Final Report

NSF Workshop on
Social Organization of Science and Science Policy

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EXECUTIVE SUMMARY

NSF Workshop on Social Organization of Science and Science Policy

On July 13-14, 2006, a workshop on the Social Organization of Science and Science Policy was held at the National Science Foundation (NSF) in Arlington, Virginia. The workshop was funded by an NSF grant from the Sociology and Science and Society Programs in the Directorate for Social, Behavioral and Economic Sciences (SBE).[1] The workshop was to provide guidance to the National Science Foundation as it launches a new focus on the Science of Science Policy. [2]

The participants in this workshop, who were drawn from across the research fields supported by the SBE Division of Social and Economic Sciences (SES), share a commitment to studying science, technology, engineering, and innovation as social processes and to working together across disciplines to understand the dynamics that conjoin science, technology, and society. The discussion asked, "How can those who study science as a social process do more to help science administrators and other participants in the policy making process understand the complex domain for which they are responsible? What new data or indicators might we develop? More generally, how can the social sciences and humanities provide better information and ideas to assist in shaping science, technology, and innovation policies intelligently?"

Science, engineering, and technology play a role in and are influenced by many policies, and they are in turn supported in order to contribute to a variety of desired societal outcomes. Workshop participants articulated a wide range of research questions about those interconnections. They identified the following areas as crucial components for a research agenda to inform and challenge current policy making with and for science, technology, engineering, and innovation. The questions illustrate but do not exhaust the range within each area.

1. **Innovation for well-being and social productivity.** Science, technology, and innovation (STI) policies aim not only to contribute to economic growth but also to make life better within nations and globally. Are those policies successful in these goals? When and how do they produce the outcomes policy makers project? Are their benefits distributed appropriately? Are they making a net contribution to generating decent work, high employment, economic competitiveness, and environmental sustainability? How might the connections between science, technology, and innovation policies and quality of life be enhanced?
2. **Social environments for innovation and creativity.** The institutional and organizational settings and social groups active in science, technology, and innovation have varied historically and across nations, and can change in response to changing conditions. What are the immediate and long-run implications of the different arrangements? What organizational, institutional, architectural,

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[1] SES-0631250 to Susan Cozzens, Georgia Institute of Technology. The views expressed here are those of the workshop participants and do not necessarily represent those of the National Science Foundation.
[2] See the FY2007 NSF budget request, at http://www.nsf.gov/about/budget/fy2007/pdf/4-ResearchandRelatedActivities/6-SocialBehavioralandEconomicSciences/25-FY2007.pdf.

geographic, and social network and infrastructural conditions are most conducive to creativity and innovation that is socially and scientifically beneficial? What roles do disciplines and interdisciplinarity play? Who is included and excluded by current institutional, organizational and social arrangements? How do various ways of knowing contribute to creativity and problem solving for society? Why are some places successful at innovation while others fail? For example, which milieus set up creative tensions between competition and collaboration? Why are some innovations celebrated and promoted, while others are suppressed or ignored? How do innovators share data and knowledge and what are the opportunities and constraints of that process? How do established structures of status and hierarchy enhance or inhibit innovation and creativity and their translation into outputs?

3. **Political economy of science, technology, and innovation policy.** The development of science, technology, and innovation is a value-laden process that may be usefully informed by the creation of scientific indicators and models for S&T and their consequences, but never determined by them. Science, technology, and innovation policies are enacted in many places by many actors, well beyond national government decision making, for example, corporations, non-governmental organizations, multinational governing bodies, and other interest groups. What actors are involved? What influence do they have? How do they draw the boundaries around this domain of policy? Do they treat science, technology, and innovation as public goods or as private commodities? What processes of social action and decision making lead to the wisest, most widely acceptable results in science, technology, and innovation policies?

4. **Evidence and expertise in science-intensive decision making.** In science, technology, and innovation policies, as in other policy areas, evidence and expertise are just one source of inputs to a broader process of reasoning and logic. Who counts as an expert on issues of science, technology, and innovation policy? What constitutes evidence and evidence-based decision making in science-intensive policy areas? Do these factors differ from other policy areas and how do they change over time and in different contexts? What kinds of evidence, expertise, and models are actually used in science, technology, and innovation policy decisions? If evidence is not used, why? How are uncertainties and tacit assumptions in this knowledge base evaluated and factored into decision making? What is the empirical support for the major assumptions underlying science, technology, and innovation policies? Where that support is limited, what data and empirical approaches would shed more light on the policies? How can the use of knowledge and expertise by different types of actors, in various areas of policy, be made more accountable and transparent in democratic processes?

5. **Science, technology, innovation, and global change.** Economic, political and social relationships are changing at global level. What roles are science, technology, and innovation playing in those changes and vice versa? When different ways of knowing come into conflict in this process, how are the conflicts resolved? How are the structures and networks of science, technology, and innovation that extend across national boundaries changing options for national

> governments and the citizens they serve? What national visions and goals move towards greater well-being for everyone in the emerging world order, and how can those visions and goals be implemented? How do science, technology, and innovation policies and policy-making processes differ between the United States, Europe, China, Japan, and other countries? What are the implications for national and international well-being of these differences?

To give the effort the breadth of vision, skill, and energy needed, those working directly on science, technology, and innovation policy issues should be joined by the wider science and technology studies community, other social scientists and humanists, and other scientists and engineers. Opportunities to involve civil society in the research agenda will be important. Comparative studies will be needed, and cross-national collaborations should be encouraged.

Workshop participants agreed that appropriate research approaches will be methodologically diverse, ranging from normative to descriptive studies and from discourse analysis, history, and ethnography to community-based research and quantitative approaches. New theoretical frameworks and methodologies are likely to be necessary. New sets of indicators should be developed, to reflect the broad range of processes and outcomes included in the research agenda. More open and machine readable access to grant records and affordable access to publication data would help the work, along with the generation of detailed case studies in policy making and outcomes.

A significantly expanded research effort in this area should be accompanied by community and capacity building efforts, including sharing of best practices, graduate training opportunities and program-building, data access and development, workshops, conferences, and public outreach. New forms of teamwork and collaboration should be explicitly encouraged, particularly those that involve mutual engagement across the social sciences, other sciences, and engineering. To be useful, the effort must also include interactions with policy audiences designed to include two-way communication of issues, information, and insights.

REPORT

A. Introduction

On July 13-14, 2006, a workshop on the Social Organization of Science and Science Policy was held at the National Science Foundation in Arlington, Virginia.[3] The workshop was originally planned by the Sociology and Science and Society Programs in response to a call from Dr. John Marburger, the U.S. President's Science Advisor, for a "social science of science policy" (more on this call in Section II below). These programs wanted to gather their grantee communities to review their past contributions to science policy discussions and suggest a future research agenda. The Science and Society program had co-sponsored an earlier workshop on a related theme, "Research Policy as an Agent of Change."[4]

As the NSF-wide response to Dr. Marburger's call evolved, the workshop became one of three in the Directorate for Social, Behavioral, and Economic Sciences held to provide advice on shaping a new funding program on the "science of science policy." [5] Over the course of the summer of 2006, each of the NSF divisions sponsored a workshop to help with this effort. The Division of Behavioral and Cognitive Sciences sponsored one on individual creativity.[6] The Division of Science Resources Statistics sponsored one on measurement of innovation, particularly at national level.[7] The Division of Social and Economic Sciences sponsored the one reported here to focus on team and organization-level innovation processes. The Executive Summary of this third workshop became one input to the prospectus for this effort, now renamed again the "Science of Science and Innovation Policy,"[8] and the program solicitation (NSF 07-547) for research proposals in this area.[9]

The participants in the workshop were drawn from across the fields supported in the Division of Social and Economic Sciences. Although there was a strong contingent of sociologists and science policy scholars, participants also came from backgrounds in political science, history of science, philosophy, science and technology studies, and economics. The group represented an unusual confluence of intellectual traditions; this mix was deliberate. The organizers felt that there was a much broader range of social

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[3] Funded by an NSF grant from the Sociology and Science and Society Programs, SES-0631250, to Susan Cozzens, Georgia Institute of Technology. The views expressed here are those of the workshop participants and do not necessarily represent those of the National Science Foundation.

[4] National Science Foundation, *Research Policy as an Agent of Change* (NSF 05-209): http://www.nsf.gov/pubs/2005/nsf05209/start.htm

[5] See the FY2007 NSF budget request, at http://www.nsf.gov/about/budget/fy2007/pdf/4-ResearchandRelatedActivities/6-SocialBehavioralandEconomicSciences/25-FY2007.pdf.

[6] Workshop: Innovation and Discovery: The Scientific Basis of Individual and Team Innovation and Discovery, May 17-18, 2006. http://www.lrdc.pitt.edu/schunn/innov2006/talks/schedule.htm, accessed March 22, 2007.

[7] Workshop: Advancing Measures of Innovation: Knowledge Flows, Business Metrics, and Measurement Strategies, June 6–7, 2006. http://www.nsf.gov/statistics/workshop/innovation06/, accessed March 22, 2007.

[8] The prospectus was posted at http://www.nsf.gov/sbe/scisip/scisip_prospectus.pdf, accessed January 3, 2007.

[9] The solicitation can be found at: http://www.nsf.gov/publications/pub_summ.jsp?ods_key=nsf07547

scientists and humanists working on topics relevant to science and innovation policy than was reflected in usual gatherings under the policy label. They wanted to hear the voices of this broader group and include their insights in the formulation of the NSF initiative.

As the Executive Summary notes, the core questions motivating the discussion were of the following kinds: "How can those who study science as a social process do more to help science administrators and other participants in the policy making process understand the complex domain for which they are responsible? What new data or indicators might we develop? More generally, how can the social sciences and humanities provide better information and ideas to assist in shaping science, technology, and innovation policies intelligently?"

These questions posed challenges for the group that kept the two-day discussion lively and wide-ranging. Strands of discussion developed across the various presentations and captured and integrated the views of the participants better than the formal agenda. This report first summarizes the workshop sessions (Section B), then reports on those themes.

Three themes treated the relationship between the arena of science policy and the field of science policy research (Section C). First, there is the *opportunity* created by Dr. Marburger's call and the NSF initiative. The participating researchers did not assume that only the questions Dr. Marburger posed in his speeches should shape the research program. Instead, they articulated their own *visions* of what science policy is, informed by research on historical and contemporary policy processes in various policy domains. And there was considerable debate, reflecting a very wide range of views among workshop participants, on the best ways for research to have *impacts* in a policy arena.[10]

The bulk of the workshop discussion was devoted to articulating *research questions*, which fall into the categories outlined in the Executive Summary and used in Section D below. There was quick agreement that the *methodological approach* should be pluralistic since many methods used by social scientists and humanists would be useful in addressing the research agenda (Section E). Finally, the discussion returned again and again to what needed to be done to strengthen a sense of *community* in this research area and *educate a new generation* of researchers for impact in science policy deliberations (Section F). Section G summarizes the workshop conclusions, including general points made across the workshop papers (which appear in Appendix 3).

B. Workshop Sessions

For purposes of facilitating discussion, the workshop was organized into seven sessions. Because the topic of science policy is fundamentally interdisciplinary, dynamic and complex, ideas were often raised at several different sessions. However, to help readers understand the context of the discussions, brief highlights of the main points raised at each session are presented below.

[10] The workshop participants had the opportunity on the second day of the gathering to discuss these questions with Dr. David Lightfoot, the NSF Associate Director for Social and Behavioral Sciences, and with Dr. Kaye Husbands Fealing, the Senior Scientist coordinating the science of science policy initiative.

Session 1. Science Policy: Institutions and Issues

There was immediate consensus that the boundaries of the science policy community are dynamic and involve numerous interactions across the public and private sectors, and across disciplinary boundaries. The science policy sphere was described as "complex," "decentralized," "fragmented," "networked," and "pluralistic." There was general agreement that working together across boundaries was important and would likely lead to more constructive activities. This should be encouraged both through policy decisions and through institutional arrangements. Reliance upon established institutional arrangements is likely to lead to somewhat predictable, perhaps conservative, agendas. Modifications to those institutional arrangements are likely to generate new agendas, unconventional models, and alternative framings of questions.

Session 2. Knowledge and Innovation Process

Workshop participants recognized the importance of better understanding where knowledge that shapes policy comes from and how it is granted legitimacy. A simple, linear model of how knowledge is incorporated in the innovation process was generally seen as outdated. It was also agreed that defining innovation in economic terms was too narrow and that "societal benefits," "social productivity," "quality of life," and the "public good" needed to be incorporated in a re-conceptualization of that term. In this session, participants also brought up the importance of using both quantitative and qualitative research methods, the need to develop more inclusive and flexible databases and data structures, the significance of both "big" and "small" science, and the value of comparative research.

Session 3. The Institutions of U.S. Science

Much of this session focused on the relationship between the political process and the scientific communities. It was widely acknowledged that funding is important and that funding decisions are oftentimes motivated by political interests. Analysis of the claims that advocates use to support funding science would be beneficial. Case studies about the processes of science policy would be helpful in identifying actors, interests, biases, and institutions. Such studies would also point out where the existing system is not conducive to good science and where procedures for decision-making might be made fairer and more participatory. Studying the failures in science policy as well as the successes is essential to understanding the dynamics of the processes of science policy.

Session 4. Cultures, structures and networks of knowledge production in the conduct of research and inquiry

During this session workshop participants began to parse the environment of the science policy community. A number of important components were identified including the conservative tendency of disciplines and universities, and the ties between universities and industry. Participants also discussed how the context of scientific inquiry might be changed so that new ideas and patterns of interactions emerge. Geography and architecture could be designed to afford more transparency, flexibility, and interaction. Research schedules could allow more empty time for the possibility of insights and novelty.

Session 5. Social processes and the generation of data: elements, categories and indicators

The question of normative elements, the "ought's" and "should's" in science policy, mentioned in earlier sessions, received more attention here. Participants also recognized that data elements and standards were to some extent relative, often embodying some set of ethics or values. Analyses of crises and failures of knowledge are also likely to reveal inadequacies and limitations in existing data. It was also pointed out that the language of research and that of policy were not congruent and that the process and logic of translation was of critical importance. Developing the capacity for understanding the broader and contextualized nature of science policy is likely to extend beyond the training of researchers to involve the larger community.

Session 6. International Context

As is true in other realms, science today is embedded in a global economy and global community. Issues, concerns, methods, and scientific communities cross national borders and cultural contexts. Despite this, workshop participants recognized that concepts such as nation and country are still important in the development and implementation of science policy. Because it is important to understand different "ways of knowing" that emerge in the global environment, it is beneficial for scholars and scientists to seek opportunities to work outside their local realms. In this global environment, reconciling concerns about equality with those of competitiveness and concerns about peace with those of national security might be assumed into issues of science policy.

Session 7. How can the social sciences inform science policy?

Participants saw opportunities for social scientists to facilitate intellectual conversation and public reasoning about science policy. A broader social science perspective might reveal that science policy is more than the allocation of funds, but also involves the design of infrastructures, the collection of data, and the development of a legal system. Understanding the ways in which knowledge is provided and disseminated to citizens, industry, and government agencies is likely to reveal the biases in the operation of existing structures and possible areas for reform.

C. Discussion Themes

Opportunity for a policy audience

Dr. Marburger began his calls for a science of science policy in spring, 2005. In a speech at a AAAS forum,[11] he lamented the current state of science and technology (S&T) indicators:

> … the indicators are based on a data taxonomy that is nearly three decades old. Methods for defining data in both public and private sectors are not well adapted to how R&D is actually conducted today. … And the indicators are not linked to an overall interpretive framework that has been designed to inform policy.

[11] 30th Annual AAAS Forum on Science and Technology Policy in Washington, D.C., April 21, 2005. Text of Dr. Marburger's speech can be found at: http://www.aaas.org/news/releases/2005/0421marburgerText.shtml, accessed March 22, 2007.

Expressing his need for a better information base to inform the recommendations on budget and policies that his office makes to the President and Congress, Dr. Marburger described what he saw as the current state of the field:

> Much of the available literature on science policy is being produced piecemeal by scientists who are experts in their fields, but not necessarily in the methods and literature of the relevant social science disciplines needed to define appropriate data elements and create econometric models that can be useful to policy experts. I am suggesting that the nascent field of the social science of science policy needs to grow up, and quickly, to provide a basis for understanding the enormously complex dynamic of today's global, technology-based society.

Having identified this area of research as a branch of economics, Dr. Marburger then made a strong plea for the development of "econometric" models to assist in the policy process. "This is a task," he said, "for a new interdisciplinary field of quantitative science policy studies." The importance Dr. Marburger assigned to this effort was clear in his comments. He was not as worried about resources, he said, as about how to use them.

> I worry constantly that our tools for making wise decisions, and bringing along the American people and their elected representatives, are not yet sharp enough to manage the complexity of our evolving relationship with the awakening globe. I want to base advocacy on the best science we can muster to map our future in the world.

Dr. Marburger repeated and expanded on this call to action in several other gatherings. In November, 2005, at their invitation, Dr. Marburger again took up this theme in a talk to the Consortium of Social Science Associations (COSSA),[12] and in keynote addresses at the Atlanta Conference on Science and Technology Policy in May, 2006,[13] OECD's Global Science Forum international workshop on the science of science policy in July, 2006,[14] and another OECD event on the future agenda for science and technology indicators in Ottawa in September, 2006.[15]

Together, the talks provide a number of examples of the kinds of problems Dr. Marburger would like models to address:

- the likely futures of the technical workforce and its response to different possible stimuli
- the impact of globalization on technical work
- the sources and implications of high rates of production of technically trained personnel in China
- the impact of yet further revolutions in information technology on the work of scientists and engineers

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[12] COSSA Annual Meeting in Washington, DC. October 31, 2005. Text from Dr. Marburger's speech can be found at: http://www.tpac.gatech.edu/nsfworkshop/essays/MarburgerCOSSA_103105.doc, accessed March 22, 2007.

[13] Atlanta Conference on Science and Technology Policy 2006: US-EU Policies for Research and Innovation, May 18-20, 2006. http://www.spp.gatech.edu/conference2006/main.php, accessed March 22, 2007.

[14] http://www.oecd.org/dataoecd/42/63/37470200.pdf. Accessed January 5, 2007.

[15] http://www.oecd.org/dataoecd/48/14/37483994.pdf. Accessed January 5, 2007.

- the effect on federal programs of the inexorable proliferation of research centers, institutes, and laboratories
- the effect of huge fluctuations in state support for public universities
- the complex linkages between R&D investments and economic and other variables that lead to innovation, competitiveness, and societal benefits

Vision of science policy

The OSTP initiative created the opportunity for the workshop, but what science policy is, and what it is intended to do is much larger than what is embedded in the decisions on which OSTP might be asked to comment. Workshop participants shared a vision of science policy as concerning more than funding for frontier science and technology, competitiveness and the economy, and the actions of formal policymakers.

In addition to the scope of science policy, participants considered the relevant participants; here, too, they viewed government science policy makers as only a part of the large and complex web of actors that influences the development of science and engineering in a national context. This network includes government science policy makers, private firms (including both national and multi-national firms), social movements, media, non-governmental organizations, universities, other research institutions, and a wide range of government agencies, very few of which are concerned centrally with research and development. In addition, national science policy makers are increasingly acting in an international context set by the global operations of firms and research institutions as well as by the collaborative networks of non-governmental organizations and of science itself. The interactions and relationships among all these actors have evolved historically and are still evolving. Thus the whole web of relationships, not just the national science policymakers, are and must continue to be the focus of research in the social sciences.

The issues that arise in this network of actors go beyond funding for frontier science and technology, even though the funding questions tend to be front and center with national science policymakers. Funding allocations across broad fields have been relatively stable in U.S. R&D for a long time, with defense expenditures dominating the profile. It is therefore odd to focus U.S. policy research on the allocation issue, to the neglect of others. For example, some of the most important issues that affect contemporary life arise from existing technologies that are *not* used to their greatest capacity. Understanding these failures, as much as supporting innovation of cutting edge technologies, should be central to science and technology policy. Hurricane Katrina, for example, exemplifies the ways in which existing engineering knowledge and the capacity of the levees were the critical elements in the outcomes of a weather event.

Levels of funding also only partly shape the character and orientation of research institutions, including universities and government laboratories. These institutions are also shaped by intellectual property, safety regulations, immigration law, expectations of contributions to economic development, etc. Science policy should address not only how much funding is distributed to research communities but also the processes by which funding agencies allocate resources. Thus, the workshop raised a challenge to the current review processes and peer review system. Science policy should consider the site of knowledge production and role of knowledge networks, collaborations and complex distributions of effort that may challenge the relevance of the current system that is modeled on an independent, discipline-bound researcher model. Understanding the processes of generating and evaluating novel and innovative science and engineering enterprises within their organizational and political contexts should be a core concern of science policy.

Science policy sets directions for society at large, an idea captured in one participant's claim that "Innovation policy is the organized transformation of the world." The context and goals of science policy thus go well beyond the economic concepts that tend to dominate current discourse, including the concept of competitiveness. Even as embodied in public goals and reflected in agency research agendas, society wants much more from science than economic growth. The expectations extend to environmental and health protection, equality of opportunity, and a supportive environment for democratic governance. NSF's evaluation criteria include one called "broader social impacts," and the workshop participants were enthusiastic about using this established principle to examine all of what government does with and for science. Participants stressed the importance of "ought" and "should" questions, like "What kind of society are we trying to create?"

Impacts for the research in this field

This broad view of what science policy is also establishes quite an inclusive concept of what a policy-relevant science policy research agenda would consist of. This raised the question of correspondence between Dr. Marburger's call and the results such a research agenda was likely to produce.

