***Background Paper***

# Maximizing the Opportunity for Transformative Research: Key Factors in Identifying and Fostering Transformative Research

A Background Paper for the NSB-Sponsored Workshop on
Transformative Research

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Santa Fe Institute
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**Executive Summary**

The National Science Foundation (NSF, the Foundation) is an independent U.S. government agency responsible for advancing science and engineering in the U.S. The NSF does not operate any laboratories or research facilities; rather, the Foundation supports a broad base of research through grants and cooperative agreements awarded to individuals and research groups, in partnership with colleges, universities, and other institutions, whether public or private, at local, State and national levels. Each year, NSF funds approximately 10,000 new awards from more than 40,000 competitive proposals submitted by the science and engineering and education communities.[1] Every proposal, whether solicited or unsolicited, receives the same rigorous and objective evaluation through a merit-based review process that involves about 58,000 independent experts each year whose job it is to recommend which proposals should be considered for funding by the NSF.[2] Program officers at NSF combine external peer reviews with their professional judgment to recommend proposals for funding.

As an independent Federal agency, NSF does not fall under any cabinet department. Rather, the National Science Board (NSB, the Board) was established by the Congress in 1950 to oversee and guide the activities of the NSF, and serve as an independent national science policy body to provide advice to the President and Congress on issues related to science and engineering research and education. The Board establishes NSF policies, identifies issues that are critical to NSF's future, and oversees NSF as it carries out its statutory responsibility to maintain the health of the Nation's science and engineering enterprise by funding research in all the basic sciences and engineering. In its role as policy advisor to the President and the Congress, the Board initiates and conducts studies on a broad range of policy topics related to science and engineering research and education, presents the results and Board recommendations in reports and policy statements to the President and Congress, and widely disseminates these documents to the President and Congress, the research and educational communities and the general public.

The Board is currently reviewing existing mechanisms and considering new policies that could enhance the ability of NSF to identify, nurture, evaluate, and fund innovative, "potentially transformative" research. Potentially transformative research is defined as research that has the capability to revolutionize existing fields, create new sub-fields, or cause paradigm shifts in thought.

To determine feasible ways that NSF could alter its current practices, the Board is interested in examining the strategies employed within NSF and at other organizations in the scientific community to identify and fund this type of research. In December 2004 the Board established a Task Force on Transformative Research. Over the next 2years this task force will convene several workshops, each of which will examine different issues related to transformative research. The task force will produce a report at the end of this time that synthesizes the contributions from its deliberations, the workshops, and other sources, and presents policy recommendations for the Board to consider. The task force convened the first NSB-sponsored workshop on transformative research in August 2005.

The purpose of the second NSB-sponsored workshop on transformative research, to be held on December 16, 2005, is to think radically and innovatively about the process of identifying and fostering transformative science. In preparation for the workshop, participants have been asked to consider and prepare brief responses to the following two questions:

- Given what is known about the current peer review system, how would one structure a process *de novo* for selecting individuals and ideas to maximize the opportunity for transformative thinking and research to occur?
- How would one organize an experiment to compare such a new system with what is in place today? Specifically, how best to structure the test group, select the control group, and what kinds

of information should be gather about the people, ideas, projects and institutions (in both groups) to ensure that important predictive factors could be identified at the end of a multi-year trial?

The purpose of this paper is to provide a brief overview of 21 scientific papers addressing various factors associated with the identification and development of transformative research. The papers were identified by the NSB task force as background reading for the December 2005 workshop.

