture to mean less than the anthropological catchall and more than the aristocratic tradition which restricts culture to refinement and to the high arts. Culture, for me, is the effort to provide a coherent set of answers to the existential predicaments that confront all human beings. (Bell 1979, pp. xii, xiv, xv.)

Adapting a table from Malcolm Waters's critical intellectual biography *Daniel Bell* (1996, p. 35) the tensions analyzed by Bell may be summarized as follows:

Realm	**Techno-economic (social) structure**	**Politics**	**Culture**
Axial principle	Functional rationality	Equality	Self-realization
Axial structure	Bureaucracy	Representative government	Creation and reproduction of meanings and artifacts
Central value-orientation	Material growth	Consent of the governed	Self-expression, novelty, and originality
Relationship of the individual to the social order	Role differentiation	Participation	Individualism
Basic processes	Specialization and substitution	Bargaining and legal representation	Disruption of genres by syncretism
Structural dangers	Reification	Entitlements, meritocracy, and centralization	Postmodernist anti-nomianism

[As an aside, in his critical assessment Waters argues that Bell is not really much of a socialist or liberal; instead, he is really just a traditionalist conservative (instead of the neoconservative he is sometimes called). "Despite all interest in the future possibilities of technology and post-industrialism Bell is an old-fashioned, traditionalistic, elitist conservative" (Waters 1996, p. 169).]

Setting aside debate about Bell's own position, his basic point can be described as having identified the existence of fundamental tensions between different aspects of a structurally differentiated social order — that is, a social order in which different aspects of culture in the anthropological sense have become disaggregated from one another and been granted relative autonomy or independence: science separated from religion (Galileo Galilei case), economics from politics (Adam Smith), religion from politics (*U.S. Constitution*, Amendment one), art from religion and politics (*art pour l'art*), science from politics (Robert Merton and Vannevar Bush), and more. Insofar as different realms of culture operate by and manifest different principles, tensions cannot help but build up in a social order. Charles Taylor's analysis of *A Secular Age* (2007) as demanding individual choice among alternative religious beliefs applies *mutatis mutandis* to culture as a whole. Human beings have to choose whether they are going to adopt the axial principles of functional efficiency, equality, or self expression as the primary foundations for their behavior; to some extent pluralism is not an option.

Extending Bell's analysis it is possible to construct a column for a fourth axial sphere of science in which the axial principle is transformative research; the axial structure, publication of results; the central-value orientation, laboratory collaboration; the relationship of individual to the social order, elitist meritocracy; basic processes, extending knowledge; and structural dangers, misconduct. There are clear tensions between this social realm and that of politics, one that can only be exacerbated by an excessive focus on transformative research.

Another way of reflecting on this tension is to inquire into the value of stability. Just as transformative change can be taken as a fundamental value in scientific research, social stability can be seen as fundamental to a social order. Regularly citizens and states public express desires for domestic stability (and against social or political revolution); the most common aim of foreign affairs is to stabilize international orders. War is always seen as an option of last resort.

There are, of course, exceptions to this principle. In some cases such as those in Tunisia, Egypt, and Libya during the "Arab Spring of 2011," domestic injustice had become so intolerable that the public deemed revolution preferable to continued maintenance of the status quo. But the point is that only in the extreme case is transformative politics justified.

By cultivating a taste for transformation, is it possible that science and transformative research could lower the bar for social transformation that would be inimical to social order? At first glance, such a question would seem answered in the negative by the fact that the most scientifically advanced societies also seem to be among the more stable. Yet this stability may be less deep that it appears. For instance, the challenge of what William Fielding Ogburn (1922) termed "cultural lag" can be argued to introduce into the U.S. culture a kind of disorienting uneasiness and dissatisfaction.

The value of political stability, of the rejection of transformation and change as unqualified goods, rests ultimately with the argument that social or political (or other kinds of) change are not the best foundations for the pursuit of the highest good for humans.

The upshot of these brief critical reflections on the extent to which transformative research is in reality always good for science or for society is to suggest that transformative research be re-conceived only as a qualified good. The argument here is that transformative change is a qualified rather than an unqualified good. This argument is, however, only a beginning — the putting forward of a hypothesis that calls for further analysis and (perhaps) transformative research.

Identifying Potentially Transformative Research: Peer Review and its Alternatives

J. Britt Holbrook
University of North Texas

Speaking of alternatives, I want to point out two alternative interpretations of my title. First, it could mean a choice between using peer review or some other way to identify Potentially Transformative Research (PTR). Under this first alternative, one might compare, say, NSF's Merit Review process with other funding mechanisms that bypass this process, such as NSF's EAGER and CREATIV funding mechanisms, each of which rely on the judgment of NSF staff rather than that of external peers. Another alternative to traditional peer review might be issuing challenges or offering prizes for results in a specific area (see, for instance, *A Strategy for American Innovation*, p. 12).

Second, it could mean a choice between using one form or another form of peer review. Under this second interpretation of my title, it could refer to using one form of peer review for most proposals, while using an alternative form of peer review (say, a "shadow panel" or an "Ideas Factory Sandpit") to identify PTR (NSF-10-27, p.p. 25-26). Or, it could refer to an alteration in the generic peer review process – for instance, adding language to the review criteria to encourage reviewers to consider PTR[9] – or even to a more substantial revision to the peer review process, which is something that NSF is about to undertake (Holbrook, 2012).

Once we really start to consider these alternatives, however, the notion of alternatives to peer review (in the sense of the first interpretation of my title) begins to slip from our grasp. For in what sense are the supposed alternatives to peer review mentioned in my first paragraph not themselves forms of peer review? Where do we draw the line between peer review and non-peer review? We find ourselves, I suggest, in a situation that parallels that of St. Augustine on 'time' – if no one asks us about it, we know; but if we want to explain it to someone, we know not.
Nevertheless, I want to make some bold claims in the face of my uncertainty. I will simply assert them here, though I hope to have time to offer some support for each of them at the workshop.

1. The notion of PTR is no more inherently incoherent than the notion of a peer.
2. One doesn't always *need* a strict definition of something (say, PTR, or broader impacts, or a peer) in order to identify it. (This is an application of the Rolling Stones Principle: You can't always get what you want; but sometimes, you get what you need.)
3. Making changes to peer review processes may in fact affect whether PTR is identified – but we need a way to figure that out. So, we need to develop metrics or other indicators of PTR.

[9] On September 24, 2007, NSF issued *Important Notice No. 130: Transformative Research*, which announced a change to NSF's Intellectual Merit Review Criterion effective January 5, 2008 (Bement, 2007). Reviewers would now be asked: "To what extent does the proposed activity suggest and explore creative, original, **or potentially transformative** concepts" (bold indicates the addition of 'potentially transformative' to the criterion)?

4. There is no way to identify PTR that escapes substantial reliance on peer review.
5. If we want to encourage PTR, then we ought to focus *less* on whether we can define PTR and whether peer review can actually identify PTR *ex ante* (which is redundant).
6. If we want to encourage PTR, then we ought to focus *more* on the kinds of things people who have produced what we now identify (*ex post*) as TR actually do – and we should ask proposers to do those things and reviewers to check to make sure those things are included in the proposal.
7. We ought to think more broadly about the notion of 'transformation' – in particular, we ought to consider the wisdom of thinking that transformation for the sake of transformation is an unqualified good. Put differently, we should think about the <u>broader impacts of PTR</u>, as well as the <u>potential transformativity of engaging in broader impacts</u> activities.