Workshop participants were in agreement that this research agenda should not be geared to provide short-term answers to short-term questions. This is the role of policy analysts, who in the U.S. system are often located either inside government or in contract research organizations like SRI or the Science and Technology Policy Institute.[16]

Just as strongly, however, the workshop participants rejected the possibility that NSF should support an applied research program in this area, that is, one with a focused and negotiated agenda driven by policy needs determined by clearly-identified "users." Such agendas in other countries, for example, might include the "users" on panels that select projects and include direct "usefulness" as a selection criterion. Evaluating a program of research of this sort would involve asking whether key user questions received direct answers. Development of fundamental knowledge about the dynamics of science might be a by-product of such a research program, but not its primary goal.[17]

Where the workshop participants began to differ, however, was in the differentiation between two common concepts in science policy: "use-inspired basic research"[18] and "pure basic research." Both concepts imply the development of basic

[16] Laredo's essay calls this "consultancy."
[17] Laredo's essay calls this "targeted research."
[18] A term introduced by Don Stokes in *Pasteur's Quadrant*, to call attention to the two dimensions of variation that are conflated in the traditional linear concepts of basic, applied, and development. Use-inspired basic research stands at the intersection between usefulness in solving practical problems and exploration of fundamental principles. Europeans would call it "strategic research." Laredo's essay refers to both of these as "long-term research."

understanding, through conceptual and methodological development and solid empirical research. But some workshop participants called for the research community to orient that knowledge to policy use by listening to policy audiences, paying particular attention to research that sheds light on the questions they face, whether classic or immediate, and challenging policy makers with new ways of seeing their problems. Others rejected this orientation towards policy questions and instead called on the field to develop its research agenda on its own terms.

Despite the debate over these two concepts, the group seemed to agree that the science policy research field should be strongly engaged both with the science and engineering research enterprise and with policy makers from the broad spectrum identified in Section III, from community to governmental. The science policy research community *should* have an impact, participants agreed, and it cannot have an impact if it is isolated in its own discourse. The key issue may then be autonomy. As long as it is understood that science policy research will raise its own issues, use its own concepts, and enlighten policy making rather than merely providing answers to its questions, the two concepts of basic research are not far apart from each other.

The relationship between research and policy, however, does raise organizational issues for the field. People who contribute to the research base for science policy now work in several different institutional locations including universities, government laboratories, think tanks, and statistical agencies. Some interact regularly with governmental policy makers and their staff; others interact regularly with civil society organizations and their staff; and still others interact with people in other parts of the policy network. Questions and knowledge from all these sources needs to be integrated to meet the challenges outlined by Dr. Marburger's call. The workshop participants did not make recommendations on how to do that, but agreed that the task needed attention under the NSF initiative.

D. Research questions

As we have pointed out, the workshop participants were an interdisciplinary group, but they envisioned an even more broadly interdisciplinary group of scholars as essential to extending knowledge in this field. In the next section of the report, we discuss the range of methods the group recommended, and in Section G we discuss their vision for an expansive research community. This section expands on the list of research topics the group recommended, as previewed in the Executive Summary.

The workshop participants saw science, engineering, and technology playing a role in and being influenced by many policies. In turn, they are supported in order to contribute to a variety of desired societal outcomes. The participants articulated a wide range of research questions about those interconnections. They identified the following areas as crucial components for a research agenda to inform and challenge current policy making with and for science, technology, engineering, and innovation. The questions illustrate but do not exhaust the range within each area. Examples come from the papers the workshop participants prepared ahead of the meeting, and which are included in this report as Appendix 4.

1. **Innovation for well-being and social productivity.** Science, technology, and innovation (STI) policies aim not only to contribute to economic growth but also to

make life better within nations and globally, for example by exploring global climate change, fire management, human-ecosystem interactions, mental health, family stability, and personal well-being. How do scientific communities set their research agendas, given goals like these?

a. *Are those policies successful in these goals? When and how do they produce the outcomes policy-makers project? Are their benefits distributed appropriately?* Better measures of the effectiveness of research should go beyond return on investment concepts to be able to assess such goals as military supremacy, social connectivity, reduced child mortality, or a smaller gap between rich and poor. Successful research does not always lead to the positive outcomes policymakers expect; for example, excellent agricultural research may be done in a state with a declining agricultural economy.

b. *Are they making a net contribution to generating decent work, high employment, economic competitiveness, and environmental sustainability?* For example, can science policy help rehabilitate New Orleans's shattered research infrastructure and direct inquiry to resilient urban ecosystems?

c. *How might the connections between science, technology, and innovation policies and quality of life be enhanced?* Research in this field should contribute to identifying which fields will bear the most fruit.

2. Social environments for innovation and creativity. The institutional and organizational settings and social groups active in science, technology, and innovation have varied historically and across nations, and can change in response to changing conditions.

a. *What are the immediate and long-run implications of the different arrangements?* For example, is the traditional reliance on expert, peer-review procedures in many research management processes still effective? What are the alternatives? Universities are shifting from a public good knowledge/learning regime to an academic capitalist knowledge/learning regime. What are the implications?

b. *What organizational, institutional, architectural, geographic, and social network and infrastructural conditions are most conducive to socially and scientifically beneficial creativity and innovation?* What roles do disciplines and interdisciplinarity play? Where does engineering fit ?

c. *Who is included and excluded by current institutional, organizational and social arrangements? How do various ways of knowing contribute to creativity and problem solving for society?* For example, what is the role of patient groups formed on the Internet in producing medical knowledge? Many U.S. black and Hispanic students are excluded from science and engineering by the educational process, and the consequences stretch into community development.

d. *Why are some places successful at innovation while others fail? For example, which milieus set up creative tensions between competition and collaboration?* Current innovation processes are not linear, but involve co-evolution and co-production. Networks are the well-spring of innovation; but why do they establish themselves in some places rather than others?

e. *Why are some innovations celebrated and promoted, while others are suppressed or ignored?* For example, how is innovation rewarded in places that produce "gray knowledge," like government and industry laboratories?

f. *How do innovators share data and knowledge and what are the opportunities and constraints of that process?* For example, now that the information infrastructure allows scientists to share data freely, the potential for data abuse is greatly increased.

g. *How do established structures of status and hierarchy enhance or inhibit innovation and creativity and their translation into outputs?* Organizational context, intra-group structure, and individual relations may all influence creativity, but we lack empirical work to substantiate the claims made about them. Fundamentally conservative social institutions, like universities, nonetheless produce novelty.

3. Political economy of science, technology, and innovation policy. The development of science, technology, and innovation is a value-laden process. Science, technology, and innovation policies are enacted and implemented in many places by many actors, well beyond national government decision making, for example, corporations, non-governmental organizations, multinational governing bodies, and other interest groups.

a. *What actors are involved? What influence do they have? How do they draw the boundaries around this domain of policy? Do they treat science, technology, and innovation as public goods or as private commodities?* For example, knowledge creation, diffusion, and use involve scientific advisory bodies and regulatory agencies, standard-setting and professional bodies, courts, and legislatures. We need to know more about their roles in the various realms of STI policy. The political dimension of science policymaking must be taken into account.

b. *What processes of social action and decision making lead to the wisest, most widely acceptable results in science, technology, and innovation policies?* Social scientists need to challenge institutionalized science-think, by broadening the frame of reference and questioning myths and assumptions.

4. Evidence and expertise in science-intensive decision making. In science, technology, and innovation policies, as in other policy areas, evidence and expertise are just one source of inputs to a broader process of reasoning and logic.

a. *Who counts as an expert on issues of STI policy? What constitutes evidence and evidence-based decision making in science-intensive policy* *areas? Do these factors differ from other policy areas and how do they change over time and in different contexts?* We need more study of "public reasoning" and "reflexive government".

b. *What kinds of evidence, expertise, and models are actually used in science, technology, and innovation policy decisions? If evidence is not used, why? How are uncertainties and tacit assumptions in this knowledge base evaluated and factored into decision making?* For example, policy has yet to act on the results of global climate change research. The economic theory of "market failure," widely used as a rationale for science policy, does not provide guidance on resource allocation.

c. *What is the empirical support for the major assumptions underlying STI policies? Where that support is limited, what data and empirical approaches would shed more light on the policies?* Many scientists believe that allocating resources to research in areas that are scientifically interesting will maximize the social benefits from investment in science. This claim neither derives from theory nor has solid empirical support.

d. *How can the use of knowledge and expertise by different types of actors in various areas of policy be made more accountable and transparent in democratic processes?* Science policy needs to engage in dialogue with a range of American peoples and to benefit from "the intelligence of democracy." Social science research can contribute to the identification, analysis, and potential resolution of multiple, conflicting styles of reasoning.

5. Science, technology, innovation, and global change. Economic, political and social relationships are changing at global level. Historical studies have examined similar dynamics in the 17th through 19th centuries, and have much to contribute to examining this phenomenon now. The concepts of "country" and "nation state" are historically constructed and historically changing, even though most science policy is "national."

a. *What roles are science, technology, and innovation playing in those changes and vice versa?* For example, recent European science policy discussions have focused on where global firms do their R&D. Economic theories do not provide a rich enough understanding of global processes; we need social theories as well to understand the place of science and technology in the dynamics of global change.

b. *When different ways of knowing come into conflict in this process, how are the conflicts resolved?* For example, who owns indigenous knowledge and how can it be incorporated on an equal footing into contemporary innovation processes? What roles do actors outside government play in global processes?

c. *How are the structures and networks of science, technology, and innovation that extend across national boundaries changing options for national governments and the citizens they serve?* For example, will the global information infrastructure break down the first world/third world divide in science?

d. *What national visions and goals move towards greater well-being for everyone in the emerging world order, and how can those visions and goals be implemented?* Images of progress almost always include roles for science and technology institutions. The U.S. needs a new understanding of its place in the world order.

e. *How do STI policies and policy-making processes differ between the United States, Europe, China, Japan, and other countries? What are the implications for national and international well-being of these differences?* For example, government in the U.S. is traditionally indifferent to expertise in bureaucracies rather than outside government in civil society. How do S&T strategies differ in developing countries? Social scientists have an opportunity to apply what is known about the role of expertise in designing global governance institutions.

E. Methods, measurement, and data

Workshop participants agreed that appropriate research approaches to address this agenda will be methodologically diverse, ranging from normative to descriptive studies and from discourse analysis, history, and ethnography to community-based research and quantitative approaches. This group placed less emphasis on models than on understanding of process, and less emphasis on measures than on empirical evidence understood more broadly.

Approaches from the humanities are crucial to this research effort. Many of the workshop contributions in general included consideration of values, ethics, and responsibility, and affirmed the importance of asking broad, framing questions about the directions policy processes are setting. Addressing these issues without contributions from philosophy is inconceivable. Likewise, many use history, and Maienschein's paper in particular provides several compelling examples of the insights to be derived for current policy issues from historical studies, another indispensable contributing field.

Several participants have done their research through participation in the phenomenon they are studying, like Star and Bowker's work in understanding computer-facilitated collaboration through their collaboration with scientists and involvement in projects. Miller also shared his experience in having an impact in a policy area through a long-term combination of research, collaboration, and consultancy. Frickel stressed the importance of connecting to community stakeholders in the policy process through action research, like that supported by the Environmental Justice research program at the National Institute of Environmental Health Sciences.[19] Comparative studies will be needed, and cross-national collaborations should be encouraged. Several workshop participants urged NSF to strengthen its processes for supporting such collaborations.

While not limiting itself to quantitative models, the group called for the development of new theoretical frameworks, methodologies, and indicators. For example,

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[19] The Environmental Justice Research Program at the National Institute of Environmental Health Sciences: http://www.niehs.nih.gov/translat/envjust/envjust.htm, accessed on March 22, 2007.

Cozzens called for ways to think systematically about social as well as economic processes of globalization, including science and technology in that analysis. Rhoten called for pushing beyond patents in the measurement of innovation. Lucena's recommendation for an alternative science and engineering indicators volume to characterize the broader vision of science policy received enthusiastic endorsement from the group. In a related proposal, Sampat urged economists to focus more attention on developing measures of spillovers, rather than direct returns, on investment in various fields of science.

The pluralistic methodological range the group recommended pointed to the need for data and infrastructure that built qualitatively as well as quantitatively unclear. For example, archives of case studies could be very valuable in several of the recommended research areas. Collaborative tools to share experiences across the ethnographic and community-based projects could also be useful. More open and machine readable access to grant records and affordable access to publication data would help the work.

F. Community and capacity building

The workshop participants made a discovery as they moved through their discussion. Starting from a sense of the efforts in this field being scattered and weak, the group gradually realized that by drawing together the various strands of working they represented, there was already significant strength in the relevant scholarship.[20] Furthermore, their vision was to expand the community even further through collaboration with interested scientists and engineers. While the roots of the intellectual effort lie in many disciplines, the key to building a significant body of research knowledge was to work together, asking key general questions and integrating answers from a variety of perspectives. The observations earlier in this report about maintaining dialogue between researchers and policymakers applies to this integration activity as well.

A significantly expanded research effort in this area should be accompanied by community and capacity building efforts, including sharing of best practices, graduate training opportunities and program-building, data access and development, workshops, conferences, and public outreach. There was a feeling that a new generation of contributors to the field needed to be trained in the interdisciplinary and integrative modes the group was recommending. New forms of graduate training are necessary that prepare scholars to engage not only in interdisciplinary research, but also trans-disciplinary research. Where multi-disciplinary research encourages scholars from different disciplines to work together on common problems, interdisciplinary research encourages full collaboration (rather than, for instance parallel or sequential work) but rooted in home disciplines. Transdisciplinary science, however, emerges from a process in which collaborators from multiple disciplines (for example, social and natural sciences) develop shared conceptual frameworks that reflect the methods, theories and contributions of each discipline but creates a new synthesis. Creating such scholars

[20] Several noted that in Europe, the science studies and science policy communities have remained integrated, while in the U.S., they have largely diverged. This divide will need to be closed for the U.S. research effort to contribute maximally.

requires new kinds of graduate training; such programs would be, for instance, consistent with the sorts of integrative and innovative graduate education encouraged by NSF's Integrative Graduate Education and Research Traineeship Program (IGERT) grants. Insights from this research field should also be included in both formal and informal science education activities,

In addition to encouraging new forms of education NSF can play a major role in encouraging new forms of teamwork and collaboration, particularly those that involve mutual engagement across the social sciences, natural sciences, and engineering. New people need to do new things in new ways. Consistent with other NSF funding initiatives, such as *CYBERInfrastructure* and *Human Social Dynamics*, NSF funding initiatives can play a powerful role in creating funding incentives for truly interdisciplinary and transdisciplinary research. Transdisciplinary science faces considerable cultural, structural and organizational barriers. By creating such research opportunities and revamping the evaluation of these efforts, NSF sends a strong legitimating signal to relevant policy makers, research organizations and administrators and may topple some of these barriers. To support, strengthen and encourage interdisciplinary and transdisciplinary team science, NSF needs to reconsider and reconfigure the associated review processes and types of funding. For instance, this type of research often requires funding for longer-term start-up projects that make them appear high-risk. Creating these new intellectual networks will be aided by a renewed commitment on the part of funding agencies such as NSF to high risk, long-term research. The peer review process for interdisciplinary and transdisciplinary research must recognize and reward team, not just independent investigator, research projects. The SES research community can and should play an active role in helping NSF develop alternative formulations of multidisciplinary and transdisciplinary theories and models of science.

G. Summary and conclusions

The workshop papers reflected many of the points made above. They pointed out that

1. Research is needed on the political, economic, social, and historical contexts and processes that underlie knowledge production and science policy. Research at multiple levels employing multiple methods is ideal.
2. Greater understanding of the development and dynamics of science policy agendas is needed.
3. The range of values that infuse science policy must be acknowledged and understood.
4. The effectiveness of more flexible, responsive funding structures that can accommodate fast-paced changes in science and technology, as well as in organizational research configurations, should be considered.
5. The science policy research field should be global in its scope, community, and research. Understanding what makes us similar and what makes us different must incorporate knowledge of culture and place.

6. The science policy community should encourage and enable public debate, acknowledging the importance of public participation in science policy along with the availability of data and information, the variety of credible ways of knowing, and the emergence of a variety of stakeholders.
7. The collection, categorization and availability of data must be improved.
8. The science policy research field should be multi-disciplinary (many disciplines), inter-disciplinary (integrative across disciplines), and trans-disciplinary (transcending disciplines). Accomplishing these goals requires fostering the development of productive intellectual and disciplinary synergies and identifying barriers and incentives.

In summary, the workshop participants were enthusiastic about meeting the challenges presented by the NSF initiative. They saw significant contributions coming from many fields of the social sciences and from the areas of the humanities that devote themselves to examining science and technology in context, and they urged stronger and more systematic collaboration with scientists and engineers in developing new questions, methods, and insights. They embraced the opportunity to open a dialogue with science policymakers, and are ready to bring a new generation of researchers into the field. The workshop offered its best wishes, as well as its advice, to the NSF staff who would continue to nurture the effort.

APPENDIX 1: WORKSHOP PARTICIPANTS

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Kamau Bobb [not able to attend, but contributed paper]
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APPENDIX 2: WORKSHOP AGENDA

NSF Workshop on
The Social Organization of Science and Science Policy
July 13-14, 2006

The workshop will explore the social science foundations of science policy in the context of today's complex, global, and technologically-mediated society. Understanding the fundamental social processes involved in the structure and organization of science policy are crucial for maximizing the ability of science policy to enhance scientific development and innovation. This understanding requires more than an examination of economic inputs, outputs and the rational deployment of economic resources towards scientific goals; these criteria are necessary but not sufficient. In order to fully understand the formulation, acceptance, dissemination, and impacts of science policy, we also need to understand its *social organization* and the political, economic, and sociological context within which science policy and science succeed or flounder.

The goal of the workshop is to identify central research questions through dialogue among an interdisciplinary group of scholars from the social sciences that are working on aspects of science policy. Workshop participants will examine areas of convergence across arenas of inquiry, identify gaps in research and knowledge, and specify the most promising issues and areas most ripe for systematic and rigorous inquiry. The workshop will examine the relations among the social organization of science, policy knowledge networks and innovation and productivity. The workshop will also explore existing theories, methods and measures to determine which require further development or testing. Finally the workshop will discuss the critical next steps regarding research funding, development of data resources, and education and training. Workshop recommendations will be published in a report and posted on the Web.

THURSDAY, July 13, 2006

8:30 Welcome, Background, Introductions and Workshop Objectives
Wanda Ward, Deputy Assistant Director, Directorate for Social, Behavioral, and Economic Sciences
Edward Hackett, Division Director, Social and Economic Sciences

9:00-10:30 Session 1. Science Policy: Institutions and Issues
Chair: Felice Levine

- What is science policy? What role does it play in national life?
- Is U.S. science policy distinctive in content or process?
- How do differences in organizational, cultural, economic, social, political, informational and resource environments affect the development and implementation of science policy?

Dave Guston
Jane Maienschein

Irwin Feller
Philippe Laredo

10:45-12:15 Session 2: Knowledge and Innovation Processes
Chair: Jane Maienschein

- What do science studies offer to the understanding of innovation?
- What do we know about how science policies affect the practice and content of science?

Scott Frickel
Geof Bowker
Diana Rhoten

1:15-2:45 Session 3: The Institutions of U.S. Science
Chair: Irwin Feller

- Are the major institutions that do research in transition? If yes, from what to what?
- What factors are challenging those institutions? What role does policy play in those challenges?
- Are the trends particular to the U.S. or more general? Why?

Sheila Slaughter
Dan Sarewitz

3:00-4:15 Session 4: Cultures, structures and networks of knowledge production in the conduct of research and inquiry
Chair: Tom Gieryn

- What organizational factors affect the conduct of research? i.e., what factors enhance or inhibit trust? Shared meanings? Opportunities among scientists across disciplines?
- What roles do competition and collaboration play in the conduct of research?
- What ever happened to disciplines and specialties? Are these concepts still relevant?
- What makes careers in science worth earning, having, and keeping?

Jason Owen-Smith
Kamau Bobb
Cheryl Leggon

4:30 – 5:30 Observations from Designated Commentator Tom Gieryn, followed by general discussion

FRIDAY, July 14, 2006

8:30-9:30 Session 5: Social processes and the generation of data: elements, categories and indicators
Chair: Geof Bowker

- What are some of the different ways in which we produce, share and disseminate knowledge and data?
- What are some of the ethical challenges of these relationships?
  Leigh Star
  Rosalyn Berne

9:30–10:30 Session 6: International Context
Chair: Philippe Laredo

- How are changes at the global level likely to affect U.S. research?
  Juan Lucena
  Susan Cozzens

10:45-12:00 Session 7: How can the social sciences inform science policy?
Chair: Dan Sarewitz

- What are the most fruitful ways for social science to inform policy making in general and science policy in particular?
  Ned Woodhouse
  Clark Miller
  Bhaven Sampat

1:00-2:30 Session 7: Taking Stock and Setting an Agenda
Discussion Leaders: Beth Rubin, Patricia White, Priscilla Regan and Fred Kronz, NSF
Discussion among all participants

Concluding Remarks

APPENDIX 3: PAPERS PRESENTED BY WORKSHOP PARTICIPANTS

WORKSHOP ON
THE SOCIAL ORGANIZATION OF SCIENCE AND SCIENCE POLICY

Consolidated Essays

National Science Foundation
July 13-14, 2006

Workshop on Social Organization of Science and Science Policy
July 13-14, 2006

Session 1. Science Policy: Institutions and Issues

Science Policy: Institutions and Issues

David H. Guston
Arizona State University

Introduction

Three years ago I participated – as did several at this meeting – in a workshop sponsored by NSF on "Research Policy as an Agent of Change." Anticipating an "agents of change" program within the Human and Social Dynamics initiative, and influenced by the presence of strong contingents from both innovation studies and science and technology studies, the participants of the earlier workshop concluded that:

1. A focused effort to study research policy as an agent of change (RPAC) is urgently needed.
2. Studies of RPAC will require the use of diverse research methods informed by a range of disciplinary, interdisciplinary, and multidisciplinary perspectives.
3. NSF should explore ways to encourage research in this area.