## Introduction

In February 2000, the Board discussed evidence of an emerging need to ensure a commit to "high risk research."[3] Subsequently, an NSF Advisory Committee for Government Performance and Results Act Assessment (AC/GPA) concluded in 2004 that "no obvious formula" exists to guide the Foundation as to the fraction of its portfolio that should be considered "high risk" research, suggesting that the issue warranted further attention by the NSB. That year, the Board organized an exploratory Task Group on High-Risk Research, which convened a workshop at the Santa Fe Institute (September 2004) to solicit the views from members of the community on NSF's approach to funding high-risk research. Participants identified several factors that affect the Foundation's ability to identify, evaluate, and fund "potentially transformative" research.[4] These include the lack of a common definition of "high-risk" or "transformative" research; the need to develop criteria for identifying potentially transformative research proposals; and the need to establish ways of tracking potentially transformative research through the NSF system and evaluating outcomes over an extended period of time.[5]

In response to these and other comments, the Board announced the formation of a Task Force on Transformative Research in December 2004 under the auspices of the NSB Committee on Programs and Plans (CPP),[6] whose role it will be to submit constructive policy recommendations to the CPP and full Board 2 years from the formation of the task force (December 2006). To facilitate its discussions, the NSB further authorized the Task Force to convene a series of workshops to address key issues. The task force convened the first NSB-sponsored public workshop in August 2005 to solicit views from a cross-section of the NSF community – current and former staff, members of the Committee of Visitors, and Advisory Committee members. Three major themes emerged during that workshop:

1. Transformative research can arise in many forms:
    a. A new instrument or technique or its application to a new field
    b. A concept or hypothesis that challenges an existing paradigm
    c. Observations that do not fit comfortably with accepted thinking

2. The NSF program structure may contain some features that are not optimal for encouraging, identifying, or selecting potentially transformative research:
    a. The system is structured to consider ideas in the form of proposals within programs, while ideas for transformative research can cross program lines and exceed the expertise of review panels
    b. Reviewers are chosen for their knowledge of field and often have a stake in defending the field's current paradigms
    c. Low "funding rates" exert pressure to select proposals with the highest probability of short-term success

3. Program officers play a key role in the success or failure of proposals for transformative research and have the discretion to recommend funding despite reviewer disagreement
    a. However, existing mechanisms like the Small Grants for Exploratory Research (SGER) program are not fully utilized or may be diverted for other purposes[7]

In September 2005, the chair of the task force announced that a second workshop would be convened in December 2005 at the Santa Fe Institute to focus on "key factors in identifying transformative science."[8]

## Review of Selected Literature

The task force identified a number of scholarly papers as background reading for the workshop, each of which captures an element of contemporary thinking about transformative research.[9] These papers reflect the work of scientists i engaged in an examination of the social, economic or other factors that influence the creation and dissemination of knowledge throughout the science and technology communities.

The work of these researchers derives from a fundamental belief that the norms of scientific progress established in the 16th and 17th centuries posit free and open communication of scientific information. However, as sociologist Robert Merton argued over 50 years ago, individuals may align research interests within the overarching goal of the advancement of science, but the institution of science is part of a larger social structure which is not always integrated within a culture. Individuals may collaborate but they also compete; they often compete within a reward structure that advances some scientists over others. For funding organizations like the NSF, contemporary research on the sociology of knowledge – such as that embodied in many of the papers identified by the Task Force – offers important insights into reasons why a promising idea in science or technology is accepted readily and may transform a field, and why some ideas simply never advance for reasons that have seemingly little to do with the merit of the idea.

The set of background readings have been organized within three broad headings:

1. Research excellence and how to recognize it
2. Characteristics of collaborative networks
3. Understanding knowledge dissemination

Appendix I provides a complete listing of papers within each topic, with articles organized alphabetically by author within each heading.

### 1. Research Excellence and How to Recognize It

Peer review is the cornerstone of NSF's work. Among the most fundamental and important characteristics of the process is the Foundation's partnership with the extramural science community to assure that "Federal financial support flows through a core system of merit-based peer review administered by NSF staff with expertise in scientific disciplines."[10]

Several papers within the set of readings represent research of potentially direct bearing on the "peer review" process; these include the work of Grant and Allen; Tjissen; Fleming and Sorenson; and Weingart. Collectively, their work suggests that if an organization asks reviewers to do something different during the review process, they will. Furthermore, it is the view of some authors that the review process itself is better served if information provided by the applicant is augmented with information on the disposition of the applicant's work within the field of science.