I would like to frame my remarks as a reflection of the RPAC conclusions, in the context of the current discussions about, as an earlier document describes it, a "Science Metrics Initiative: Towards a Science of Science Policy" (SSP).[21] I'll do my best to provide some perspective on the questions intended to spur discussion in this first panel, namely:

- What is science policy? What role does it play in national life?
- Is US science policy distinctive in content or process?
- How do differences in organizational, cultural, economic, social, political, informational and resource environments affect the development and implementation of science policy?

I conclude that we need a research program that I will call a "political economy of research policy" that is broader than SSP and more akin to RPAC, but that is well-oriented toward decision contexts, more akin to SSP than to RPAC.

What is science policy? What role does it play in national life?

The first issue to note is that the RPAC group defined its intellectual space as "research policy" rather than "science policy." As most of us recall, Harvey Brooks classically resolved the ambiguity in science policy by defining "policy for science" and "science in policy" as separate, albeit interacting, domains. "Research policy" appears to do away with that ambiguity by holding to one

[21] I do this at the acknowledged risk of offending a host. I do not know who authored SSP -- the copy I received was without attribution – and I don't even know if its distribution was authorized.

aspect of it only, as the RPAC report defines it as "a strategy for achieving developments of new knowledge, new forms of expertise, and new infrastructures." The RPAC report, however, expands greatly upon this definition by looking to the vast array of actors, institutions, and strategies that are involved in knowledge, expertise, and infrastructure, e.g.:

- The array of substantive areas pertaining to public and private investment in research and development (R&D), including commercialization and technology transfer, the safe and ethical conduct of research, the organization of scientific activity and its reward system, etc.
- The activities of scientific advisory bodies and regulatory agencies, universities and hospitals, standard-setting organizations and professional bodies, courts and legislatures – all of which shape knowledge creation, diffusion, and use.
- The variety of institutions – across the sciences, the professions, administration, and politics (or what Don Price called the four "estates"), as well as intermediary or boundary organizations, manufacturers, venture capital, insurance and re-insurance, NGOs, media, civil society and social movements – that are involved in any aspect of knowledge creation, diffusion, and application.
- The range of mechanisms, from public budgets and other legislative action, to court rulings, non-state policies, patterns of practice, and the direct, indirect, or unintended consequences of any of these mechanisms on the knowledge enterprise.

The SSP document limits science policy to public and private investments in R&D – still narrower than RPAC's research policy, which is itself really only half of science policy, canonically defined.[22]

RPAC recognizes that "[s]cience and technology are integral to major social, political, economic, and environmental transformations, with significant implications at local, national, and global scales." In search of greater clarity about this integral relationship, the earlier group argued that "RPAC should encompass studies that treat research policy as an independent variable and studies that treat it as a dependent variable." The SSP document restricts the scope of interest entirely to the economic, articulating its majro goal "to reach a point where the nation's public and private sectors are able to evaluate reliably the returns they have received from past research and development (R&D) investments in science and engineering and to forecast, within tolerable margins of error, likely returns from future investments." It thereby treats research policy as an unproblematic precursor to the more important and uni-dimensional "investment," that is, the budget for R&D – rendering science policy analysis

[22] One might also note that some of the most important and creative areas of science policy research of late have in fact been those dedicated to understanding how to do better with the research, science, and technology that we have and are making, e.g., the Decision-making Under Uncertainty program and the Center for Nanotechnology in Society, that is, with what might be called "science for science in policy."

almost exclusively a budgetary tool rather than, say, a technology assessment capability.

Although both RPAC and SSP share an interest in science metrics, RPAC's vision includes not only data about investments and returns, but also historical and policy archives and, importantly, "better measures of the effectiveness of research policy" that are not necessarily limited to return on investment. Indeed, the effectiveness of research policy requires an understanding of outcomes that are connected with the goals of the research itself – which, for better or for worse, may not include yielding an economic return. For example, research policy may have as a goal military supremacy, or social connectivity, outcomes that are problematic to measure in the investment frame, or reduced child mortality or a smaller gap between rich and poor, outcomes for which the economic measure is somewhat beside the point.

SSP does, however, envision an

> integrated approach [requiring the] understanding and assessment of the underpinnings and ramifications of innovation among individuals, organizations, and societies. It will require identifying the stages and feedback mechanisms that influence R&D directions, processes, products and outcomes, and their relationships to individual, economic, and social well-being.

Here SSP seems to envision something that I encouraged RPAC to consider, what I called a "political economy of research policy" that would take into account the full cycles of transformations that create public and private values from knowledge socio-political activity, and back again. But, according to the timeline provided in the SSP document, it is not until the third year of work that the program would grapple with a "fundamental understanding of invention and innovation as psychological and social processes." This framing thus treats the non-economic aspects of innovation as secondary to the economic ones – either because they are less or important or because they may be more difficult to analyze. While the latter may be true, the analysis of the non-economic aspects will neither become more tractable nor become competitive with the analysis of the economic aspects by postponing research on it.

Is US science policy distinctive in content or process?

Both RPAC and SSP recognize a global and regional perspective on science policy, in addition to the national one on which they focus most attention. SSP envisions US science policy as deeply embedded in an international system, acknowledging that "it will be advantageous to approach this undertaking in light of efforts in other nations and international agencies (such as OECD) to deal with similar concerns and with an eye to the globalization of science and technology."

Similarly, RPAC recommends significant "comparative studies of research policy" as well as attention to transnational organizations like WHO and IPCC.

Neither agenda speaks to what is or may be distinctive about the content or process of science policy in the US per se. But to the extent that innovation is not simply the product of the inexorable accumulation of technical know-how and the impersonal operation of economic laws – that it involves historical factors and path dependencies such as the availability of particular resources at particular historical moments, cultural factors such as attitudes toward risk or food or religion, developmental factors such as levels of education and styles of governance, institutional factors such as the independence of the university sector or the strength of intellectual property laws, or the relative degree of trust in or alienation from governing decisions by the populace – the RPAC agenda is a much more promising one for bringing out national distinctions that might lend insight to policy making.

Despite the great intellectual ambitions of RPAC and SSP, neither agenda is wholly capable of managing what might be noted as one of the greatest distinctions in US policymaking, particularly with respect to science but apparent in other areas: its relative indifference to the expertise institutionalized in public bureaucracies rather than that offered by the plural groups of civil society. This critique is not limited to any alleged troubles in the current administration, but rather to a broad phenomenon that embraces many characteristics of the US system including the proliferation of think tanks, most with partisan affiliations, since the 1970s, the decline of offices of analysis in public bureaucracies over much the same period, the stable level of legislative staffing over recent decades (after a decline) despite the arguably more complicated and technology-laden legislative agenda, and the use of the adversarial system for a great share of what amounts to technological choice in this country. Whether a new research agenda will be shaped like RPAC or SSP or something else entirely, where will this information go? And can the agenda be structured to be maximally useful in a political system that is minimally interested?

It is likely that SSP is somewhat better poised than RPAC in this regard because, limiting itself largely to economic analysis within an investment framework, it hews more closely to the language of its potential patrons and, moreover, is more tightly linked with the kinds of explicit decisions that they are already making, in institutional contexts that are already clearly defined, e.g., the federal budget process. Although we lack, as Frank Baumgartner notes in his RPAC essay, a overarching research policy, we do have both a budget for research and a multitude of uncoordinated research policies. There are policy contexts within which RPAC analysis would certainly be useful, and perhaps even more significant for societal outcomes. But these contexts are more likely to be involved in agenda setting and problem framing, and therefore less likely to be institutionalized than the budget allocation issues. They are also more likely to be decentralized decisions about how to manage inquiry and knowledge-based innovation, and less likely to be centralized decisions about large-scale resource allocation. If, however, as the SSP document hints by imagining research centers dedicated to analyzing the return on investment in particular disciplines, the goal is not merely to use the economic analysis for large-scale budget priorities but for more micro-level decisions, then one might quite seriously worry that the ambitions of the two approaches would in fact be seriously in conflict and not potentially complementary.

How do differences in [various contextual] environments affect the development and implementation of science policy?

As one might infer from my remarks, I believe this question to be a critical one – not for science policy per se, but for knowledge-based innovation, and thus for science policy. Because the inputs to knowledge-based innovation are much more than a budget, and because the outputs from it, not to mention the societal outcomes – intended or otherwise – are much more than economic return, creating an evaluation scheme that focuses on only economic return threatens to displace goals toward generating only economic returns.

Recognizing, however, that these different contextual environments affect science and thus, by necessity, science policy is not the same as being able to turn this knowledge to instrumental use for science policy to guide science to being a more integral and more productive enterprise. This leads me back to my RPAC essay, which attempted to articulate a variety of influences – beyond economic returns – that could be traced to research policy:

- The organization and behavior of sciences, both through the creation of funding opportunities and programs as well as through the consequences of developing new research tools and technologies.
- The delivery of innovative goods and services – both those that are easily priced and thus measured and those that are not – as new knowledge is organized, transferred, absorbed, applied, and developed.
- The organization of public and private institutions around the performance of research.
- The changes among larger-order national and global systems through the rise and fall of industries, innovations in the technologies of warfare, increasing understanding of global environmental change and vulnerabilities, the ability to intervene in human health and reproduction, etc.

And, most importantly, the influence of these changes on the quality of life people across the country and around the world.

Although there are research agendas on most or all of these topics, my sense is that such research does not complete a full political-economic cycle that would create the most value from the research. For example, as little as we know about the dynamics of how researchers "follow the money," we probably understand even less about how that behavior cycles through scientific productivity, innovation, and demand for institutional change in the organization of research funding. As much as we know about the role of university-based research in regional economic development, we know far less and in a less systematic way about the influence of that role on political processes and outcomes in such regions and the subsequent changes on universities priorities and internal operations. Even if we can understand how research institutions respond to the structure of and incentives contained in research policy, we do not fully grasp how they react to the consequences of the knowledge-based innovations they then produce. To the extent that we include global perspectives in the research agenda on research policy, we still face the challenge of fully accommodating the diversity of global contexts into our analysis and the feedback from global conditions in local politics.

I hope that an initiative to bolster science policy analysis for science policy decision making would take into account the diversity of questions and approaches identified by the RPAC agenda, even while integrating them with the attentiveness to the decision context expressed in SSP.

On the Value of History of Science for the Social Sciences of Science Policy

Jane Maienschein
Arizona State University

To make progress in addressing the social scientific understanding of science policy, we need a clearer understanding of what the subject matter is. Following the strategy used by many people, asking Google "what is science policy" leads to a diverse range of answers. Most focus in some way on funding for science and technology. The assumption is that decisions about funding enable or constrain science and therefore make up policy, whether intentionally or accidentally. Some links point to the 2004 Gordon Conference on Science and Technology Policy, citing passages that I wrote for that website, and noting the need to raise the question and to get beyond "science policy as funding policy." We need to be clear what the domain of the study will be.

Two questions arise: (1) why is science policy so often treated as if it were a matter of funding only, or primarily? And (2) if science policy should really include a much wider range of discussions of funding, managing, regulating, presenting to the public, and other issues related to science, then how do we get at those? In both cases, historical perspective will be centrally important. That is my contribution to the discussion: I contend that the history of science should be a central part of informing our understanding of what science policy is and why it matters in the various diverse societies in which it exists.

Historical thinking matters for many reasons. Perspective helps us avoid looking myopically at only immediate issues and missing the larger contexts in which they reside and traditions from which they arose, missing forces and agents of change that will continue to shape future actions and values. Historical thinking keeps us from overreacting to what seem like urgent immediacies and instead allows us to reflect also on what is more deeply important. In particular, history illuminates the processes and forces that shape and effect change in science and policy. And, perhaps most importantly for our purposes here, historical case studies provide additional data to allow compilation and comparison. For example, if we want to understand how, why, and to what effect different nations formulate and implement policy, we can look at the US and USSR with respect to space exploration or computer development, following the historical examples in detail and documenting data about the process and impacts. History helps us understand what sorts of decisions have had what sorts of impacts, for example, and help us assess what sorts of outcomes measures have been available, reliable, and informative with respect to defined goals at a given time and place. Therefore, any effective social science of science policy must include history of science at the core, and not just as background that is then set aside.

I make this strong claim and offer some examples to support the claim. These are selected from work I know well. These are just examples, with the assumption that there are many others. Some historians already think about the questions we are raising here and have documented case studies available. Others will have to be pulled into the discussion, since they typically have not been welcomed among the social sciences. We do need more serious scholarly exploration in order to demonstrate what only historical study can show, namely: how has science policy changed over time, in evolving social, political, economic, and other contexts? What factors, or agents, have brought change, how, why, and to what effect? This initiative can help to attract historians of science and begin to show them how they can contribute most effectively.

For Example

Current "hot topics" include Global Climate Change, Fire Management, Human-Ecosystem Interactions, Embryo Research, and Space Exploration/Study, or to cut another way, Internationalism. These are just a select few initiatives with which I am most familiar and that are directly relevant to the objectives at hand.

- <u>Global Climate Change</u>: historian James Fleming will be a 2006-2007 AAAS Revelle Fellow, explicitly bringing the historical perspectives from a group of researchers to the Hill. Fleming studies the history of meteorology, and in Summer 2006 co-organized an intense week long seminar at the Marine Biological Laboratory to examine the history of "Oceans and Atmospheres." Meteorology, Oceanography, and Marine Ecology have all been highly economically and politically driven sciences, and the impacts of policies are clear. A group of 25 historians and scientists worked together to examine ways that policy decisions have been made (e.g., who gets to go to sea on a government funded vessel, which projects are funded and which are published by governments or others, what questions are deemed important) and how those processes have changed over time and in different contexts. The result is a much richer sense of how choices are made and why it matters. The study supports the conclusion that it will be valuable to have historians at the policy-making table, to avoid past mistakes in some cases but even more importantly to raise questions about underlying assumptions that we can see in the past more easily than we see them in ourselves.

- <u>Fire Management:</u> historian Stephen Pyne serves as president of the American Society for Environmental History. As the expert on fire management, Pyne reminds us that current Forest Management and National Park policies are misguided. Policies have swung from suppression-all-the-time to let-them-burn to let's-thin-every-forest-according-to-a-fixed-policy. Managers gather to consider the best science available, add "social factors" such as human desire to live and play in forests, and then generate formulas and policies. The US makes no serious effort, though some countries do a better job, to assess what we know about the cultural history of fire and forests. Pyne shows that well-established traditions in fire control shape the way social groups think about and work with fire. These can and should impact our decisions today. We ought not to start with the assumption that there is some forest science that tells us what we must do. Cultural historians as well as historians of science need to be part of the policy making process.

- <u>Human-Ecosystem Interactions:</u> the head of NSF's Biology Directorate James P. Collins studies environmental impacts on biodiversity. An international research team that he directs has shown that amphibian decline results, at least in part, from emerging infectious diseases. They recently applied a model based on historical comparative data to predict that a particular geographical area would be impacted next, based on patterns of change to date. When their prediction was fulfilled, they have begun to ask what follows? Ethically, what should they do when they can predict the human impacts on biodiversity? What policies can and should be implemented to effect what results? From their immediate perspective, it seems obvious that they want to stop the decline of amphibian populations. But they realize that they need help from social scientists, and social historians and historians of science and of science policy to understand how to effect social change, how to educate the public – and to what end? They need collaborative teams of scientists and social scientists to understand, to analyze, to evaluate and to implement appropriate actions to achieve results that are accepted as desired. Collins is working with the AD for NSF's Social and Behavioral Sciences Directorate to establish a program in Biology and Society to promote such studies. Collins already recognizes that we need historians and other social scientists at the table. The challenge in this case is how best to shape what they do there.

- Embryo Research: historian Jane Maienschein is working with a team including Manfred Laubichler (history/biology), Jason Robert (philosophy/bioethics), Daniel Sarewitz (policy), and Gary Marchant (law) at Arizona State University to develop The Embryo Project. In collaboration with the Max Planck Institute for the History of Science in Berlin, we are building a database of what we know about embryo research – focused on 6 selected episodes over the past 150 years and documenting all aspects of contributions to science and technology (including biographies, along with the social, institutional, legal, ethical, religious, policy, and other contexts shaping the science. The goal is to bring together knowledge and ways of knowing normally separated by disciplinary boundaries. We anticipate impacts on public understanding of embryo research, as we develop more educational products. Already, we see impacts on judges (through educational programs Maienschein has carried out), the general public (through programs by Maienschein and Robert), and in one important state court ruling where a judge drew on materials provided by The Embryo Project to rule that a frozen embryo is not a human being and therefore its loss cannot be murder in any sense. We wish that the US Congress would embrace a richer understanding of what embryos have thought to be and why that matters before they vote on issues of stem cell research regulation and funding, for example, and in the future we would hope that The Embryo Project can help. This complex project will take some time to bring to full effect, but the central presence of historical thinking is already making a difference for the network of 25 participants involved in the project.

- Space Exploration/Study: NASA historian Steven J. Dick has established a fellowship through the American Historical Association and is developing another with the History of Science Society to promote historical study of space science and exploration. The goal is to develop a rich understanding of what we have learned from exploration of space, which is expected to help NASA justify its existence. By showing what we have learned from the past, and particularly what sorts of investments and decisions have had what sorts of measurable impacts, the hope is that NASA can better shape future decisions and investments. Dick's own work has shown the results of past investment, by governments and others, including most recently an analysis of the exploration of origins of life questions. Dick's roles formerly at the Naval Observatory and now in NASA's History Office show that historical study can be taken seriously by policy makers and can help shape decisions made. His emphasis now is on bringing others into the discussion to strengthen and extend the impact.

- <u>Internationalism:</u> The history of science field has long recognized the importance of internationalism in science, and a number of now classic studies from the 1960s and 1970s sought to demonstrate the impact of investment (government and private) in different countries and cultures from the past. These studies largely focused on the 17th through 19^{th} centuries. For the 20^{th} century, we have a diversity of different studies, which have tended to look more at particular individuals, labs, ideas, techniques, or social contexts. The general sense has been that it is too difficult to capture "the science of 20^{th} century Germany" (or the US or USSR or whatever). Instead we get studies of molecular genetics in Germany, or nuclear physics in the US or France. The History of Science Society has taken another look, however. The most recent issue of the Society's *Osiris* is edited by John Krige and Kai-Henrik Barth, as *Global Power Knowledge: Science and Technology in International Affairs*.(http://www.journals.uchicago.edu/Osiris/journal/contents/v21n1.html) Barth was inspired in part by his previous work on Capitol Hill to pull together examples of individualized studies to begin to develop a collective sense of our historical understanding of science and society. This should inspire other historians to join that discussion, and hopefully inspire policy-makers to invite historians to their discussions. We can and should educate scholars in all areas to reach out and learn from other scholarly approaches and other perspectives.

Therefore:

These are just a few select examples of the value of history of science for our social sciences understanding of science policy. Unfortunately, historians of science have not done a terribly good job of making our case. We do not make our studies widely accessible, nor do we do a good job of explaining why they matter. Fortunately, the History of Science Society now has an activist Executive Committee dedicated to taking the history of science beyond our own disciplinary boundaries. The British Society for the History of Science is making similar moves.

This is a time of great opportunity. We can draw on history of science to bring reflection and insight. It is not the case that every new issue in science brings completely new problems and must be addressed from scratch. Nanotechnological advances, to pick one example, have much in common with previous innovations, so we will gain by always including assessment of what we know and what we can know from past episodes. Furthermore, this is a time of great need. We need a wide range of data, since any claim will be better when tested against a wider range of cases. Social scientific study of science policy will benefit from the many historical cases that can be compiled and compared with current cases. Thus, we can learn from history. We gain by bringing historians of science, with their historical knowledge and their historical thinking, to policy discussions and to the social science study of science policy.

Notes on A Social Science Research Program Directed at the Scientific Study of Science Policy

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Introduction

One can approach the workshop's stated goal of identifying "crucial research questions through dialogue among an interdisciplinary group of scholars from the social sciences that are working on aspects of science policy" from the perspective that there is nothing new under the sun and that will be pouring old wine into new bottles, or from President Grover Cleveland's dictum, "This is a condition we face, not a theory".

This brief essay contains elements of both perspectives. It starts from the premise that the questions posed in Dr. Marburger's Science editorial- "Wanted: Better Benchmarks", such as "How much should a nation spend on science: What kind of science", etc. are "perennial practical problems of U.S. science policy, and that in explicating theoretical frameworks and methodologies in the course of advancing any new research agenda, we inevitably will be treading oft traveled paths. Indeed, among this workshop's participants are several of the individuals who first broke the trail and its offshoots. But Dr. Marburger's editorial also serves as a new start. It represents a de facto assessment that earlier answers have been forgotten, ignored, or viewed as unsatisfactory, or some combination of all of the above. It provides a true challenge to the social sciences to think afresh about knotty problems, with the potential existing for high intellectual and pragmatic returns. It involves a collective responsibility on our part to assist NSF and its Directorate for Social, Behavioral and Economic Sciences produce findings of policy relevance as well as of academic interest lest we inadvertently reinforce the views of those in the Congress who hold little brief for the social and behavioral sciences.

The need for brevity, as well as professional specialization, dictates that these notes focus primarily on "policy for science", less so "science in policy". This emphasis is not intended as a weighing of the relative importance of these two research traditions in setting forth any new agenda. Thuler, Wheeler, and Finson's 2005 analysis of competing perspectives in public characterization of risk assessment and Mooney's, The Republican War on Science are but two examples of the important analytical and political issues associated with how science is used/ignored/or misused in public policy decisions.

Approach

I draw upon a collection of National Research Council and National Science Foundation committee and panel reports, participation in recent workshops similar to this one, as well as ongoing research in the US and internationally on much the same benchmark-type questions presented in Dr. Marburger's editorial to generate the research questions below. The rationale for this sample frame is that "charges" to NRC committees come from congressional committees and federal agencies (which, in turn, frequently reflect background congressional committee directives or needs to meet Executive branch requirements, especially those derived from OMB's PART processes). In effect, the charges represent a real-time, grounded overview of the science policy agenda that Federal and state agency policy makers, as well as representatives from selected major constituencies, such as universities and firms, affected by these decisions are currently working on or with which they are grappling.. The findings and recommendations from these reports (some of which are currently in draft, and thus cannot be directly cited) in turn can be interpreted as representing the "expert" judgments of mixed assemblages of a multidisciplinary array of researchers and practitioners on contemporary science policy issues.

The "problem" oriented generation of this list of questions overlaps with the topic of interdisciplinarity in 2 important but distinct ways. First, "interdisciplinarity/multidisciplinarity" –what is it; how to organize it; how to assess its quality--itself constitutes a contemporary science policy question, and is the subject of several recent NRC reports. (e.g, NIH Extramural Center Programs;, Large-Scale Biomedical Research). Second, I take it as given, or so I believe my writings have indicated, that eclecticism in drawing upon insights, methods and measures from different social and behavioral science disciplines increases one capacity to unravel knotty intellectual problems as well as to address questions posed by policy makers, including suggesting to them the need to reframe these questions. Thus, the agenda below freely moves across disciplinary boundaries without bothering to note their presence.

Crucial Research Questions

My bumper sticker statement of contemporary crucial research questions is that (1)they relate primarily to ex ante rather than to ex post questions, and (2)that they may be subsumed under the headings of "organizational design and mechanism design".

(1) I interpret Dr. Marburger's questions as a quickening of renewed interest in the "criteria for scientific choice" problem, frequently associated with the Weinberg criteria. This new interest in turn induces a relatively lessened "research" emphasis on ex post, programmatic evaluations, which have dominated policy interest, research funding, and methodological and data base developments for the past decade.

This shift may seem counterintuitive in light of the attention devoted to OMB's R&D Investment Criteria and the PART process and the current mantra of evidence-based decision making. It also may reflect the singular view of Dr. Marburger, who as a physicist has publicly stated the importance of "vision" in justifying large investments in fundamental science. It also may simply reflect a weighing of the accumulation of theories, methods and data sets available to assess existing programs, albeit more so technology than science programs, since the mid-1980s set against the meager support provided for studies of the science of science policy.

(2) Evident in an increasing number of NRC reports, the rationale underlying NIH's Roadmap Initiative, NSF initiatives, and developments in foundation funding of major academic research initiatives are concerns, assessments and actions that call into question the fundamental structures and operating procedures of the post-World War II system of Federal government support of basic science. In play, so to speak, is a system built about disciplinary-based modes of organizing and funding scientific inquiry, reliance on universities as performers of publicly funded basic research, the coupling of academic research and graduate education, and priority-setting and proposal selection processes based upon expert, peer-review procedures. Underlying these trends are macro-level questions about the ability of research universities to effectively revise their organizational structures, policies, and cultures, and micro-level questions about the determinants and impacts of differential rates of adjustment among institutions.

The workings of the peer review system, in particular, are coming under increasing question. The salient issues are less the historic but by now relatively well vetted ones of various forms of bias or distributive impacts, as studied for example by Cole or Cole and Chubin and Hackett. (e.g., Peerless Science). Rather, recently called into question has been the effectiveness of the system in identifying and selecting transformative science research, defined by the National Science Board as research "that has the potential to revolutionize an existing discipline through a paradigm shift or create a new one".) Also called into question is whether current organizational arrangements, such as study sections, are capable of or willing to discriminate among fields of inquiry in terms of relative "vitality".

If one accepts the validity of these criticisms, or at least is willing to entertain them as working hypothesis, then the question(s) arise as to what alternative(s) would be better. This is not a new question; indeed proposals for altering the structure and workings of the Federal agency review process already exist. Arkes, for example, has suggested converting panelist evaluations into z scores to minimize the influence of any panelists's unusually high or low standards. Jaffe has proposed randomly accepting and rejecting a sample of proposals that cluster near the cutoff point or pay line and then using a regression discontinuity approach to evaluate the impact on agency funding on the productivity of applicants. Rejection by agencies of these and related proposals also raises research questions about innovation and diffusion processes within government agencies involving modification of priority setting and proposal selection organizational arrangements and procedures.

The Vision Thing

Contemporary rationales for public support of science, especially basic science, are cast largely in terms of economic rationales and criteria: market failures; international economic competitiveness, endogenous growth theory. This emphasis is likely to continue. But it poses a set of questions related to (a) why support science beyond its promise of prospective tangible economic or other mission-relevant benefits; and (b) abstracting from promises of tangible benefits, how do scientific communities set their respective research agendas, and (c)how does one assess the relative merit/priority claims of different communities of scientists on public resources, a full circle return to the criteria of choice issue..

The question posed by (a) may be seen in the forthcoming NRC report, Revealing the Hidden Nature of Space and Time: Charting the Course for Elementary Particle Physics". The most immediate science policy recommendation flowing from the report is its advocacy that the U.S. make the necessary investments to become the host country for the International Linear Collider. For this NSF workshop, what I consider to be the most relevant and striking about the report is that although it notes the numerous economic benefits projected to follow upon siting the ILC in the US, it indeed is quite diffident about advancing its case on economic grounds. Instead, the report is suffused with a tone of excitement of the scientific opportunities in particle physics. Phrasing the claims of basic science for public support along similar lines, the Nobel-prize physicist, Steven Weinburg has written: "Some work on the most interesting problems of biological or physical science does have obvious practical value, but some does not, especially research that addresses problems lying at the boundaries of scientific knowledge. To earn society's support, we have to make true what we often claim: that today's basic scientific research is part of the culture of our times" ("The Boundaries of Scientific Knowledge" in Facing Up, p. 82)

If, as seems to be happening across several natural science and life science fields, "big science" proposals bulk large among national science policy choices, involving large, discontinuous allocation decisions within and between fields of inquiry, then we need to know more about the agenda setting within scientific fields. By this I mean something quite different from the laboratory or cultural studies approach to the study of science. Instead I am keying off here from the observation that "scientists address the most challenging problem they think they can solve". In effect, I am asking what constitutes challenging problems?; what determines what problems can be solved? Obviously, further unbundling is needed here, relating to the degree of consensus/dissensus within/between fields about specification of question and methodology, criteria and processes by which solutions are accepted within and across fields, and more. The point to this question though is that answers are likely to be found only through systematic research on the behaviors of scientists, singly and collectively.

Data

Subsumed within any positing of a research agenda is the existence and availability of reliable and valid data. At the simplest level, use and/or misuse of data serves to raise or lower the saliency of policy issues. The recent imbroglio over the accuracy of the data used to compare the number of engineering graduates between the United States and China in Rising Above the Gathering Storm is an important reminder of our collective widespread reliance on secondary data, even those of us who are empirically oriented and devote parts of our efforts to generating primary data sets.

Across a swathe of science policy issues, existing data sets are inadequate to formulate or test hypotheses. The recent NRC report, "Measuring Research and Development Expenditures in the U.S. Economy", for example, states that "The NSF research and development expenditure data are often ill-suited for the purposes to which they have been employed…Public policy attention to early-stage technology development, the Advanced Technology Program, and process innovation requires data beyond these basic components of R&D".

The need for improved data on multiple facets of the U.S. scientific and technological enterprise was detailed in a recent NSF-sponsored workshop. A report from the workshop will soon be forthcoming, so no attempt it made here to summarize its discussion or conclusions. What warrants repeating though for this workshop is that our collective scholarly capacity to test the research questions that we may pose is of modest value unless we can systematically undertake the studies that will answer them. Many of these questions require construction of data sets that do not currently exist, or at least in a form that would withstand critical scrutiny.

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Workshop on the Social Organisation of Science and Science Policy

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Four considerations about the "science policy" issue raised

The workshop aims at exploring "the social science foundations of science policy". The documentation received contains the now well known speeches of Dc. Marburger, the director of the US Office of Science and Technology Policy.
I was struck when reading these documents, by the issue raised. These papers are focusing on tools supporting decision making, on the need for metrics that are more specific than "science as a whole" and that are dealing with allocations between areas and topics. They also argue that allocations should take into account potential effects and anticipated returns.
The central issue is thus not to address the role of science in the economic and social dynamics, nor do we have to consider more specifically the role of social sciences. The question is about the capacity of social sciences to build instruments that help policymakers make the relevant (and if possible "optimal") "area allocations" within a given overall allocation.
I see four elements in this approach. Two deal with the global conditions for public intervention, and two consider the underlying rationales for public interventions with an intrinsic tension between both.

The global conditions first

(a) The texts de facto consider that the overall allocation is beyond the remit of the present discussion. It is interesting since we clearly have here a typical transatlantic divide (and even a transpacific one if we consider the policy argumentations underlying the growth of public expenditure in Japan during the 1990s). The total amount of efforts focussed on R&D has been central to the European debate following the Lisbon declaration. Europe should spend more – 3% of GDP – which has strong implications on the public level of expenditure which is today well below the targeted 1%. The fact that European countries at large have been unable to address this challenge might drive them to reconsider the target. However it is revealing that the debates concerning these changes are grounded outside the conditions of allocations per se, on both innovation and knowledge dynamics. The Ahoo report (2005) for instance does not elaborate so much on where we should invest research public money, it focuses on issues such as the evolving locations of R&D facilities of global firms (and thus conditions that build up the attractiveness of given countries or territories) or the role of innovation in "collective goods" (they speak of societal challenges) as a "public engine for innovation" (my interpretation) and it highlights the importance of standards and public procurement.

(b) The ways in which knowledge flows and circulates are not addressed once. We thus have to consider that (and this is the dominant view now expressed for over 15 years and promoted at international level) IPR and patenting are the "one best way" to

insure the circulation of knowledge and that potentially patentable knowledge should go on enlarging. It thus ignores completely the growing discussions and academic literature (including from neo-classical economics) on the topic. This covers issues such as variations in this frame (see the different forms of "general licence" around open source software, and its take-up even by large firms today), recent revocations of patents (such as those of Myriad Genetics) or experiments being developed in free access of knowledge for research (as this between agricultural research centres), not withstanding debates about the notion of "scientific commons".
The underlying rationales second. Here we are faced with a tension between the two de-facto models at work in official documents about US priorities. The tension is between the adoption of a "revised" linear model of innovation, and the continuation of a vastly "mission oriented" intervention which is rooted in the institutional setting of Federal R&D expenditure.

(c) In all the arguments developed I see one central underlying assumption: science (or said differently long term research activities) are at the core of future economic developments. This could be a generic assertion, covering all fields of science; we would then be back to the old 1960s argument beautifully developed by Arrow and Nelson. But this is not: there are areas of science that are more important than others, that can bear more fruits than others, and we should develop processes for identifying them and measures for quantifying the potential returns. Nothing new here for the analyst external to the US setting: we seem to rediscover the longstanding approach developed by the ATP (of course not a mainstream initiative in the US policy landscape, but able to mobilise numerous NBER scholars). It would be probably interesting to understand why these efforts were "fruitless".

However such a reasoning opens to one key issue. We know that, at any given point in time, there are highly differentiated rates of growth within science: for instance research in human genetics has consistently grown 8% per year during the last decade (OST 2004) and scholars looking at nano scale S&T announce two digits figures for the last 5 years (our own calculation stand at 14%, see www.nanodistrict.org) while the average growth of WoS publications stand at less than 2%. This has strong institutional implications for performing institutions willing to stay at the forefront of the world competition or even for funding bodies. How can we explain these? Is it enough to only mention researchers abilities? How much is it linked to the existence of adequate facilities or infrastructures? Should we simply recognise that they are successive waves and that after high energy physics, ICT and biotechnology, we face a nano wave? Or should we rather discuss the specific conditions of such waves in term of knowledge production and circulation?

Within the PRIME network I coordinate (see below for some explanations), we consider that the latter change "dramatically" from one wave to the other, and that applying recipes derived from the previous wave (such as reinforced patenting and start-up) might turn counterproductive, and even more applying the same recipe for all areas of science would be highly counterproductive. We thus need a theory of knowledge production that is "differentiated", taking into consideration the content of knowledge and the ways in which it translates into new products or services (collective or private).

For the latter, it is not enough to speak of networks and co-construction or co-shaping to locate public intervention. Account must be taken of processes through which both knowledge and new breakthrough products or services are produced. When using such approaches as those linked to changing paradigms or shifting "dominant designs" (which have the interest of gathering scholars from sociology, economics and management), it drives to consider institutional aspects dealing with nurturing variety (and multiple explorations), accompanying "crystallisation" for enabling given options to demonstrate their potential, and promoting selection mechanisms (through standards, regulation and other well know market shaping activities). There is thus much more than simply supporting academic research activities, and there is a need to revisit US processes, mechanisms and their location (e.g. the role of defense) before entering into more in depth modelling and data collection.

This also drives to a rephrasing of the initial question. How much is the growth we observe in given fields linked to available funding, that is to concentration of public funds on some areas. Could we for instance say that the biotechnology wave is in great part the result of the concentration of US federal funding on life sciences through the NIH? And thus that what is central lies in the political choices made and the "implementation structures" (or institutional settings) that render them lasting? This then links to my fourth consideration.

(d) Most of Federal funds for R&D flow through "mission oriented" organisations (first and foremost defense, energy, health and now security with environment and agriculture playing secondary roles). This corresponds to the very first OECD model (from the 1960s), and while there has been debates on the importance of adopting a "collaboration paradigm" during the Clinton years, this did not have any impact on either the structures of national labs (see Crow and Bozeman on the topic), nor the structures through which money allocations flow. This is in vast contrast with Japan, Korea or European countries (both individually and with the emergence of the European Commission as a player) which have undergone major institutional changes. This, once more, has strong implications. I see two of them: I shall address below capability building in social sciences and humanities. And I Shall concentrate here on implications at policy level.

How can one consider the adequacy of "area allocations" when priority areas depend on political priorities: public research money is allocated (in congress) not as one but following each mission, and is thus dependent upon the evolving mix of priorities in the overall budget. Is there not then a contradiction with the will to develop models to support policy making? Or should we consider that the questions raised and the models aimed at are specific to each "public mission area" (defense, energy, security…)? If the latter is the "de facto" model, it is then important to take stock of the US recent history (and in particular of NASA or ATP) before entering into any consideration about quantitative modelling and broad-ranging data gathering. It could be said that recent initiatives like the NNI cross departmental organisation, but this would require in depth examination, since implementation follows the traditional path. In short, should we discuss the interest of developing transversal, cross-cutting models when all policy making processes remain "departmentalised" (that is arbitrations are not made between fields of research but within the overall budget for each mission). And if so, are models interesting for allocating funds to given areas or for addressing the overall federal investment in science, an issue outside of the remit (see my first point)?

Discussing the potential role of social sciences and its "limited" impact on science policy

To address this issue, I shall focus on three considerations. First the present efforts and what the Federal government can expect from it. Second the present situation of the field (in Europe) and the on-going developments. And third their implications within the perspective set by OSTP.

First, capability building in the social sciences and humanities. To say it roughly (at this stage only sketchy figures are needed to make the point, they can be fine tuned later), the role of national labs both as performers and funders of academic research is acknowledged and assumed to be in the order of one third of total fundamental research performed in the US. But they support nearly no social sciences nor humanities. The latter are thus left to NSF to cater for them. We thus have to put into relation two simple figures: a share of the NSF budget (a budget which amounts to roughly 5% of the total US federal expenditure in R&D) with the share of social sciences and humanities in the total "public" human capital (using OECD definition of government and university research performers, and which, again roughly, amounts to 40%). A good case in point is to take "nanoS&T" and compare the total of the NNI 'co-ordinated' budget and the amount put by NSF in the social sciences dealing with this area… Either one considers that social scientists cost far far less, or one should simply recognise that public agencies only get what they have paid for, that is virtually nothing. This explains Dc Marburger's dissatisfaction, but roots it elsewhere, in the poor level of investment made. The answer to this is differentiated growth, that is focussing federal money on the topic and thus privileging a specific research community (even promoting it if it has not yet turned into an transepistemic one). This was probably the case 20 years ago, but no longer seems the case, at least in comparison with the European landscape.

Second, the present European situation. In Europe the role of innovation in economic dynamics has raised such issues that we have witnessed in the last decade quite a strong interest from policy makers in our area. However this has been mostly through "short-term" utilitarian studies or consultancy, driving to a very conservatist agenda, and very few new theoretical developments. We have thus argued for the need for rebalancing the research portfolio. We have been successful in pushing the deployment of a *specific programme* at EU level on the topic in the on-going 6^{th} Framework Programme. The agenda is both very general and sketched in term of well-established present-day concerns. Just to give an order of magnitude I have made a simple calculation of the efforts of the Framework programme for the 2003-2006 period. I arrive at over 50 million euros, accepting the fact that this only covers a fraction of the costs of the projects supported (I estimate this fraction at under 30% overall) and that Europe is fragmented (meaning that to this should be added all the national efforts on the issue (My estimate for the UK is that EC funded activities represent around one third of total activities in the field).

Within this overall agenda we have taken up the opportunity of a new instrument "networks of excellence" to propose that, besides project funding on topics of interest for stakeholders, there should be a space for:

a) discussing the long-term agenda of the speciality (and this is not easy as shown for instance by the difficulty we face to develop a meaningful review of the role of Defense)

b) organising at a collective level the "infrastructures of the field", mostly training (especially at PhD level) and indicators, with effects such as a new indicator of project funding (that OECD is now considering) and even more with the opening of a debate on a new family of indictors besides the OECD based input-output ones, positioning indicators (see PRIME position paper on the topic and the forthcoming Lugano conference).

c) re-opening a space for exploration of heterodox approaches and methods, and organising on a few topics we anticipate to be important in the future (but not taken-up by usual procedures) the first phases of collaborative research projects.

PRIME has been running for 3 years. It gathers some 50 institutions and over 60 research groups. It has a small budget (6 million euros over 5 years) which we now know is not enough to respond to the three dimensions mentioned. But we also have demonstrated (at least we think we have!) that these three aspects were missing and that the speciality as such is able to self-manage them (even if all is not that simple!).

Third, implications for coping with the perspective set by OSTP I gather two main institutional conclusions from this analysis. First that there is an issue of critical mass if OSTP and NSF want to develop a specific action, and this should not be overlooked (there is no interest whatsoever to develop a research programme under "business as usual"). Second a programme should differentiate between three levels: consultancy to the Government for implementing under present knowledge some of its ambitions (we call this "expertise" – with all the problems raised about ways of handling and undertaking it); targeted research on topics of interest for stakeholders where a shortage of knowledge is recognised driving to research operation (meaning that time lags are there recognised, and not grossly underestimated as too often) and long-term research (there are other ways than the one we are experimenting, but there is a need if we do not want to stick to present conceptual frameworks and their limitations to address issues raised).

This has clear implications: there are no short-cut that would enable to get significant results and relevant new models and datasets within 3 to 5 years. This is the result of long public underinvestment and this cannot be solved in the short term (a situation which would easily be accepted in any natural science but has difficulty to be dealt with for policy targeted social science).

Session 2: Knowledge and Innovation Processes

Science Policy for Resilient Urban Ecosystems

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I live and work in New Orleans. My city is hurting. So is the ecosystem that surrounds it. So are its universities, colleges, biomedical centers, and training hospitals that once sustained the region's intellectual capital base. Post-Katrina, what's left of these knowledge institutions offer conditions for a natural experiment on the 'political, economic, and sociological context within which science policy and science succeeds or flounders.' How can science policy help rehabilitate a region's shattered research infrastructure, direct inquiry to better understand the dynamic interface of the built and natural environment, and identify basic design principles for resilient urban ecosystems?

These are big science policy challenges. They extend far beyond the immediate plight of New Orleans,[23] and they require not just input but leadership from the social sciences. Getting there will mean building a 'social science of science policy' with a far broader focus than R&D investment efficiencies in globalizing markets. It will mean in-depth analysis of the ways that nature, technology, organizations, social and intellectual movements, and the state interactively shape knowledge growth. To understand those change processes better, that new science needs to be broadly interdisciplinary, drawing on the contributions of anthropologists, historians, philosophers, sociologists, and of course, economists, who study the social dynamics of science. Their research invaluably contextualizes and historicizes innovation processes in the knowledge political economy.

Social scientists have some way to go, but we're beginning from solid ground. We have good studies on the factors that condition the emergence of new ideas, new cultural practices, new forms of research organization, and we should extend that work more deeply into the policy arena. On the other hand, we know considerably less than we need to about why some knowledge is not made, or why some knowledge becomes lost in the policy-making process. An example of the latter: Louisiana ecologists have known for some time now that a major contributor to the state's vanishing wetlands is intense canalization and dredging by the oil and gas industry, but that knowledge is remarkable for its near-total absence in wetlands policy discourse. To understand why, a critical institutional perspective on science and science policy would need to encourage inquiry into stasis as well as change. It would need to encourage comparative work that takes into

[23] It may be tempting to discount the destruction and disorganization in New Orleans as a worst-case anomaly, but the levee breaks on the American River that flooded the Sacramento area this spring, and the recent rain-induced flooding that closed government offices in Washington D.C. and sent people in towns near Philadelphia to their roofs to await helicopter rescue caution against quick dismissal. In the context of rising sea levels, increased storm intensity, and aging urban infrastructure, many coastal cities here and elsewhere are similarly vulnerable (Cutter 2001; Pelling 2003).

consideration more of the broad range of science and engineering than is typical in our work – not just the high-stakes, high-profile research, but the mundane and the marginalized and the forgotten as well. It would need to pay closer attention to the ways that even 'successful' science and science policy can fail people, their communities, and their environment (see e.g. Frickel and Moore 2006).