Grant and Allen (1999), writing from The Wellcome Trust in Great Britain, note that peer review is often criticized as inherently conservative and biased against speculative and innovative research. In 1996, the Wellcome Trust set up an annual competition to fund high-risk, "non-obvious" research in the biomedical sciences. The Trust published a review of the first year of the program in 1998. Part of the evaluation involved an experiment designed to assess how the "innovative" awards were perceived to be when compared with "a sample of standard project grants funded by the Trust." (Grant and Allen, 1999).

Using a sample of proposals from the both the standard and the "innovative" programs, the researchers asked panelists to assess how risky or novel each project was using a 5-point scale based on a set of research summaries that had been rewritten by the experimenters (in consultation with the original applicants). While the shortcoming of this paper is that the authors did not look at the outcomes of the actual funding decisions in terms of whether the research actually resulted in "innovations," the authors found that the reviewers successfully discriminated innovative from more standard applications. This well-structured study demonstrated that reviewers can be directed to record their perception of the innovativeness of grant proposals.

Another approach to the evaluation of "research excellence" suggested by Tijssen (2003) is the use of a "scoreboard." A scoreboard utilizes both qualitative and quantitative measures, adding value to the assessment of an innovative proposal. The author points out that "research excellence seems to vary from field to field, and country to country, depending on the domain's science development, its goals and policy." However, "stylized overviews of factual information pertaining to research excellence in the form of systemic summary tables ('scoreboard') may help bridge the gap between the more traditional benchmarking studies of research performance and the required policy-oriented quantitative studies of research excellence." Information describing the quality of the research group, for example, could be constructed along four dimensions such as the epistemological-cognitive aspects of their research; the organizational aspects; knowledge production; and knowledge dissemination. Using data from cases in the Netherlands the author demonstrates the potential value-added by such data in the review process. The conclusions presented in this paper point to the possible application of "scoreboards" as an "interface between data-driven analyses of research excellence and policy-driven evaluations of research performance" (Tijssen, 2003).

It is important to strike the right balance when utilizing so-called "objective measures" of research excellence. Weingart (2003) discusses the pressures placed lately on the higher education system in the United Kingdom (UK) following the "evaluation craze" launched in 1983 by the publication of articles on "indicators" of research quality by Martin and Irvine. "Indicators of research quality" of academic departments are not yet generally accepted in the UK, according to Weingart. This has been due in part to a commercialization of "rankings" of programs in the absence of traditional peer review. The author suggests that "informed peer review" might combine the two approaches.

While the science policy community has made significant progress in dimensionalizing "research excellence" in science, there has been less progress in describing the identifiable elements of "excellent" research in technology. Fleming and Sorenson (2001) attempt to develop a valid theory of invention by looking at the diffusion of inventions with a model borrowed from the world of evolutionary biology. They fit patent citation rates to a landscape model borrowed from evolutionary biology, looking for the interdependence of "complexity" of an invention with the "certainty" that it will be adopted. Fleming and Sorenson claim that the model is a good fit to the data, although with a number of caveats associated with the use of patent data. However, Fleming and Sorenson do not provide a comparison model nor do they offer a discussion of quality-of-fit metrics.

## 2. Characteristics of Collaborative Networks

When scientists request funding for a research project, they may specify the composition and intended role of a project team in their proposals. Several papers in the set of readings examine "collaborative networks" in science and technology. Findings from this research steps that might be taken during the peer review process to focus attention on the characteristics and composition of the teams being proposed.

Barbasi (2005) offers a useful starting point for understanding network research. His "thought piece" describes how emerging network theory techniques can be used to quantify human dynamics. To illustrate his point, Barbasi points to the increasing amounts of stored data, such as email, credit card data and the like. Although a mathematical theory of social complexity remains "a pipe dream," the author suggests that it is not as far-fetched as it may appear.