Science Policy and Cumulative Disadvantage

My colleague Nancy Mock is a member of the Tulane School of Public Health and Tropical Medicine who specializes in complex emergencies and disaster management. She is studying recovery efforts from the December 2004 Indian Ocean tsunami and from Hurricanes Katrina and Rita. The comparison, as she tells it, is striking. In Sri Lanka, international aid went swiftly and directly to Sri Lankan communities, local businesses, and families. The resources enabled people and institutions to first survive the immediate crisis and then stabilize, clean up, and begin rebuilding. Infamously, much the opposite has occurred in New Orleans. 'The first principle of disaster recovery,' Nancy told me, 'is to get resources into the hands of locals who have the incentive, the local knowledge, and social capital to get things done. This includes the scientific community.'

Rebuilding the research infrastructure in New Orleans poses a fundamental challenge to the structure of federal science funding. Nearly one year out, we can see how the hurricane has amped up the cumulative disadvantage among those institutions most directly impacted by the hurricane. EPSCoR[24] is NSF's functional equivalent to social welfare, and if there was ever a need for a research handout, this is it. Of course, the program was never intended as a mechanism for serving this or any other region's suddenly acute scientific infrastructure and research needs. Its purpose, instead, is to keep promising researchers in science-poor states from starving – with a travel grant here, a visiting grant there, ten grand to set up a pilot project. Useful as they may be to individuals who receive them, EPSCoR is not designed to recruit new talent to the area or retain existing talent, which is vanishing about as quickly as our coastal marshes. Neither are the Small Grants for Exploratory Research (SGER) earmarked for Katrina research. Only six percent (N=7) of the 114 grants funded thus far are based in New Orleans-area institutions and a third of those cover remediation and/or infrastructure costs, not actual research. A simple way to insure that this program would make more of a regional impact would be to require that Katrina SGER grants include a PI from one of the impacted Gulf states.[25] Another would be to institute a special class of small grants that allow researchers in the impacted region to buy off courses so they can instead spend a year collecting rapidly disappearing data.

One NSF funding mechanism that seems better suited to tackle the complex problems Katrina brought to New Orleans, and which would provide a boost to the local knowledge economy, is the Long-Term Ecological Research program administered through the Biology Division. Currently, NSF funds two urban-based LTERs, in Phoenix

[24] Experimental Program to Stimulate Competitive Research
[25] To date, twenty-eight SGERs have been awarded to institutions in Alabama, Louisiana, and Mississippi.

and Baltimore. Metropolitan New Orleans is an urban ecosystem ripe for an LTER. In fact, Tulane researchers were recently invited by the program officer to submit a proposal. The problem? The structure of the granting process is at sharp odds with ecological processes now underway in and around the city. LTERs are multi-institutional, multi-disciplinary, multi-project grants that take two to three years to write. Our urban ecosystem is in a state of intense flux. Three years from now, biological and ecological evidence that would help scientists answer fundamental questions regarding urban ecosystem vulnerability and resilience will be gone; an opportunity for producing cutting-edge science greatly curtailed. The funding process is also at odds with the university's current institutional capacity. LTERs are highly competitive. In stable organizational environments, the potential payoff for university programs with solid histories of federal support is obvious. Under present local conditions, however, researchers at area universities are at a distinct competitive disadvantage. Since Katrina, the organizational field itself acts as a structural disincentive for research capacity building at the local level.

It's no secret that in science money matters, but in the long term, the systems that distribute that money matter more. From my vantage point 'down the bayou,' building a more flexibly responsive funding system should be a high national priority – responsive to sudden shocks, like Katrina, and responsive to the slower drift of social values, like those embodied in the terms resilience and sustainability.

Interdisciplinary Contexts for Knowledge Production

Another of the science policy lessons that Katrina teaches is the importance of the interdisciplinary contexts of decision making. Levees that are designed and built by engineers with little attention to what geology has to say about the hydrodynamics of subsurface soil structures are a tragic case in point. Another is environmental testing that is organized with little thought to the social history of contamination. These are examples of the way that basic/disciplinary knowledge forged in university contexts can misinform scientific practice in applied/interdisciplinary settings. They illustrate both how difficult it is to shed the disciplinary tethers that come attached to knowledge, and the need for institutionalizing interdisciplinary contexts at the site of knowledge production.

NSF has considerable experience promoting interdisciplinary collaborations of various sorts. Some focus on specific technologies that can function as 'boundary objects' linking research from different fields (see Star and Griesemer 1989). We see this, for example, in the urban ecosystem LTERs' heavy investments in Geographic Information Systems (GIS) as tools for spatial analysis of ecological and social data. Another approach to interdisciplinary collaboration is ELSI, which examines the 'Ethical, Legal, and Social Implications' of genome and nanotech research. A third approach is illustrated by the Research Coordination Networks in Biological Sciences program (RCN). These 5-year grants foster interdisciplinary collaboration through networking strategies organized around broad life science themes or research questions.

Implicit in each approach is a differently limited conceptualization of interdisciplinary knowledge and each definition presents a correspondingly different challenge. With GIS, interdisciplinary knowledge is linked spatially, such that outcomes of social/geographic and ecological analysis are paired. The challenge is to integrate these data conceptually. Too often, it seems to me, GIS is used to provide a "human dimensions" context for the analysis of ecological change, but theoretical integration or synthesis is rare. ELSI offers a sequentially additive vision of interdisciplinary knowledge, with ex post social analysis of technology. The challenge from within ELSI is for getting questions about the unforeseen consequences of new technologies considered further up the research stream in a way that integrates social analysis with technical design/research processes. The RCN approach is built around an idea rather than a specific technology or policy issue and probably goes furthest in allowing interdisciplinary knowledge to emerge organically. The programmatic challenge is to extend the RCN's scope explicitly to the social sciences. This is not a facile challenge, but one that asks natural scientists to accept and find value in our work, the successful construction of a Latourian "detour."

A social science of science policy will need to integrate these and other approaches to maximize the long-term viability of synergies that result from interdisciplinary attention to ecosystems, urban systems, and knowledge systems. New Orleans is now a model laboratory for this very thing.

Gray Knowledge into Policy (and Back Again)

We need to know more about where the knowledge that informs public policy comes from. My suspicion is that it isn't primarily from basic science laboratories in universities, even though this assumption is the basis of most science policy: direct funding into promising basic research in hopes that the information derived from that research will enable policy makers to make sound decisions (in addition to indirectly boosting national economic performance). But is that how the process really works? My guess is that it isn't and that a lot of policy – I don't know how much, but probably too much – is grounded in 'gray knowledge' based on research that is neither peer-reviewed nor published.

The two main sources of gray knowledge are government and industry laboratories. In his study of the politics and social organization of salmon biology, Rik Scarce (2000) argues that state fisheries biologists produce most of the research that informs salmon management policy in Washington State, and that very little of this information ever makes it into the open scientific literature. The university salmon biologists that Scarce interviews worry that this practice leads to short-sighted policy that ultimately damages the economic and environmental health of the region's salmon fisheries. Gerald Markowitz and David Rosner's (2002) study of the history of medical research and knowledge suppression in the vinyl chloride industry is a chilling example of gray knowledge informing national environmental and occupational health policy in deeply damaging ways. New Orleans' own levee system provides another case study. We now know that the final design decisions involved the combined interactions of two Army Corps of Engineers offices and some local engineering firms, but no university engineers, geologists, hydrologists, or urban planners. Of course, now the city is crawling with university experts who have spent thousands of person-hours walking the levees and reviewing blueprints, memos, and decision trees to pinpoint where, when, and by who mistakes were made. A little late, but oh well.

If we want to maximize the social impact of science policy, we could do worse than study these out-of-the-way venues where research is often designed with specific policy questions in mind, and where organizational cultures that have little to do with science can do much to shape knowledge outcomes that in turn reshape public policy, including science policy.

Conclusion

I argue for a more responsive and flexible research allocation, for more meaningfully collaborative work across the social and natural sciences, and for a better accounting of where science policy knowledge comes from. These goals reveal contradictory tensions in science and science policy. A call for interdisciplinary research is constrained by a federal funding system for research organized by disciplinary divisions. That same system reproduces another boundary between basic and applied knowledge that flies in the face of use-oriented or problem-centered knowledge. The adjudication of research through peer review does not flow seamlessly into public policy where notions of fairness and social justice take different cultural meanings. The catastrophic destruction in New Orleans magnifies these tensions and underscores the urgent need for a social science of science policy that confronts these and other pressing concerns.

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Scientific Data: A Policy Perspective

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a) *Ownership of Scientific and Technological Ideas and Data*

It has often been asserted that science is a public good: meaning that scientific work does not fit into the globally dominant market economy. In the new knowledge economy, however, we are increasingly seeing the penetration of the market right down to the molecular level, right down to the stuff of scientific enquiry. Thus it is possible to patent genes, genetically modified plants, animals and so forth. In this process, there has developed. Taking a fairly wide definition of ownership, we can see three main sets of issues arising from the implementation of this knowledge/information market: control of knowledge; privacy; and patterns of ownership.

By control of knowledge, I refer to the question of who has the right to speak in the name of the science. Since the mid-nineteenth century this has been a fairly simple question to answer: only professionally trained scientists and doctors can speak for science and medicine in turn. Only they had access to the resources that were needed in order to speak authoratively about a given subject – they had the journals, the libraries, the professional experience. Within the new information economy this is not the case. For example, many patient groups now are being formed on the Internet. These groups often know more about a rare condition (for example, renal cell carcinoma) than a local doctor does – they can share information twenty four hours a day, and can bring together patients from all over the world. This flattening out of knowledge hierarchies can be a very powerful social force. It carries along with it, though, the need to educate the enfranchised public about critical readership of the web. There are many websites which look official and authoritative but in fact only push the hobby-horse of a particular individual. We have through our schools and universities good training in reading and criticizing print media; but we have little expertise as a culture in dealing with highly distributed information sources.

Privacy concerns are a significant dimension of science and technology policy in the new economy. It is now technically possible to generate and search very large databases, and to use these to integrate data from a whole series of domains. As this happens, the potentialities for data abuse are increasing exponentially. Much has been written, for example, about data mining of the Icelandic population. After much public debate, citizens of Iceland agreed to sell medical and genealogy records of its 275,000 citizens to a private medical research company. There were two central reasons for choosing Iceland: it has a population that has a relatively restricted gene pool; and it has excellent medical records dating back some thousand years. While the science may prove useful (the question is open); it certainly opens the specter of genetic screening of prospective employees by a given company. It is extremely difficult to keep records private over the new information infrastructure – many third party companies, for example, compile together data from a variety of different agencies in order to generate a new, marketable form of knowledge. There is no point in trying to adhere to the old canons of privacy; however open public debate and education about the possibilities of the new infrastructure are essential.

Thirdly, we will look at patterns of ownership of information/knowledge. Science has frequently been analysed as a 'public good'. According to this line of argument, it is in the interests of the state to fund technoscientific research since there will be a payoff for society as a whole in terms of infrastructural development. With the increasing privatization of knowledge (as we turn into a knowledge-based economy), it is unclear to what extent the vaunted openness of the scientific community will last. Many refer back to a 'golden age' when universities were separate from industry in a way that they are not today. While a lot of this talk is highly exagerated (science has always been an eminently practical pursuit) it remains the case that we are in the process of building new understandings of scientific knowledge.

A key question internationally has been that of who owns what knowledge. This is coming out in fields like biodiversity prospecting, where international agreements are in place to reimburse 'locals' for bringing in biologically active plants and so forth. However, the ownership patterns of knowledge of this sort are very difficult to adjudicate in Western terms. For example, consider a Mexican herbalist selling a biologically active plant in a market in Tijuana. He owns the plant, but is not the source of knowledge about biologically active plants. This knowledge does not go back to a single discoverer (as is needed in many Western courts of law adjudicating matters of ownership of intellectual propoerty) but to a tradition held, often, by the women of a collectivity. The herbalist may well not be able to trace back the chain of ownsership that goes back to the original harvesintg of the specific he or she is selling. Similarly, Australian aborigines or the Native Americans had very different concepts of land ownership from the white settlers; leading to complex negotations that continue today about the protection of natural resources. We need anthropological/sociological studies of local knowledge (to the extent to which this is being mined by scientists) again in order to help design just frameworks and studies of issues of data ownership in different countries. There is a danger when we talk of the explosion of information in the new knowledge economy that we forget the role of traditional knowledge in the development of sustainable policies for a region. Thus research has shown that management of some parks in the Himalayas has relied on models brought in from the outside and taught to villagers through the distribution of television programs – while at the same time ignoring centuries of local ecological knowledge because it is practice based, and has its own intricate weaving of knowledge about the environment, religious belief and mythological expression and cannot be easily conjured into a form that can be held on a computer.

Sharing Data

The form of scientific work which has been most studied by sociologists of science is that which leads from the laboratory to the scientific paper by means of the creation of ever more abstract and manipulable forms of data, which Latour has dubbed 'immutable mobiles' (Latour, 1987). In this process, there is no need to hold onto data after it has been enshrined in a scientific paper: the paper forms the 'archive' of scientific knowledge (frequently adopting names redolent of this storage ambition, such as the Archives for Meteorology, Geophysics and Bioclimatology). The scientific paper, which is the end result of science, contains an argument about an hypothesis (which is proved or disproved) and a set of supporting data which is, saving a controversy, taken on faith by the scientific community. The archive of scientific papers can then be indexed both in terms of arguments made and information stored.

However, over the past twenty years we have seen in a number of new and of formerly canonical sciences a partial disarticulation of these two features of scientific work. Increasingly, the database itself (the information stored) is seen as an end in itself. The ideal database should according to most practitioners be theory neutral, but should serve as a common basis for a number of scientific disciplines to progress. Thus one might cite the human genome initiative and other molecular biological projects as archetypical of a new kind of science in which the database is an end in itself. The human genome databank will in theory be used to construct arguments about genetic causation of disease, about migration patterns of early humans, about the evolutionary history of our species; but the process of producing causation is distinct from the process of 'mapping' the genome – the communities, techniques and aims are separate.

This disarticulation, which operates in the context of producing a working archive of knowledge, is not in itself new. To limit ourselves arbitrarily to the past two hundred years, a significant percentage of scientific work has been involved with creating such an archive. Napoleon's trip to Egypt included a boatload of geologists, surveyors and natural historians, and reflected a close connection between the ends of empire and the collection of scientific knowledge. Thus also Smith's geological survey of Britain or Cook's travels to Australia. Richard's (1996) The Imperial Archive presents some wonderful analysis of the imperial drive to archive information in order to exercise control (a theme familiar of course to readers of Latour). The working archive is a management tool. What is new and interesting is that the working archive is expanding in scale and scope. As Michel Serres (1990) points out we are now as a species taking on the role of managing the planet as a whole – its ecosystems and energy flows. We now see nature as essentially only possible through human mediation. We are building working archives from the submicroscopic level of genes up through the diversity of viral and bacterial species to large scale floral and faunal communities and the mapping of atmospheric patterns and the health of the ozone layer. There is an articulation here between information and theory, but the stronger connection is between information and action – with competing models based on the same data producing policy recommendations. In this new and expanded process of scientific archiving, data must be reusable by scientists. It is not possible to simply enshrine one's results in a paper, the scientist must lodge her data in a database which can be easily manipulated by other scientists.

In the relatively new science of biodiversity, this data collection drive is achieving its apogee. There are programs afoot to map all floral and faunal species on the face of the earth. In principle, each of these maps should contain economic information about how groups of animals or plants fend for themselves in the web of life (http://curator.org/WebOfLife/weboflife.htm) and genetic information (about how they reproduce). In order to truly understand biodiversity, the maps should not only extend out in space but back in time (so that we can predict how a given factor – like a 3 degree increase in world temperature – might effect species distribution). Very large scale databases are being developed for a diverse array of animal and plant groups and the SPECIES 2000 programme of IUBS, CODATA and IUMS has proposed might eventually be merged into a single vast database of all the worlds organisms. NASA's Mission to Earth program is trying to "'document the physical, chemical, and biological processes responsible for the evolution of Earth on all time scales'" The UK Systematics Forum publication *The Web of Life* (Forum, 1998: 25) and quotes E.O. Wilson invocation: " 'Now it is time to expand laterally to get on with the great Linnaean enterprise and finish mapping the biosphere'" and speaks of the need to: "discover and describe the Earth's species, to complete the framework of classification around which biology is organized, and to use information technology to make this knowledge available around the world'". These panoptical dreams weave together work from the very small scale molecular biological to the large-scale geological and temporally from the attempt to represent the present to a description of the history of all life on earth. They constitute a relatively direct continuation of the drive for the imperial archive, where the notional imperial archive sought to catalog completely the far-flung social and political empire in order to better govern it, biodiversity panopticons seek to catalog completely the natural empire, for much the same reason. Although they work as oligopticons – covering only a thin slice of species and environments - they are created to be, and are manipulated as if they were, panopticons.

The information collection effort that is being mounted worldwide is indeed heroic. Databases from far flung government agencies, scientific expeditions, amateur collectors are being integrated more or less successfully into very large scale searchable databases. Science and technology policy analysts have a significant contribution to make to the process of federating databases in order to create tools for planetary management. We can produce means to engage the complexity and historicity of data within the sciences so that social, political and organizational context is interwoven with statistics, classification systems and observational results in a generative fashion. We need to historicize our data and its organization in order to create flexible databases that are as rich ontologically as the social and natural worlds they map and so which might really help us gain long term purchase on questions of planetary management.

Even if we can name everything consistently, there are the problems of how to deal with old data and how to ensure that one's data doesn't rot away in some information silo (in Al Gore's memorable phrase) for want of providing enough context. The problem with much environmental data – is that the standard scientific model of doing a study doesn't work well enough. In the standard model, one collects data, publishes a paper or papers and then gradually loses the original dataset. A current locally generated database, for example, might stay on one's hard drive for a while then make it to a zip disk, then when zip technology is superseded it will probably become for all intents and purposes unreadable until one changes jobs or retires and throws away the disk. There are a thousand variations of this story being repeated worldwide – more generally along the trajectory of notebooks to shelves to boxes to dumpsters.

When it could be argued that precisely the role of scientific theory as produced in journals was to order information – to act as a form of memory bank – this loss of the original data was not too much of a problem. The data was rolled into a theory which not only remembered all its own data (in the sense of accounting for it and rendering it freely reproducible) but potentially remembered data which had not yet been collected. By this reading, what theory did was produce readings of the world that were ultimately data independent – if one wanted to descend into data at any point all one had to do was design an experiment to test the theory and the results would follow.

However, two things render this reading of the data/theory relationship untenable. First, it has been shown repeatedly in the science studies literature that scientific papers do not in general offer enough information to allow an experiment or procedure to be repeated. This entails that in a field where old results are continually being reworked, there is a need to preserve the original data in as good a form as possible. Secondly, in the biological sciences in general – and the environmental sciences in particular, the distributed database is becoming a new model form of scientific publication in its own right. The Human Genome Initiative is resulting in the production of a very large collaborative database, for example. In the environmental sciences, where the unit of time for observing changes can be anything from the day to the millennium, there is a great value in having long, continuous data sets. The problem of what data to retain in order to keep a data set live is a metadata problem; and as Ingersoll et al. note: "the quality of metadata is probably the single most important factor that determines the longevity of environmental data" (Ingersoll, Seastedt et al., 1997,310).

Science is an eminently bureaucratic practice deeply concerned with record-keeping, as Latour (1987) reminds us in *Science in Action.* Disciplines do mixed jobs of keeping track of their own results over time – indeed a key finding of science studies has been that using 'theory' as a way of storing old, and accounting for potential data, can be highly problematic since replacement theories do not automatically account for all the data held in the outgoing one (the locus classicus is Kuhn, 1970). The difficulties become apparent when you move beyond the arrangement and archiving of data within a given science to look at what happens in the efforts of a vast number of sciences (working from the scale of molecular biology on up to that of biogeography or even cosmology) to coordinate data between themselves within the field of biodiversity. In practice, the sciences use many differing 'filing systems' and philosophies of archival practice. There is no automatic update from one field to a cognate one, such that the latest classification system or dating system from the one spreads to the other. Further it is often a judgment call whether one needs to adopt the latest geological timeline, say, when storing information about ecological communities over time; particularly if one's paper or electronic database is structured in such a way that adopting the new system will be expensive and difficult. Such decisions, however, have continuing effects on the interpretation and use of the resultant data stores.

There has been relatively little work dealing with the organizational, political and scientific layering of data structures. It is clear, though, that the assignation of an attribute to the world of discourse or of materiality is shifting, post hoc. Information infrastructures such as databases should be read both discursively and materially; they are a site of political and ethical as well as technical work (cf Bowker and Star, 1999, Chapter 1); and that there can be no a priori attribution of a given question to the technical or the political realms.

Practically, this means that it is a policy priority to pay attention to the work of building very large scale databases, or developing large-scale simulations. It is no longer the case that knowledge held in a particular discipline is enough to carry out scientific work. From the 1940s on (with the Manhattan project) one might say that large scale technoscience is inherently massively multidisciplinary. However, scientists are not trained to share information across disciplinary divides. And computer scientists cannot do the work of translating between disciplines. Indeed, one of the major difficulties with developing new scientific infrastructures using computers is that the work that is interesting for the computer scientist is often very high-end: involving, say, the latest object-oriented programming and visualization techniques. However, the work that is important for the scientist might be theoretically uninteresting for the computer scientist: for example, producing good ways of updating obsolete databases. There are two sides to the solution here. One the one hand, career paths must be developed which are more in tune with the needs of technoscience. This has worked successfully with the training of a cadre of bioinformaticians with the human genome project at the University of Washington. This cadre knows both molecular biology and computer science – and has a possible career path outside of the confines of the traditional disciplinary structure. On the other hand, we need to put the maintenance of the information infrastructure high on the agenda. Many scientists will go for grants to get the latest equipment; few will concern themselves with upgrading old databases. Huge amounts of data are being lost this way – data about the effects of human activity on this planet which is essential if we are to build a workable future. Just as software re-use has become the clarion call of the latest revolution oin programming techniques, so should data-re-use become a clarion call within technoscience.

H.

a) International Technoscience

There has been much hope expressed that in the developing world, the new information infrastructure will provide the potential for a narrowing of the knowledge gaps between countries. Thus an effective global digital library would allow third world researchers access to the latest journals. Distributed computing environments (such as the GRID, being developed in the United States) would permit supercomputer grade access to computing to scientists throughout the world. The example of the use of cell-phone technology to provide a jump in technology in countries without landlines has opened the possibility of great leaps being made into the information future.

As powerful as these visions are, they need to be tempered with some real concerns. The first is that an information infrastructure like the Internet functions like a Greek democracy of old – everyone who has access may be an equal citizen, but those without access are left further and further out of the picture. Further, access is never really equal – the fastest connections and computers (needed for running the latest software) tend to be concentrated in the first world. This point is frequently forgotten by those who hail the end of the 'digital divide' – they forget that this divide is in itself a moving target. Thirdly, governments in the developing world have indicated real doubts about the usefulness of opening their data resources out onto the Internet. Just as in the nineteenth century, the laissez-faire economics of free trade was advocated by developed countries with most to gain (because they had organizations in place ready to take advantage of emerging possibilities) so in our age, the greatest advocates of the free and open exchange of information are developed countries with robust computing infrastructures. Some in developing countries see this as a second wave of colonialism – the first pillaged material resources and the second will pillage information. All of these concerns can be met through the development of careful information policies. There is a continuing urgent need to develop such policies.

International electronic communication holds out the apparent promise of breaking down a first world/third world divide in science. With develops like the remote manipulation of scientific equipment (see U Mich's UARC - upper atmospheric research collaboratory - project, where scientists on the internet can manipulate devices in the Arctic Circle without having to go there. The possibility of attending international conferences virtually is also being held out. And if universities succeed in wresting control over scientific publications from the huge publishing houses (a very open question) then easy/cheap access to the latest scientific articles becomes possible for a researcher in outback Australia... . At the same time, there are a number of forces working to reinforce the traditional center/periphery divide in science internationally. Even with the move to open up access to scientific publications and equipment, there is no guarantee that the 'invisible colleges' - which operate informally and determine who gets invited to which conference and so forth – will change: indeed the evidence seems to be to the contrary. Further, at the current state of technological development there is a signficant gap between information access in different regions of any given country, or different parts of the world. Consider the analogy of the telephone. In principle anyone can phone anywhere in the world; in practice some regions have more or less reliable phone services, which may or may not include access to digital resources over phone lines.

We can go beyond the continuing digital divide, however, to consider the possibility of mounting very large scale scientific data collection efforts. Such efforts are central to the social sciences, and to the sciences of ecology and biodiversity. With the development of handheld computing devices, it is becoming possible for a semi-skilled scientific worker with a minimum of training to go into the field and bring back significant results. Thus in Costa Rica, the ongoing attempt to catalog botanical species richness is being carried out largely by 'parataxonomists' who are provided with enough skills in using interactive keys (which help in plant recognition) to carry out their work almost as effectively as a fully trained systematist. Computer-assisted workers together with the deployment of remote sensing devices whose inputs can be treated automatically hold out the possibility of scaling up the processes of scientific research so that they are truly global in scale and scope.

See also: http://dataaccess.ucsd.edu for an OECD working group report on sharing Scientific data

Innovation: All we really need to know we didn't learn in kindergarten?

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Nations everywhere have become once again focused on science and technology for development, with many now accentuating the importance of innovation. In the U.S., "innovation has become the watchword for our nation's future. It is both a rallying cry and a challenge, one that is now touted by every sector of society—industry, academia, and government" (Bement, 2006). Beyond rhetorical buzzwords raising the profile of innovation, there is evidence of political strategies targeting its potential. As early as 1999, the National Science Board noted a need to revitalize a commitment to innovative research (NSB-00-39). In 2000, the former NSB Chair, warning the Committee on Science's Subcommittee on Basic Research that "industry is increasingly dependent on the Federal government to support long term and high risk research at the same time that the Federal share of the U.S. R&D enterprise is declining." By 2003, the NSB began discussing ways in which it could help NSF develop new and more effective approaches to reviewing and funding both multidisciplinary and innovative research that has the potential to transform disciplines (NSB, 2006). And, in the last year, Arden Bement (Director, National Science Foundation) stated that:

> … we are living and working in a new age of scientific and engineering discovery. The conduct of science has changed—thanks in part to new information and communications technologies. Combine this with sensors, satellites, and other observational tools that supply ever-burgeoning streams of observations and data, and we have turned science fiction into reality. Work at the frontiers of discovery has accelerated. … Science is also growing more complex, and the boundaries between disciplines are blurring. This naturally makes interdisciplinary and collaborative research the norm rather than the exception (Bement, 2005).

Federal-level imperatives to innovate science and technology thus seem to revolve around assumptions about the interdependencies of industry-government-academia (Etzkowitz and Leydesdorff, 1998) and the importance of interdisciplinarity. In fact, together they are often seen as necessary, if not sufficient, conditions of innovation. As Gordon Kingsley (2004) reminds us, however, while "… the goal of policy makers is to achieve transformation, the goal of participating organizations seems to be more instrumental, anchored in their own institutional goals and missions" (p. i). Applying this wisdom to the issue at hand, the success or failure of policies – like ideas – in pursuit of innovation is not an abstract probability but one embedded in the institutional contexts, organizational conditions, and interpersonal relations that ultimately embody. And, though the linear model of innovation[26] is long dead in the science studies and policy communities (David, 1997), we lack analytic approaches that can adequately capture the dynamic co-evolution of scientific, technological and social systems and the interaction of individuals, organizations, and institutions that shape the iterative character of innovation.

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[26] Where innovation is perceived to stem from a uni-directional flow of information from science into technology.

In this essay I spend less time recounting what science studies has taught us about innovation to date and more time imagining what we might seek to learn going forward. I argue that gaps in our current knowledge about how cross-sectoral interactions and interdisciplinary collaborations foster innovation are a result of the fact the field of science studies turned its attention away in the 1970s from institutional level analyses towards micro-ethnographic, culturally interpretive, and socially constructivist approaches, rather than looking to integrate them. As a result, we know more about how innovation plays out at the level of the individual, the single organization, or a single set of organizations (e.g., network) but less about how the conditions, processes, or implications of innovation compare, contrast, and collide across different organizational sites (e.g., university campuses or departments, industrial firms or laboratories), intellectual domains (e.g., life sciences, materials sciences), institutional regimes (e.g., public domain, commercially proprietary), and operational models (e.g., individual-to-individual collaboration, organization-to-organization collaboration).

Technoscience and innovation

Earlier theories about the character and conduct of science believed that the practical payoffs and novel discoveries often thought to mark innovation were not predictable, that they came about primarily as a result of serendipity (e.g., Bush, 1945). It was also thought that the attention of policy discourses and the allocation of scientific resources should not be guided by the anticipation of or the expectation for innovation, but rather by the informed judgments of scientists about what constituted the most challenging and promising scientific questions. Under this mode of thinking, the idea of supporting scientific research with public monies and cumulating scientific findings in the public domain for the community to test and develop were considered the most effective steps toward fostering innovation and enabling its benefits (Merton, 1973; Nelson, 1993; 2004). Based on these theories, for much of the twentieth century, the financial structures, organizational arrangements, and the ideological commitments of scientists themselves preserved the loose boundary between Mertonian communal science and market commercial research in the U.S. (Guston and Keniston, 1994; Stokes 1997). This dynamic gave shape and substance to the conception of science as a public and common good and of scientific knowledge as the ‘source’ for downstream technological advancement and innovation.

Science has changed markedly in recent decades, however. Transformations in the subjects, institutional locations, incentives, and legal supports of scientific research have altered how science is practiced and how scientific knowledge is shared, particularly in the life science and information technology fields which have been the loci of many of these recent shifts. In these fields (and ever more again in materials science), universities—once home to the strongest claims of public domain science—have increasingly allied with commercial firms, bringing growing areas of the research enterprise into the competitive and proprietary knowledge economy (see various works by Jason Owen-Smith and/or Walter Powell). As a result, long-standing separations between commercial and non-commercial research cultures have blurred, and the dividing line between science and technology has faded. In the new world of technoscience, the republic of science – where historically scientists trade predominantly in reputation made possible by the free circulation and reproduction of research through publication – intermingles with the republic of technology – where firms trade primarily in proprietary knowledge produced for economic ends made transactable and appropriable by patents and other intellectual property tools (Dasgupta and David, 1994).

The growing interdependencies of these once different and separate scientific and technical communities and the resulting opportunities for individual scientists and their institutional "rules of the game" (Nelson and Nelson, 2002) to intermingle raise implications that should cause us to rethink our view of innovation in the twenty-first century. Most significantly, while technical progress is clearly and strongly shaped by scientific progress, the conceptualization that ideas only flow "downstream" from the sphere of science to the domain of technology must be recast.

This is not a particularly novel insight on my part but one that raises a number of critical questions regarding how the complex social organization of science and technology interact and overlap as well as co-evolve and co-produce as part of the innovation process (Murray, 2002). Only a few studies have really exemplified the more nuanced view of this "whirlpool" of, or overlap between, the production (science) and the application (technology) as mutually reinforcing and each shaping the other (Garud and Rappa, 1994; MacKenzie, 1992 in Murray, 2002). And, there have been few if any studies that truly explore the more multiplex nature of the interconnections between the scientific and technical communities. Yet, in light of the common understanding that institutions, organizations, and individuals of science and technology are ever more richly and deeply embedded in inter-sectoral and interdisciplinary collaborations, there exist a number of intriguing and unexplored issues essential to our rethinking, including that should be taken up in future work, including for example: What is the true extent of overlap between science and technology? What are the conditions and processes that shape their interaction and co-evolution? Which of these conditions or processes lead to successful innovation processes or products? How do institutional norms of the public domain commingle with norms of the patent economy and even newer norms of the "open source" community? How do these different valences of "openness" condition the ways scientists elaborate and innovate research? What evidence traces or better captures the multi-level and iterative nature of these interactions and innovations? Finally, how do these processes and outcomes vary across different disciplinary domains and/or organizational landscapes? Much attention has been given to studying innovation models in highly commercializable and patentable areas of research—such as, for example, biotechnology, artificial intelligence, and photonics/optics. However, far less attention has been paid to the dynamics of fields with low commercializable or patentable potential, or fields with much more routinized pathways for the commercialization of research. And, so too has little attention been given to considering how changes in patterns of science and technology may be affected by the social value rather than commercial potential of innovation in some fields. Consider that, traditionally, university settings explored arenas that industry did not pursue. But, today, it is not obvious where innovation for the public interest and social good may come in such areas as vaccines or low cost technologies. In some circumstances, new models of technoscience have fostered the development of first-to-the-world medicines and affordable communications technologies, but in other realms, such as renewable energy or cancer, widely available breakthroughs have not been common.

Interdisciplinary collaboration and innovation

There is a vast literature in the sociology modern science about how interdisciplinary research is and should be organized, (see, for example, Barmack and

Wallen, 1986; Robertson, 1983), how scientists behave in interdisciplinary collaboration (see, for example, Crow, Levine and Nager, 1992; Qin et. al., 1997), and how such activities could be facilitated through better management (see, for example, Hurley, 1997; Sapienza, 1995). Literature in the sociology of science often discusses the role of interdisciplinary collaboration as antecedent conditions of discovery and innovation (see, for example, Gibbons et al., 1994; Hollingsworth and Hollingsworth, 2000). But empirical work is only beginning to examine how and where interdisciplinary collaboration transpires, when and why it produces exceptionally original or innovative results, and whether and how the nature or the structure of such collaboration might be institutionalized.

Thus, while there is agreement in principle that interdisciplinary collaboration is a valuable antecedent condition to innovation, there is relative absence of explanatory – rather than simply descriptive – analyses (or meta-analyses for that matter) examining how collaborations of this kind actually lead to innovation in practice. The literature suggests three identifiable but intersecting antecedent conditions – organizational context, intragroup structure, and individual relations – that may influence the potential innovative success of collaborations but little empirical work can substantiate the power or generalizability of claims made around them. For example, on the one hand, rival ideas of disciplinary diversity are thought to enhance the innovation potential of collaboration. On the other, they are said to increase the functional distance[27] that separates collaborators and reduces their technical overlap, thereby impairing their potential communication and integration. Thus, in this sense, the very strength of an interdisciplinary collaboration may also be its very undoing if a common vocabulary, a common understanding about means and purposes, and a foundation of reciprocal exchange, trust and respect are not formed. But, where is the empirical tipping point (so to speak)? What is the right balance of diversity and complementarity? Likewise, earlier research suggests that individuals are more likely to interact with others where gains are made in one's cultural capital and are more likely to innovate when investments are made in constructive emotional conflict (see, for example, Collins, 1998). However, more recent research by De Dreu and Weingart (2003) found that the predicted positive relationship between emotional conflict and group performance on innovation did not hold up despite these existing narratives. What is the right amount of productive friction (Hagel and Seely Brown, 2005), and how does this vary across organizational, interrelational, and individual conditions? Can individual source analyses repeated over and over under different conditions lead to summative and generalizable conclusions about composition of collaboration and prediction of innovation?

Measuring innovation

Finally, just a quick word on assessing the presence or absence of innovation.

[27] Functional distance is a measure of both the epistemic differences as well as the cognitive boundaries between disciplines. The interdisciplinary practices literature identifies functional distance between disciplines that use different vocabularies, concepts, beliefs, methods, and modes of inquiry as the most significant factor affecting the type of knowledge transfer, conversion and integration that characterizes interdisciplinary collaboration (Rhoten 2003).

Conceptually, the notion of innovation can refer to a new way of doing (process), or the result of a new way of doing (outcome), which solves a particular problem or leads to a number of smaller advances toward solving a particular, larger problem (Rosenberg, 1994 in Hollingsworth and Hollingsworth, 2000). For any process to be classified as innovative, it entails the introduction of a new or radical research idea, the development of a new or radical methodology, the application of a new or radical instrument or invention, or a new set of ideas and information that answer old research questions or raise new ones (Polanyi, 1966). For any scientific output to be considered an innovation, it must have an inherently practical component, it must confer intended benefit at a level beyond the individual scientist, and it must be new to the individuals or the organizations engaged in its development (West and Farr, 1990 in Anderson, De Dreu and Nijstad, 2004: 148).

Standard approaches and indicators for measuring measure innovation most often center around patent, survey, and bibliometric analyses. The weaknesses of these approaches and indicators are well-known as are the difficulties in deriving alternative measures. So, while I don't wish to revisit the vulnerabilities of these techniques, I do hope to raise the flag just a bit higher in the search for new ways to operationalize and capture the often intangible but impactful conceptual, intellectual, and social innovations that are identified in current definitions of innovation but which often go undiscovered in traditional analyses. For example, Paul Thagard (1992) argues that conceptual innovations in science are reflected in the use of new concepts, data, and methods to produce more coherent explanations. Likewise, Kiesler and Cummings (2005) identify new ideas and research tools as the major research outcomes to consider when measuring innovation. In this regard, especially important is the amalgamation of data, methods, and tools into new ensembles of research tools and technologies that open new spheres of research and/or change old research paths (Rheinberger, 1997; Hackett, 2005). Finally, innovation has implications for social transformations that may entail the development of new knowledge value collectives (Rogers and Bozeman, 2001), invisible colleges (Crane, 1972), and scientific/intellectual movements (Frickel and Gross, 2005). Neither patents nor papers will necessarily capture any of these phenomena? How can we capture them in neat ways that allow for explanatory and comparative analyses? Innovation has been argued to restructure individual cognitions and directions, working group roles and practices, team structures and styles, etc, but we have little systematic data to demonstrate how, where, and when?

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Session 3: The Institutions of Science

Political Coalitions and Support for Science & Technology Funding[28]

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As science and technology become more central to national and global economic development the question of science and technology (S&T) policy formation re-emerges. The present National Science Foundation initiative—"Building the Science of Science Policy: Innovation, Investments and Outcomes"—thus far is taking a rational approach, trying to determine the human dynamics that underlie innovation, concentrating on the processes of science and engineering. However, there is a political dimension to science policy formation. When nations try to maximize the contribution of S&T to economic innovation to gain greater shares of global markets, do the political coalitions and parties that drive science funding change, are national science and technology bureaucracies altered, and are formal priorities of S&T policy shifted? If they are, in what ways, and what are the implications for the scientific community, particularly the academic scientific community, which has historically been somewhat removed from the market?

Politics are an essential element of policy. Whether partisan or bi-partisan, Congressional coalitions that vote predictably for increasing appropriations for mission agency and NSF budgets are the cornerstone of academic of R&D. Historically, science and technology policy scholars have downplayed politics, perhaps to distance themselves from their dependence on parties and platforms, focusing instead on Presidential leadership and the administrative branch of government. However, understanding the stability or instability of political coalitions at the federal level is essential to grasping the future funding of academic science and technology. To paraphrase, it is time to "bring…" politics "back in" (Evans, Rueschemeyer and Skocpol 1985).

To approach the problem of S&T policy during a period of when knowledge regimes are shifting from a public good knowledge/learning regime (1945-1980) to an academic capitalist knowledge/learning regime (1980-present—see Slaughter and Rhoades 2004), we focused on the United States in the mid to late 1990s. To understand the political dimension of S&T policy, we analyzed *Unlocking our future: Toward a new national science policy*, also known as the National Science Study (U.S. House of Representatives 1997), a S&T policy document produced by the House Science Committee when the "revolutionary" Republicans gained control of the House the mid-1990s, to see if the priorities of science policy were changing, if state agencies were being reorganized to achieve new priorities, and if universities were expected to play a

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[28] This essay is drawn from Sheila Slaughter and Gary Rhoades Sheila Slaughter and Gary Rhoades. 2005. "From endless frontier to basic science for use: Social contracts between science and society." *Science, Technology and Human Values.* 30,4: 1-37.

new part, working more closely with industry in reconfigured agencies (Slaughter and Rhoades 2005). To understand the economic dimension of S&T policy, we analyzed the social-spatial locations of the board members of the eight S&T policy organizations whose reports informed the Republican science policy study to see what sectors of the economy they represented.

Generally, we found that Republican policy supported both basic science *and* competitiveness or civilian technology policy but did not advocate reorganization of state administration of S&T.[29] However, a number of the S&T policy groups, particularly those with board members drawn from the financial sector, pushed for the establishment of a separate mission agency for civilian technology. This suggests that conceptualization of a unitary social contract between science and society or an iterated principal-agent relation expressing the interaction of science and society is insufficient because there may be multiple social contracts and many principals and agents (Guston 2000)

In our 1996 article, we argued that in the 1980s a bipartisan Congressional competitiveness S&T coalition emerged that operated simultaneously with the Cold War coalition. We saw the Cold War coalition (DOD/DOE/NASA/aerospace) contracting while competitiveness research, specifically, civilian technology and health (NIH) research expanded. Through examination of Congressional voting behavior on competitiveness policy legislation, we demonstrated that the bipartisan competitiveness coalition delivered resources to science and technology agencies and projects as reliably as did the Cold War coalition.

In this article, our study of the House Science Committee's National Science Study's policy network has led us to refine our conception knowledge regimes, acknowledging a complex array of political actors, including the. In the course of our analysis, we discovered that the grand narrative about a social contract between science and society obscured what were very likely multiple social contracts between groups of scientists, political parties, specific political economic organizations, and various institutions. The ability to conceptualize how these contracts are constructed, instantiated, and maintained or changed over time might serve academic S&T well.

In our 1996 article, we argued that non-defense corporations, particularly in high technology and health, joined together to promote an S&T competitiveness coalition when globalization pushed civilian rather than defense technology to the fore. Our present findings lend some support to that view, but also led us to revise our thesis. As expected, very few (one, TRW) defense contractors were represented on the boards of S&T policy organizations that informed the CRS report to the House NSS. Contrary to our expectations, no health corporations were represented. Moreover, the firms that were represented were not exactly the ones we anticipated: on the one hand, the telecommunications and computer industries were well represented; on the other hand,

[29] The National Science Study did not examine health research, which falls under different Congressional jurisdiction and is embedded in another set of mission agencies, which very likely have 'social contracts' with society that are distinct from those we consider here.

the largest representation was from banking and financial services corporations that do not invest much in research or produce high technology, but depend on advanced technology for infrastructure and capital formation. Given the corporations represented on the S&T policy organizations, we concluded that the competitiveness policy reports linked together S&T actors who were heavy users of civilian technology but lacked support of a dedicated government mission agency or specific programs.