Barbasi's work certainly stands as an endorsement for the writings of R. Guimera and his colleagues (2005). These authors suggest that the mechanisms by which creative teams assemble determines the structure of the ensuing collaboration. The "self-assembly" process they propose is modeled along three parameters: (1) team size, (2) the fraction of newcomers, and (3) the tendency of incumbents to repeat previous collaborations. The paper does a very good job of explaining how "team assembly" plays a role in the ultimate approach taken by the team to problem solving.

In a similar vein, Hollingsworth and Hollingsworth (2000) examine the relationship between the structural characteristics of research organizations and the capability of those organizations to make major discoveries in biomedical science. The authors postulate a reasonable definition for a "major discovery" and then proceed to conduct case studies of two organizations to look at the relationship between the structural characteristics of the organizations and such discoveries. The central finding is that major discoveries occurred "because there was a high degree of interdisciplinary and integrated activity across diverse fields of science," although the authors note that "limitations of space permit us to do little more than report in a rather preliminary fashion some of the characteristics of research organizations which are associated with major breakthroughs in biomedical science." From a public policy perspective, it is not clear whether maximizing the number of prizes won by any particular institution may be more important than other outcomes.

There is, also the notion of the "invisible college" of scientists within which most researchers conduct their work. Scientists have begun to examine the broader notion of more loosely organized networks of experts. They do so using a variety of conceptual models – with mixed success.

Sosa and Gero (2005), for example, demonstrate how the "cellular automata" approach holds promise for exploring creativity and innovation, using computational models that can explain and predict social behavior. The aim of these studies, the authors assert, would be to investigate in human groups "the foundational idea" that creativity is a property of systems and varying different system components or "the way in which equivalent components interact" could generate quantitative and qualitative differing results.

Likewise, the paper by Newman (2001) demonstrates how it is possible to construct networks of collaboration between scientists in various disciplines. While the author applies well-established graph research processes, the results are most likely invalidated by imperfect data. Furthermore, the paper fails to show how the results lead to answering the most important question: What does one do with these results?

Bettencourt and his colleagues (2005a and 2005b) examine the potential use of a disease model to describe the transmission dynamics of knowledge within the scientific and technological communities. The analysis begins with a set of three groups of data, and then, postulating a simple model, the authors attempt to find inputs for that model so the model reproduces the data as closely as possible. The approach can be useful in two ways: first, it can logically rule out some model formulations, and second, the best model formulation can be used until further improvements in the model or the data can be made. It should be noted that the historical data used by the authors are not data on the spread of an idea but rather the times of first use of a particular technique (the Feynman diagrams). It is quite reasonable to assume that no one started using Feynman diagrams before they learned about them; but it is quite likely that many people learned about Feynman diagrams long before they started to use them.

Other scientists have begun to unpack the communication process in science and technology itself, although the two papers included in this set of readings represent just a sample of this diverse literature. One approach describes the interaction of communicators across sectors – academia, government and industry. Leydesdorff and Meyer (2003) describe a “triple helix” model for policy and planning. As the authors explain, the model posits that communication is the driver for systems of organized knowledge production but note that “the problem of structural differences among the communications and the organization of interfaces remain crucial to understanding innovation in a knowledge-based economy.”

In a similar effort, Sorenson and his colleagues (2002) offer several qualitative descriptions of the complexity of messages and the effort required to successfully send, receive and understand messages. They argue that the complexity of the message moderates the proportion of future citations, and offer a measure of interdependence K, which explains differences in diffusion rates. Clearly, K need not always exist; perhaps the authors meant to say that such a K “might exist.”

## 3. Understanding Knowledge Dissemination

Today, there is greater emphasis with the Federal government on demonstrable “program outcomes.” Several papers in this set are pertinent to *post hoc* analyses of funding decisions in that they represent contemporary approaches to knowledge discovery and its dissemination within a community.

Unfortunately for evaluators, the work of Van Raan immediately points to a dilemma evident is the world of scientific publishing, which is that new ideas are taken up at different rates within the scientific community. In his work on “sleeping beauties,” Van Raan examines the phenomenon whereby articles attract no attention when first published but later become highly cited. Perhaps more importantly, Van Raan in this and his other works cited here demonstrates that bibliometrics research offers an important tool for understanding the dynamics of the knowledge diffusion for purposes of outcome analysis. It seems likely, however, that further work is needed in the development of appropriate bibliometric methodologies for purposes of identifying transformative research – given that the estimates of error in Van Raan’s work are quite high.