We think that defense and health corporations were not mobilized to participate in competitiveness policy reports because they are already connected to federal funding agencies through different political/policy circuits. We recognize that following the end of the Cold War, defense firms contracted, some consolidated, many conglomerates divested themselves of their defense divisions (see also Markusen and Yudkin 1992), and the remaining defense firms have specialized S&T policy organizations that are linked to the DOD (Grieder 1998). They developed their own policies ("dual use," war on multiple fronts) using their own policy organizations (Klare 1995). Health S&T policy organizations grew rapidly in the 1980s and 1990s along with for profit hospitals, managed care, health products and technology, especially biotechnology (Navarro 1993, 1994); these health S&T policy organizations and businesses are linked to the NIH, whose budget has increased more than any other mission agency in the 1980s and 1990s. The corporations on the S&T policy organizations in our sample seem to represent non-health civilian technology corporations or corporations that are heavy users of civilian technology that are in search of regular funding, either through a dedicated agency or program, or series of programs that cut across agencies.

Although the competitiveness S&T coalition is one of several, the general direction of the ideology of S&T policy organizations and the corporate leaders who sit on their boards has been quite consistent. The leaders have participated in maintaining a bipartisan competitiveness coalition in Congress despite the shift from a Democrat to a Republican controlled Congress, and during a Presidential term in which Republicans attempted to impeach Clinton, while systematically challenging his policies in one area after another. In this extraordinarily partisan time period the expansion of the bipartisan competitiveness coalition, and the resource shift towards civilian R & D that has accompanied it, are truly remarkable. As was the case in the Cold War, S&T seems best served by not identifying with a particular party, but by linking its funding to societal priorities supported by powerful corporations that keep the economy dynamic. These corporations generally contribute to both parties, to ensure that they have access to decision makers, although they tend to give more heavily to (centrist) Republicans. Thus, successful S&T coalitions are bipartisan *and* political in that they have redefined academic science to better fit with the neo-liberal state.

The events following 9/11, particularly the invasion of Iraq, underscore the bipartisan and political elements of S&T policy. The equipoise between defense and civilian technology research, achieved in 2000, shifted after 9/11. The Bush Administration gave high priority to increasing DOD spending in general and particularly to increasing DOD development spending (AAAS 2003a,b). The invasion of Iraq had broad bipartisan support, creating conditions for the Cold War Coalition to re-emerge as the War on Terrorism, greatly increasing the budgets of DOD and defense related agencies such as Homeland Security.

However, the Bush Administration's War on Terrorism is not a return to the *status quo ante*, despite the rapid rise in defense funding. In the final Defense bill for 2004, the total DOD R&D will reach $66.0 billion, an increase of 12.4 percent or $7.3 billion from FY 2003. Funding for Homeland Security also rises dramatically. Other mission agencies' funding, including NSF, remains steady or declines slightly. However, current funding priorities suggest that the Vannevar Bush model, which placed a high priority on basic research and organized science so that university scientists and engineers had a modicum of autonomy, is not being resurrected. Although the DOD University Research Initiatives are slated to rise 22 percent, much of the increase will go to "congressionally designated, performer-specific projects (earmarks)" (AAAS 2003a, 2 of 5, parenthesis in original). DOD funding for basic research will decline by 6 percent. The rise of earmarking suggests that peer review, which accedes to scientists judgments, is waning (see Savage 1999, as well as National Science Board 2002, for the continued growth of earmarking). The decline of "basic" research also suggests revisions to the Vannevar Bush model. The War on Terrorism may offer the academic R&D community a defense based social contract that is somewhat differs from the historic Cold War contract.

Given that defense firms contracted in the 1980s and 1990s, scholars interested in the way that S&T policy coalitions expand and exert influence on the Congress and the executive branch might do well to see if the number of defense firms, relatively small at the initiation of the War on Terrorism, expands, and whether firms not now connected to defense diversify to develop defense related products and services. If firms do diversify, scholars will want to pay attention to whether the firms are new economy firms with core products in information technology and biotechnology, which may signal possible linkages between the defense coalition and the competitiveness coalition. Alternatively, if the competitiveness coalition has strong support from firms that see the future in civilian technology for global markets, they may continue to support S&T policy organizations that push for academic R&D that is linked to commercial products and processes.

Many questions remain. We have concentrated on an important, but individual policy network, as it works through Congress. What is the overlap and relationship between this network and others that surround other social contracts? What part does the executive branch, the judiciary and the mission agencies play in writing social contracts with the academy? How influential are university actors, who often engage in lobbying, including earmarking? And what about faculty, both those who serve on S&T policy organizations and faculty organized in associations such as AAAS? In the competition for resources, and the renegotiation of the mechanisms through which federal R & D funding will be channeled, what coalitions will gain ascendance? To what extent will the specific direction of these appropriations and mechanisms be shaped by the next presidential administration and congress?

We have concentrated on corporate influence on R & D policy exercised through one structure—membership on S & T policy organizations. To what extent are the changing and competing coalitions evident in other structures through which capital shapes public policy, the most obvious being Political Action Committees and donations to congressional candidates' campaigns? We have concentrated largely on the ways in which capital shapes policy, overlooking other groups and social movements. To what extent are particular funding priorities among and within the various coalitions shaped by single issue campaigns and social movements, which can have a powerful impact on Congress? Further, the question of priority setting has yet to be resolved. What criteria will be used to allocate R&D funds, and to what extent will those who set and implement them come from within or outside of the academy? In asking these questions, we remain focused on the social contracts between science and society that structure the direction and practice of research. In closing, we underscore the complexity of the multiple contracts that have been negotiated, which is underlined by the shifts in funding stemming from the War on Terrorism.

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Institutional Ecology and Societal Outcomes

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Research goes on in a variety of familiar institutional settings, and we may commonly refer to them as "institutions that do research," but we could with equal precision call government agencies "institutions that create bureaucracy" or the military "institutions where people follow orders" or museums "institutions that induce people to look at things on walls." Since research is almost always carried out for instrumental reasons, another way to think about research and institutions might be to ask: what are institutions trying to accomplish by doing research, and how well do they succeed?

This question has a variety of familiar answers. Universities perform research to, e.g., create knowledge; generate indirect cost returns; generate revenue from IP; build institutional prestige; help solve a diverse set of problems extrinsic to research; train the next generation of researchers. Government laboratories perform research to, e.g., help solve problems that the government is expected to help solve. Corporate laboratories perform research to, e.g., help enhance competitiveness and profitability while producing things that consumers want or need, or can be convinced to want or need.

Such answers provide one approach to engaging the issues raised by the workshop agenda, which asks this panel to consider whether major institutions that do research are in transition, and explore the challenges faced by such institutions. Again, we can list a reasonably familiar set of transitions and challenges: increasing competition for funding, increasing privatization of knowledge, increasing globalization of the scientific workforce, allegedly decreasing pool of future scientists and engineers, increasing administrative burdens (for academic researchers), increasing political scrutiny (for some government researchers), increasing public skepticism (of some avenues of private research), decreasing time-horizons to demonstrate results, decreasing role of individual investigators, increasing pressure to hype results, increasing pressure and incentives to work across disciplinary, institutional, and sectoral boundaries, increasing competition from abroad, etc. It's not clear to me how many of these challenges are actually new, or that policy changes over the past several decades have done more than accelerate or decelerate rates of transition. But a much more important point is that most of these familiar challenges are concerned with "institutions that do research" qua "institutions that do research," rather than as components of larger institutional settings that pursue research as one strategy to achieve broader goals.

Since research is almost always viewed by its sponsors and advocates as instrumental to pursuing something else, it seems fundamentally problematic to consider "institutions that do research" in isolation from the "something else" (although that's mostly what we [i.e., those of us engaged in "the social science of science" or "research on research"] do when we consider institutional transitions, challenges, and policies). I'm not being hyperbolic when I say "fundamentally problematic," because unless our understanding of "institutions that do research" is constructed as part of a larger understanding of the ecology of knowledge use and value in society, any assertions about the links between what we know about "institutions that do research" and real-world outcomes is based on assumption, coarse-grained correlation, and metaphysics. In other words, we may have a well-developed theoretical and empirical framework for, say, the "health" of the research activities that go on within institutions that do research (productivity, citation rates, collaboration networks, patents, stakeholder input, etc.), but we have little reason to be confident that metrics of healthy research institutions are also metrics of potential social benefit.

This is getting too abstract so let me give a concrete example. I was recently contacted by a member of the board of regents of a large Midwestern, Research I state university that is known in particular for its agricultural research (and football teams). He was interested in how to think about connections between research carried out at his university (i.e., an "institution that does research"), and the outcomes of that research in society. "We need to find ways to be strategic about planning and stimulating research to address social need," he wrote to me. "Most of our agricultural research has ultimately led to rural decline" in the state. He went on to note that his "interest in this issue is met with significant apprehension" by the university's president and other regents.

The regent's concern highlights two key points. First, his university's success as an "institution that does research" seems to be connected to the emergence of an undesirable outcome—rural decline—that is the opposite of what was intended. Another, less tendentious way to phrase this is that the metrics of success for "institutions that do research" are decoupled from at least some metrics of social value; i.e., institutional success can be accompanied by social failure. The second point is that those who are accountable for the university's performance may have an interest in protecting this decoupling—and the thinking that underlies it. This is understandable—it's hard enough to maintain the excellence and productivity of a research university without having to worry about being judged based on outcomes over which the university may have little control.

At the core of this dilemma is a radical incoherence that is completely internalized in much science policy discourse. Using the above example, the structure of the incoherence works something like this: 1) agricultural research is necessary to boost farm productivity and profits; 2) the state's university is a prestigious, Research I institution with a reputation for scientific excellence, excellent facilities, scientists who publish and are cited a lot, etc. etc. 3) therefore, additional support for agricultural research at the university will benefit the agricultural sector so central to the state's economy. The "therefore" connecting the first and second statements is the heart of the incoherence. It has the effect of allowing institutions to make claims about broad social benefits yet to insulate accountability within the confines of internal institution attributes, and shutting out the complexity of the real world where science and society actually co-evolve. The health of "institutions that do research" continues to be viewed by most people as a proxy for the institution's ability to generate beneficial social outcomes.

One may consider this sort of example typical or not, but I don't think it can plausibly be argued that those who do research on research (or anyone else) have a good set of theories, methods, models, cases, and data that can situate "institutions that do research" within a broader institutional ecology that would allow us to tease out the dynamic interactions between research activities and social value. The one partial exception to this diagnosis are the NeoSchumpeterian or evolutionary economists, who have done a pretty good job of specifying the roles that "institutions that do research" play in national innovation systems as part of the wealth creation process. This panel's moderator is much better qualified to address this work than I am.

Wealth creation is not the only thing we ask from science, of course. Let me briefly raise two examples to illustrate the breadth of my concerns. The first is global climate change. Over the past 15 years or so, the U.S. has spent upwards of $25 billion on research to understand the behavior of the global climate system as a basis (according to those who advocate for such research) for creating appropriate public policies. One result of this investment has been the creation of a world-class scientific capability housed within a variety of "institutions that do research," mostly universities. While these institutions face challenges and undergo transitions, they are nevertheless highly productive and have considerably expanded our understanding of climate behavior.

On the other hand, the U.S.—and the world, too, but that's another argument—have yet to take any meaningful action on climate policy. This of course is not the fault of our "institutions that do climate research," but if we measure success on the basis of those promises upon which the research is justified, it's reasonable to question how much public value we have actually garnered from our considerable investment in climate science. A sophisticated institutional critique of climate science would need to assess where "institutions that do climate research" fit into an institutional ecology that includes, among many other entities, utility, natural resource, and manufacturing corporations, government regulatory bodies, international trade organizations, and political decision-making bodies at the local, regional, national, and international level. A sophisticated science policy would integrate that ecological knowledge into its decision processes. The point is that if we fund science to accomplish things outside the laboratory, then it makes no sense to talk about "institutions that do research" in isolation from the context within which those things get done.

Another interesting example is the California Institute for Regenerative Medicine (CIRM), funded, through a voter initiative, to support stem cell research. CIRM was promoted to the public on the basis of the potential for stem cell research to contribute to curing a wide variety of diseases and disabilities, such as Alzheimer's, spinal cord injury, and Type I Diabetes. CIRM was also designed to be a highly insular "institution that does research," explicitly exempted through the language of the referendum from any meaningful public accountability. In other words, those who conceived CIRM sought to simplify the institutional ecology by protecting researchers from political institutions that could act to restrict the types of research that CIRM would be doing. Note that the mental model here (or at least the promotional model) equates the performance of the "institution that does research" with the achievement of particular social needs. Left out of the equation is the complex and troubling institutional ecology within which health care gets delivered. Any plausible assessment of CIRM's capacity to achieve its promises would have to include an analysis of institutions (and their interactions) ranging from HMO's and hospitals to pharmaceutical companies to the U.S. Patent Office to the Medicare and Medicaid programs to the private insurance industry and its public regulatory bodies. As an empirical matter, we know that there are many countries that spend less on biomedical research and health care delivery than the U.S. yet whose populations are more healthy than Americans, so it is hard to see how a consideration of "institutions that do biomedical research" can tell us much about the public value of those institutions without also considering the broader institutional context in which they operate.

While science studies has done a good job of revealing the contextual embeddedness of all knowledge creation, and of illuminating aspects of the complex co-evolution of science and society, and science policy research has done a good job of understanding the political dynamics of "institutions that do science" themselves, and of assessing and measuring what goes on in those institutions, the bigger questions around what science actually does, and how it does what it does, has been amazingly neglected. One of the big challenges for science policy research, if it has any ambitions toward contributing to the public value of science policy itself, is to begin to seriously tackle the institutional ecology of knowledge creation and use.

In this light let me suggest a few, interrelated research areas that could begin to help meet this challenge.

1. We need a conceptual framework, perhaps analogous to "national innovation systems," that can help put some boundaries around, and illuminate structure and dynamics within, the complex institutional setting for knowledge creation and use aimed at goals other than wealth creation.

2. Given that public investments in research are usually justified in terms of particular, desirable outcomes, we need to develop generalized approaches for systems-based institutional analysis and mapping that would allow such justifications to be contextualized and tested simply for plausibility. (For example, we might want to test the idea that more fundamental knowledge about the climate system is important for catalyzing a global technological shift to a decarbonized energy system.)

3. As Toulmin realized 40 years ago, science policy discourse often focuses on trade-offs between various scientific fields, rather than between science and other approaches to a particular social need or goal. Because we don't understand research institutions ecologically, we still lack a decent analytical basis for understanding the role of research within a broad portfolio of potential policy interventions aimed at some goal. Mapping institutional ecologies of knowledge creation, use, and value could provide a foundation for developing new decision tools that allow policy makers to confront fundamental questions, such as: When is "more research" the right prescription? When is it unlikely to make a difference? What other factors are necessary for it to make a difference?

When would a different intervention offer a more efficient or plausible route to a desired outcome? (For example, California voters might have benefited from a discussion of the variety of ways that the $5 billion allocated for CIRM might be applied to improving public health in the state.)

4. We do know that "institutions that do research" are embedded in different ways in broader institutional ecologies. In particular, certain types of research settings—e.g., agriculture; private sector software development; clinical medicine—have been identified as sites where feedback between knowledge creation and use is supposed to be strong. We need many, many more institutional case studies to help map out the variety of designs that are available, and to develop comparative frameworks and metrics based on the relations among institutional design, and knowledge creation, use, and value.

5. And of course we also need to reflect on where our own efforts fit in. "Bring back OTA" is not an adequate prescription. What are the loci of decision making where better understanding of, and discussion about, the institutional ecology of knowledge creation, use and value might make a difference? What types of insights, tools and products might decision makers find useful? This workshop strikes me as a huge opportunity to begin to enhance the public value of science policy and science studies research, but we need to start, needless to say, by attending to the institutional ecologies within which we now operate.

Session 4: Cultures, Structures and Networks of Knowledge Production

Sociology, Science Policy, and Context

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Two theoretical questions that confront sociology also raise important practical issues for science policy. The abstract issues center on (1) the means by which action and constraint articulate in practice and (2) the mechanisms that allow social arrangements to simultaneously reproduce themselves and innovate.

Why do some actors seem able to sculpt their worlds into favorable configurations while others labor under constraints they cannot control? Views that emphasize agency of various sorts are evidenced in studies of 'genius' and in Latour's now famous phrase 'give me a laboratory and I will raise the world.' Approaches that highlight constraint, in the form, for instance, of stratification orders that systematically limit the life chances of some, advocate a worldview that more closely approximates Marx's dictum: 'men make history but do so under conditions not of their own choosing.' Understanding the interplay of agency and constraint in the context of scientific and technological innovation can offer science policy new insights into how to systematically improve access and facilitate discovery.[30]

How can conservative social arrangements generate novelty? Social systems are conservative when they reproduce themselves over time with little change. Conservative arrangements that also produce considerable novelty are theoretically interesting. The institutional characteristics of American academic science offer a case in point. There is almost no social institution more conservative than the combination of a single blind peer review process with an up or out tenure vote. Yet American science and engineering has been phenomenally innovative in the years since World War II. That success may depend on a mix of responsiveness to and organized skepticism about radical discoveries.

The benefits of bringing sociology and science policy into closer contact, I suspect, will be dramatically increased by finding questions and settings that address practical issues as well as more abstract concerns. Both of these needs can be served by close attention to the types and effects of contexts that constrain and enable scientific work. Consider three.

Organizations translate features of macro-social orders into greater or lesser degrees of constraint on the daily actions of individuals. People at work in organizations differ in their abilities to navigate their environments. Social structures (such as networks and stratification orders) emerge from patterned behavior to shape future possibilities for

[30] While I dispense with citations and bibliography in this informal essay, most of the ideas I present here draw upon the work of others. Should anyone care to ask, I will enthusiastically detail my intellectual debts.

action. Emergent structures direct flows of information and resources to differently positioned actors at different rates.

Institutions are the rules, regulations, and conventions that guide action in a recognizable area of social life. Those rules are simultaneously constraining and generative. They often describe what is forbidden but not all that might be permitted. When rules and regulations mandate certain actions they are often ambiguous about implementation. As a result, even rules intended to set limits on behavior may (at least for a brief time) create broad possibilities for action.

How do networks, organizations, and institutions interact to create contexts that might be of interest to science policy? I sketch an example drawn from my work with Woody Powell and Kjersten Bunker Whittington before turning to a brief discussion of the ways this view of context might contribute to policy debates. I hope to raise more questions than I answer.

Innovative regions in biotechnology

Human therapeutic and diagnostic biotechnology translates often basic life science discoveries into commercially and clinically valuable products. In the U.S. and around the world, successful biotechnology companies are clustered in geographic regions such as Cambridge and Boston, MA or the San Francisco Bay Area. Those regions are home to large numbers of firms in the industry, diverse public science organizations (universities, hospitals, research institutes) and sources of private venture financing.

In addition to scale (many organizations in the same industry) and diversity (many types of organizations working to different ends under divergent institutional and resource constraints), successful regions are characterized by dense networks that link organizations and scientists to each other. Networks are the wellsprings of innovation in biotechnology. Much of the work that goes into discovering and developing new drugs occurs outside the boundaries of particular firms in collaborations with titular competitors and public organizations.

Location in a regional cluster characterized by organizational scale, institutional diversity, and dense network structures benefits firms in numerous ways. Biotech firms in such regions patent more than their competitors outside of regions. Fully 60% of patents owned by U.S. biotechnology firms are assigned to companies located in just three regions. Firms located in the country's most densely populated and networked regions also account for the lion's share of the industry's best-selling products. Six biotech companies produced the industry's ten best selling drugs in 2001. Five of those firms are located in Boston, the San Francisco Bay Area, or San Diego. Dense, diverse, connected regions provide a beneficial context for biotechnology.

Understanding how high technology regions germinate, grow, and sustain themselves could contribute to economic development efforts around the country. My own state, Michigan, is home to an ambitious attempt to create just such a region with the aid of large influxes of tobacco settlement money. Michigan's efforts are far from unique. I suspect that many governors covet the political and economic benefits that would accompany a 'new' Silicon Valley in their state. Yet the components that are necessary for regional economic development are far from clear and, to date, no one has determined if there are sufficient conditions for the birth of high-tech clusters.

I can't answer the sufficiency question, but I do suggest that the benefits of regional agglomeration can be explained by the peculiar contexts overlapping networks, organizations, and institutions create. This view offers interesting insights to sociological theory and science policy alike.

Consider organizations. It may only be a slight exaggeration to say that all cutting edge life science is anchored in formal organizations such as firms, universities, government agencies, research institutes and hospitals. Such organizations share a few common features. They coordinate complex work, channel necessary inputs (human capital, information, raw materials of various sorts), and produce outputs (patents, publications, new drugs, skilled scientists, cured patients etc.). A dense cluster of organizations undertaking the same types of work will create (or attract) concentrations of necessary inputs. Labor markets for life scientists in Boston, the Bay Area or San Diego, for instance, are deeper and more fluid than anywhere else in the nation. Venture capital, too, concentrates in these regions. Such concentrations, which economic geographers sometimes call scale externalities, limit the amount organizations must search for, in this example, talent or capital, thus allowing them to economize on the costs of some inputs without necessarily sacrificing quality.

But the organizations undertaking scientific work in biotechnology regions also differ in consequential ways. Science-based firms and public research organizations do similar kinds of work under disparate institutional conditions. Take research universities. Such campuses are distinguished by influxes of federal (and state) funding, by a mandate to discover and disseminate fundamental knowledge, train students and serve the public good. While universities vary in their reliance on public funds and their commitments to research, teaching, and service, these institutional characteristics add important dimensions to high technology regions. Universities broadcast knowledge into a region (and beyond), but they also add stability.