Tackling the problem of scientific communication from a more sociological perspective, Zucker and Darby (1996) examine the role of “star scientists” in the biotechnology enterprise. They find that the very best scientists play a “key role in the formation of new and transformation of existing industries” a finding that might be argued by some to be self-evident. The research method embodied in this article requires extensive data and a technology that must already be well-established.

Glanzel and his colleagues (2002) also use publication patterns to examine knowledge dissemination, but they do so to compare publication activity across countries. Glanzel’s techniques are simpler than the more sophisticated approach of Van Raan. However, Glanzel successfully demonstrates that the journal literature can be used for comparison of the publication activity of countries in quantitative form.

The article by Leydesdorff and Heimeriks (2001) offers an interesting counterpart to Glanzel’s work. The authors focus on the metamorphosis of the European S&T system through transnational collaboration and a “technoscience” network of biotechnology. Using a key word analysis of journal articles, Leydesdorff and Heimeriks demonstrate that Europeans develop an interactive intellectual space among their countries which draw from work from Americans and Japanese, albeit to a lesser extent. The network effect at the

European level appears to be related to institutional interactions, more than might be expected based on the actual content of the papers utilized in this analysis.

The article by Redner (2005) is a reminder that the choice of journals in which an author publishes has some bearing on citation patterns. Redner examines the cumulative distribution of citations within a single journal using a variety of measures, such as the average citation age, distribution of citing ages and distribution of cited ages of a given paper. Fitting curves, the author concludes that a small number of physicists have played a "remarkably large role" in top-cited publications in Physical Review. Questions remain, however, with respect to the impact of those authors in disciplines not represented by this journal.

**Conclusions**

This series of papers offers insights into the complex research presently underway to identify and measure the flow of scientific ideas throughout the research and technological communities. Much of the work depends upon complex models and access to large data sets. Nonetheless, important lessons emerge from this review. First, it is useful to provide reviewers with supplemental information from which – if asked – they can make better informed judgments regarding innovative research being proposed by scientists. Second, scientists represented here offer documentation of what constitutes effective teams. Finally, the many retrospective analyses of the outcomes of innovation do not appear have yet to offer practical ideas for identifying (anticipating) transformative research.

**Appendix I**

***Research Excellence and How to Recognize It***

Fleming, L. and O. Sorenson “Technology as a Complex Adaptive System: Evidence from Patent Data” (2001)

Grant, J. and L. Allen, “Evaluating High Risk Research: An Assessment of the Wellcome Trust’s Sir Henry Wellcome Commemorative Awards for Innovation Research” (1999)

Tijssen, R. “Quality Assurance: Scoreboards of Research Excellence” (2003)

Weingart, P. “Impact of Bibliometrics upon the Science System: Inadvertent Consequences?” (2003)

***Characteristics of Collaborative Networks***

Barbasi, A-L. “Network Theory – the Emergence of the Creative Enterprise” (2005)

Bettencourt, L.M.A.,, A. Cintron-Arias, D.I. Kaiser, and C. Castillo-Chavez “The Power of a Good Idea: Quantitative Modeling of the Spread of Ideas from Epidemiological Models” (2005a)

Cintron-Arias, A., L.M.A. Bettencourt, K.I. Kaiser, and C. Castillo-Chavez “On the Transmission Dynamics of Knowledge” (2005b)

Guimera, R., B. Ussi, J. Spiro, and L.A. Nunes Amaral “Team Assembly Mechanisms to Determine Collaboration Network Structure and Team Performance” (2005)

Hollingsworth, R. and E.J. Hollingsworth “Major Discoveries and Biomedical Research Organizations: Perspectives on Interdisciplinarity, Nurturing Leadership, and Integrated Structure and Cultures” 2000

Leydesdorff, L and M. Meyer “The Triple Helix of University-Industry-Government Relations” L. (2003)

Newman, M.E.W.J. “Who is the Best Connected Scientist? A Study of Scientific Co-Authorship Networks” (2001)

Sorenson, O, J. Rivkin, and L. Fleming “Complexity, Networks and Knowledge Flow” (2002)

Sosa, R. and J.S. Gero “Studies of Creativity and Innovation in Complex Social Systems” (2005)

***Understanding Knowledge Dissemination***

Glanzel, W., A. Schubert, and T. Braun “A Relational Charting Approach to the World of Basic Research in Twelve Science Fields at the End of the Second Millennium” (2002)

Leydesdorff, L. and G. Heimeriks “The Self-Organization of the European Society: The Case of ‘Biotechnology’” (2001)

Redner, S. “Citation Statistics from 110 Years of Physical Review” (2005)

Van Raan, A.F.J. “Sleeping Beauties in Science” (2004a)

Van Raan, A.F.J. “Measuring Science: Capita Selecta of Current Main Issues” (2004b)

Van Raan, A.F.J. “Fatal Attraction: Conceptual and Methodological Problems in the Ranking of Universities by Bibliometrics” (2005a)

Van Raan, A.F.J. “Measurement of Central Aspects of Scientific Research: Performance, Interdisciplinarity, Structure” (2005b)

Zucker, L.G. and M.R. Darby “Star Scientists and Institutional Transformation: Patterns of Invention and Innovation in the Formation of the Biotechnology Industry” (1996)

## Endnotes

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[1] Although NSF represents about 4 percent of total Federal support for research and development, NSF funding accounts for approximately 13 percent of all Federal support for basic research and 40 percent of non-"life sciences" basic research at US academic institutions. See National Science Foundation Strategic Plan FY 2003 – 2008 at http://www.nsf.gov/publications/pub_summ.jsp?ods_key=nsf04201 Data regarding award patterns have been drawn from the "Report of the National Science Board on the National Science Foundation's Merit Review System" NSB-05-119, September 200.

[2] "Merit review is a critical component of the National Science Foundation's decision-making process for funding research and education projects. Two years ago, NSF announced changes in its merit review criteria (Important Notice No. 121, *New Criteria for NSF Proposals*, July 10, 1997). The changes reflected extensive analysis and discussion, with community input. Recommendations were considered to simplify the merit review criteria and harmonize them with the NSF strategic plan, in order to weigh a proposal's technical merit, creativity, educational impact and its potential benefits to society. This process resulted in the two criteria now in effect, which address the intellectual merit of the proposed activity and its broader impacts." See http://www.nsf.gov/pubs/1999/nsf99172/nsf99172.htm for a discussion of current merit review criteria.

[3] Minutes from the 356th Meeting of the National Science Board February 2000 (NSB-00-39) http://www.nsf.gov/nsb/meetings/2000/feb/nsb0039/nsb0039.htm

[4] "'Potentially transformative research' has been defined as research that has the capability to revolutionize existing fields, create new subfields, or cause paradigm shifts in thought." A Background Paper for the NSB-Sponsored Workshop on Transformative Research, 12 August 2005, Arlington, Virginia

[5] Workshop participants also discussed aspects of the peer review process that militate against selection of potentially transformative research. A fuller discussion of the results of the September 2004 workshop will be found at http://www.nsf.gov/nsb/committees/cpptrcharge.htm

[6] NSB/CPP/TR-04-1, December 16, 2004.

[7] Taken from "Summary Notes from the NSB-Sponsored Workshop: Transformative Research at the National Science Foundation" draft document 091405.

[8] "Summary Report of the September 28-29, 2005 Meeting" at http://www.nsf.gov/nsb/meetings/2005/0928/summary_report.pdf

[9] Many of the papers summarized here may, in fact, address more than one of these topics.

[10] See the "Memorandum from the Chairman of the National Science Board" September 30, 2005 transmitting the Report of the National Science Board on the National Science Foundation's Merit Review System, *op.cit.*