Large, research intensive universities rarely go out of business. Public funding insures some degree of insulation from the vagaries of the market. Small, science-focused biotechnology firms are very different creatures. They are subject to dramatic market pressures and often die. While many receive public support (in the form of, for instance, SBIR grants or subsidized clinical trials for orphan drugs), most depend on private equity or public capital markets. While they contribute little stability to a region, their relative independence from the time tables of granting agencies and the conservative features of theory-driven academic science makes them highly responsive to discoveries on a fast-moving research frontier.

In some cases the institutional arrangements that characterize public and private research efforts can be highly complementary, matching stability with responsiveness and deliberate, longer term research endeavors with more flexible, discovery oriented science. When that happens, institutionally diverse regions will be more stable and productive than those that are densely populated by as single type of organization.

Organizational scale and institutional diversity seem to me to be likely candidates for the list of conditions that are necessary for the development and maintenance of successful high technology regions. Research in this area though, has demonstrated time and time again that neither scale nor diversity are sufficient. The failure of areas like Chicago, Philadelphia and the Tri-State region to develop biotechnology communities akin to those I have been describing offers a case in point. Simply adding institutions and organizations and stirring is unlikely to be enough.

Which brings me to the third dimension of context, networks. Let me be clear. I do not contend that networks are a sufficient condition for the development and maintenance of high tech regions, but I do believe they are necessary. 'Formal,' contractual networks that connect organizations and more 'informal' ties based in the mobility of scientists and engineers from organization to organization within a region forge dense, institutionally diverse clusters into self-aware communities.

Networks make regions into communities in several ways. First, formal and informal ties facilitate the flow of resources from position to position within a regionally bounded social structure. When multiplex networks connect firms with public science organizations they facilitate thick, rich flows of information, allowing cutting edge basic science and the tacit knowledge necessary to its implementation to diffuse into the region. When ties connect titular competitors they introduce a degree of forbearance into hotly contested winner-take-most innovation races. Co-located firms that collaborate on one therapeutic candidate while competing on another indication and scientists whose social connections cross firm boundaries compete. Ferociously. Victors, though, will be loath to destroy the vanquished if some portion of their research effort is mutually dependent. When information and resource flows as well as mutual awareness and restraint emerge from cohesive networks, self-aware communities result.

Where scale helps to create accessible local pools of important resources and diversity contributes to the stability and innovative potential of a region, cohesive networks are the skeleton that defines a region's shape and character. The benefits of location in a regional cluster defined by scale, diversity and cohesion are benefits of a particular social context. Understanding that context is interesting to me as a sociologist because it speaks to the two theoretical questions that opened this essay. Successful regions are conservative in the sense that they stably reproduce themselves over time (even as firms in the region die). They are also hugely innovative in organizational and technological terms.

To a certain extent, regional communities raise all boats. Location in a region confers benefits relative to those outside it. Outcomes vary dramatically within regions, however. Some organizations and individuals seem much better positioned to benefit from their membership in a regional community. For others, the networks that create community are ties that bind. As a result, examining the creation and dynamics of regional clusters and particularly the interplay of organizations, institutions, and networks seems to me to have real theoretical potential.

Such an examination, though, could also have immediate practical payoffs by providing systematic advice to policy-makers intent on creating new regions. Broader, more interesting, questions relevant to policy could also be addressed. If we learn how economic competitors develop and maintain practices that also allow them to be scientific collaborators, we will have taken an important step toward understanding the organizational capacities necessary to the sorts of mega-projects that may become commonplace in the age of genomics. If we determine how universities and firms can collaborate without impeding the ability of firms to compete in the market or forcing universities to, we will have taken a step toward unraveling the changes that increased research commercialization is wreaking on academe. Answers to both of those questions, could provide new insights into the ways that federal (and other public) investments in fundamental science can percolate out into the market without placing onerous requirements on the investigators and organizations that perform basic research.

The list could go on, but my point (I hope) is simple. Context matters, but it's complicated. Unpacking the theoretical and practical nuances of context requires rich, strategic research sites. I believe that the concerns of science policy point to numerous possible locations for that research. I also hold that the theoretical concerns of sociologists can lead them to raise and answer questions about those settings in a fashion that has much to offer policy-makers.

Ground Level Knowledge Gaps in U.S. Science and Mathematics Education

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United States science and science policy communities agree that scientific discovery and innovation are critical contributors to economic growth and societal development. Implicit in that agreement is that having a technically trained workforce is important. Discovery takes place at the boundaries of fields of knowledge, and innovation takes place at the intersection of those boundaries. From this point of view, the importance of a rigorous system of technical education is obvious. Several questions arise from this assertion. *What is a rigorous system of technical education? Who are the recipients of such an education? Is such an education as valuable to the recipient as it is to the society?*

There are well established bodies of work devoted to the first two of these questions. One of the outcomes is the discussion of whether the United States produces enough scientists and engineers. Labor economists suggest that there is no shortage in the U.S. science and technology workforce. The presence of foreign scientists and engineers meets our market needs and it is immaterial where they come from. Labor economists further suggest that concerns about future decreases in the influx of foreign talent is purely speculative and not backed by historical precedence. Their argument is strengthened by the climate of globalization and the relatively unencumbered movement of technical workers to specialized markets wherever they exist in the world.

One of the unintended consequences of this argument is that it undermines research efforts from the science policy community into the value system that undergirds U.S. technical education. Assuming that the output of the U.S. technical education system is adequately supplemented by foreign talent diminishes the importance of examining who among U.S. students has access to that system. This examination is central to answering any question about the worth of a science career or the value of a technical education. It shifts the object of inquiry from the education system itself to the students. What value does the science community place on domestic students who do not have access to technical education or are disenchanted by it? Contentment with the conclusion that the domestic technical workforce is adequately supplied says that we place little value on this category of excluded U.S. students.

This uncomfortable value assessment is where the demographics of the U.S. science enterprise enter the discussion. For nearly forty years there have been concerted efforts to include Black and Hispanic students in the technical education system and in the science careers that follow. The incremental successes of that effort do not overwhelm the reality that these groups remain disproportionately excluded from U.S. science and U.S. science policy. Much of the work dedicated to this problem has focused on gathering data that provides evidence of exclusion. One of the results of that data collection effort has been focus on the academic performance of Black and Hispanic students at each of the major transition points: high school to university and university to graduate studies. The mathematical and scientific proficiency of these groups of students on average is a deterrent to participation in academic technical disciplines and pursuit of science careers.

The statistical demonstration of these shortcomings is clear. This evidence of course falls short of providing reasons why. An advantage of examining the social organization of science and science policy is precisely the introduction of *social value* into the research and action agenda of the science community. One of the unasked questions is, *How does this group of students perceive its own value?* The science enterprise is not operating in a vacuum. The momentum of the anti-affirmative action effort continues to grow unabated. The isolation of Black and Hispanic students in science, technology, engineering and mathematics programs still carry with it a significant personal burden that to date is not well understood. Implicit in both of these realities are messages about value. How are these students valued by the institutions they attend and by the United States in general? In response to externally determined value, how do individuals from these groups assess their own value as members of the science enterprise? The answers to these questions represent a gap in knowledge within the science policy community.

Another consequence of the efforts to address equal representation in the U.S. science enterprise has been an unrelenting association between Black and Hispanic students, academic difficulty and scientific and mathematical weakness. As a result, the worth of a science career for Black and Hispanic Americans may go well beyond wages, job security and potential technological impact. It may cross into community development and successful representation of self and kind. For the science policy community to recognize these potential determinants of worth is a prerequisite to including them in the set of metrics developed to determine worth.

Scientific discovery and innovation are indeed critical components of a competitive economy and a productive society. Understanding the incentives and barriers to participating in that process are equally important. As a science community in the United States, we have the luxury of a unique combination of foreign and domestic talent. The sheer numerical reality is such that the science engine will not stall if Black and Hispanic Americans simply do not participate. That unfortunate truth does not release the science and science policy communities from the responsibility of understanding this fundamental component of the United States innovation system. The academic rigor that has been applied to understanding the U.S. innovation system suggests that not only is studying this component well within the core competence of the community; it is also necessary for this branch of the social organization of science to be complete. In this current era of U.S. government proclamations of American freedom and values it would be inconsistent and unethical for the U.S. science community not to investigate the conditions of this indigenous natural knowledge resource.

Traditional Academic Disciplines: Obstacles or Opportunities to the conduct of research and inquiry

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Traditional academic disciplinary boundaries pose major obstacles to the conduct of research and inquiry in three major ways by limiting perspectives (ways of viewing and framing problems), funding for research, and publication outlets for research.

The process by which knowledge domains become disciplines focuses on establishing lines of demarcation or boundaries that separate one knowledge domain from another—what Becher (1999) calls carving out "turf". One of the defining characteristics of academic disciplines—specialized language—is the basis for framing or posing questions for research and inquiry; how questions are framed sets limits on how those questions are approached, including what is—and what is not—to be observed. Consequently, strict adherence to traditional disciplines often results in researchers being unable to communicate across disciplinary boundaries, and hence unable to bring diverse perspectives to bear on the conduct of research and inquiry.

Since its inception, the academy has been an exclusionary as well as an exclusive institution. Academics function as gatekeepers in two major ways. First, by devising and implementing standards for admission, certification, licensure, and performance evaluation, academics control the quantity (and quality) of practitioners in a given discipline. Second, by creating, applying, and enforcing standards of legitimacy, academics exercise control over the development—or lack thereof—of knowledge fields or disciplines. Academic disciplines come into being in a variety of ways. Some develop as off-shoots of established disciplines, such as for example molecular biology as an offshoot of biology. Others evolve from having been a subfield of an established discipline, e.g., aerospace engineering. Still others develop as a combination of two or more established disciplines, e.g., social psychology (a combination of sociology and psychology). Whatever their genesis, disciplines are primarily defined by two distinct but related factors: legitimacy; and a department base. If a discipline is not based or housed in a "relevant" academic department, it is not considered to be authentic. The legitimacy of a discipline is based on the extent to which it conforms to a set of notions of academic credibility, intellectual substance, and the "appropriateness" of the subject matter (Becher 1996). This includes clear definitions of appropriate publication outlets.

Legitimacy is conferred on a knowledge field in academe to make it a discipline in much the same way that legitimacy is conferred on a given occupation to make it a profession. Both involve a type of social contract between practitioners of a particular occupation or disciples of a particular discipline and the larger society. The practitioners must convince the larger society—especially the legal apparatus—that it is performing services for the public which require special knowledge, training, and experience. Because a profession is characterized by specialized knowledge and training, nonpractitioners lack the requisite knowledge to evaluate professional performance (Hall 1994). In academe, this same process applies to the way a knowledge field becomes a discipline. A social contract is struck between proponents of the knowledge field and the academy. Proponents make two major claims about their knowledge field: (1) that it is distinct from others insofar as it focuses on a specifically defined set of questions/issues; and (2) that the field has a developed theoretical framework and rigorous, systematic methods to address specified questions. If convinced of the proponents' claims, the academy bestows on them the right to establish and enforce curricula and the criteria for entry and advancement in the field. The final indicator that academic legitimacy has been achieved is when formal departments become established in institutions of higher education (and enter the competition for scarce resources).

This process whereby knowledge fields become established disciplines illustrates two things about knowledge and social organization: that they are equally important and mutually determining. This process is political insofar as relevant governing bodies—as well as the public—must be convinced that the claims of proponents of a given knowledge field warrant granting it a certain amount of autonomy and control in governing itself. This includes not only establishing and reinforcing criteria for entry, performance evaluation, and recognition, but also the authority to develop and apply sanctions for poor performance and/or non-compliance. Transforming a knowledge field into a discipline is no simple matter, as Becher says, "In the intricate, Byzantine world of academia, nothing is a simple as it seems" (Becher 1996:33).

Academics are fiercely territorial. Not only do they identify and lay claim to a knowledge field as their own, they also contend that new knowledge in that discipline can be generated only by insiders—i.e., authorized proponents legitimized by them. Moreover, they will "defend to the death", their intellectual "turf" from outsiders and would-be interlopers (Kuhn 1962). This academic territoriality can be an obstacle to the conduct of inquiry when strict adherence to the language and perspectives of one's discipline precludes establishing some common ground with researchers from different disciplines from which creative research might develop. This in turn misses the opportunity to bring different perspectives to bear on the conduct of both inquiry and research.

Interdisciplinarity can be conceptualized as a rigorous mode of inquiry that draws from the knowledge bases of a variety of disciplines to enable researchers to see problems "through different eyes" and to develop new forms of inquiry, concepts, explanatory systems, and perspectives. These newly constituted critical perspectives facilitate not only asking new questions, but also asking old questions in new ways. Scientific and technological knowledge is proliferating at an unprecedented rate. Often, applications of scientific and technological knowledge result in ramifications in which social, economic, and political factors are often inextricably intertwined; therefore, they must be addressed from many disciplinary perspectives.

Currently, I am on the research team for a multi-national research project in progress that seeks to identify the conditions that facilitate creative research in human genomics and nanoscience. One preliminary finding is that one of most important facilitators of creative research is the lack of institutional boundaries between disciplines. For example, having free access to common physical space increases opportunities for interaction among researchers from a variety of traditional academic disciplines, and often serves as a powerful catalyst to research collaborations. Some researchers interviewed for this project noted that currently in the United States, research funding tends to be structured around traditional disciplines. Flexible funding mechanisms provide opportunities to conduct interdisciplinary research and inquiry. Some study participants expressed the opinion that the most prestigious academic journals tend to be discipline based and are therefore less likely to publish results from research and inquiry that cross disciplinary lines. Dissemination mechanisms should transcend traditional disciplinary boundaries to maximize the dissemination of the results of research and inquiry.

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Session 5: Social Processes and the Generation of Data

Considerations of Language, Values and Intentions in Science Policy Studies

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As a humanities trained ethicist I very much appreciate the opportunity to participate in this workshop. My own research interests are in the ethical implications of scientific research and the development of emerging technologies; especially their inter-connections to human belief systems and social awareness.

Following from John Marburger's editorial [1] and David Lightfoot's letter [2], it seems to be that our task, very simply put, is to figure out how to use social science to optimize investments in science and technology. In other words, to make formal, predictive assessments of how to spend public money in ways and on endeavors that will bring the highest possible returns. The working assumption here is that scientific research and technology do that the very best of any public funded endeavor. An additional challenge to our group seems to be to raise the stature of social science by proving its ability to define and measure the richest sources of innovation. In other words, using social science to predict when and under what conditions that innovation is most likely to assure the promises of new technological development.

Has a gauntlet been thrown down? Or, is social science being seen as a true and significant source of understanding. Assuming the later, I offer the following ruminations, in response to the two questions posed under category number three, to which I have been assigned:

I. What are some of the different ways in which we produce, share and disseminate knowledge and data?

Before knowledge and data are produced, research questions are culled out of a competitive social process of negotiations over values. Those which win and rise to be supported with large scaled public funds then become the fertilizer for the cultivation of major research initiatives. *Innovation* is an excellent example of one such value. In the context of science policy, innovation is construed as the ability to think of new ideas, ask new questions, and create new things of value in the marketplace. It gains luster as a public good being polished through narrations of beliefs about how competition drives economic gains and domination. The very question of "how to optimize science and investments" leads to the production, sharing and dissemination of data which further foster innovation for purposes of economic gain and domination. If innovation were driving other motivations, then the data would come from other kinds of sources and be understood in other kinds of ways.

Which data? The data we use to answer questions like, "how can we optimize science and technology investments?" We produce data from observation and information pulled from inside of our own ideological dwelling places. If questions are asked from outside of our persistent perceptions and established systems of values, then new understandings can be gleaned about the nature and practice of science. So, for example, rather than primarily looking to economics models of GNP and balances of trade to determine the value of our investments in science, perhaps there is something to be learned by looking at other quantifiable factors, such as the mental health of children; the stability of family life; the internal sense of personal well-being; happiness among our citizens; healthy patriotism rather than that based on fear and manipulation. If those qualities of human life were established to be high, but GNP were low, would it confirm or confound our assumptions that technological innovation is worth our public investments of funds?

Marburger wrote in his editorial, "Relating R & D to innovation in any but a general way is a tall order, but not a hopeless one. We need econometric models that encompass enough variables in a sufficient number of countries to produce reasonable simulations of the effect of specific policy choices." He calls upon economists and social scientists to turn to these issues so that we may increase the effectiveness of science policy. This statement spurs two thoughts for me: First, the use of the word "effectiveness" in the context of science policy needs to be explored and understood very carefully, because it carries the weight of very specific, not necessarily widely shared, values. Again, economic gain is only one quality of success that might be included in the making of science policy. The second thought is that if we care to go beyond the measurement of effectiveness to the conscientious understanding of ourselves as human beings living in an increasingly socio-technical world, then we will also need help from the humanities. Religious studies, philosophy, history and the arts have an important contribution to make toward getting at core systems of belief about qualities of human experience which in fact, shape who we will be.

In his talk to the Consortium of Social Science Associations [3], Marburger made reference to "a broad transformation affecting all of science that is changing the tools, the methods and the sociology of every field." That transformation, he continued, is "being driven by extraordinary enhancements in our ability to gather, sort, analyze, visualize, and communicate vast quantities of information." I would suggest that we are not passive recipients of that transformation, but rather, the architects of it. It is risky business to use the social sciences simply to predict and measure the effects of that transformation, without also understanding just why we are doing what we are doing, and just how much our own human nature has to do with the transformation that is upon us.

Science policy should come from that as well, which leads to the second question posed:

II. What are some of the ethical challenges of these relationships?

Marburger's worry, he says, is that our tools for making wise decisions, and "bringing along the American people and their elected representatives, " are not sharp enough to manage the complexity of our evolving relationship with the awakening globe." My own worry, in terms of ethics is, *who* he is referring to as those who are using tools and bringing along the American people and our representatives? Who is bringing whom along; and whose values do they reflect in their endeavors to manage the complexity of "our" relationship with the globe? Embedded in his use of language may possibly be the implication that science policy precedes the movement of the American people, and our representatives. Might science policy proceed along with, and evolve from, engagement in dialogue with a range of American peoples, and understanding of those American peoples? Might it emerge from the representative body itself? There seems to be a silent mover here, and it behooves us to know its nature and kind.

So how can the social sciences be of assistance to the setting and making of policies about the use of public funds for the enterprise of science? My initial answer is the following: Partner the social sciences with the humanities, to search out a clear sense of how decisions are currently being made and by whom. And further, to learn the nature of the relationships between those decision makers and various constituencies here and abroad. Learn what kind of values and personal or collective/cultural belief systems motivates those relationships. We might also look to the history of science policy for predictive elements of change. And, study the patterns and themes that emerge in the rhetoric of science policy. At some point we need to make determinations of who is being served currently, under the ideal conditions reflected in our U.S. Constitution, who might need to be better served through the making of science policies. International studies, coupled with economic models and political sciences, might bring to bear sets of data which will illuminate our understanding of the rapidly changing features of human living in multiple societies, in and outside of global economies. Understanding science as art may be helpful in making clear the connections between creativity, innovation, observation and what we learn, or think we are learning, in scientific research. Religious belief and science are not as unrelated as is commonly believed. Understanding the religiosity of science will be useful to us in making connections between what we fear, hope for, and aspire to, in the endeavors inside our laboratories.

These should give us a good beginning. But no matter what we do, the very best of social science seeks understanding and knowledge free from a predetermined sense of what will be found, and putting aside all imperatives to see something in particular. We must be wide open to finding new ways of understanding who we are and what we do with science, in terms of what ought to be done, ethically, that is only loosely connected to the good we perceive to be increasing economic power and domination on the globe.

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Session 6: International Context

How have changes at the global level likely to affect science and technology policy?

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Conceptual shift I: Attention to concepts of 'country' and 'nation.'

Since early 1980s attention to economic competition has led policy analysts to pay attention to differences in s/t among countries. For example, since the mid 1980s the NSB S&E indicators regularly include data on international comparisons for most of its main headings: education, workforce, R&D, etc. Prior to the 1980s SRS collected and published data on separate countries but data did not have the visibility and legitimacy of an NSB publication. Non-US data sources, mainly from OECD and individual industrialized countries, also reveal not only a regular gathering and publishing of data since the 1980s but an ongoing interest on country-by-country comparisons.

Social science analyses of s/t policy have begun to respond to the availability of data from other countries. For example, a review of article titles and abstracts in the journal Science and Public Policy shows that, while attention remains focused on single country analyses, there is growing attention to science-policy research outside of the US, both to single-country analyses and comparisons across countries.

However, both policymakers and science-policy researchers have taken for granted the key organizing concepts of 'nation' and 'country.' For example, the NSB takes 'nation' and 'country' as given when it writes in its recent 2020 Vision for the National Science Foundation that "[NSB's] vision for the future is informed by a sense of our Nation, our knowledge of the trajectory of global science and engineering (S&E) research, and our confidence in a promising future. History suggests that a nation that relinquishes the torch of science puts its future prosperity at risk and jeopardizes its place in the history of civilization. The Board believes that we must not let this fate befall our country" (italics added). It does not consider that the possible conceptualizations of either entity might significantly influence s/t policymaking. Similarly when most s/t policy researchers analyze s/t issues in a particular country, they take 'country' as a given. For them, country is the spatial boundaries within which the s/t activities under research take place.

But as the interest in country comparison continues to increase, key conceptual question emerges for social science research of s/t policy: how to conceptualize 'nation' or 'country' in the analyses of s/t policy? Where and how should country or national boundaries be drawn in the analyses of s/t? Should the boundaries for consideration in these analyses be spatial, political, cultural?

Spatial and cultural boundaries

The question of geographic boundaries and their relationship to s/t policy have begun to preoccupy few policy scholars. For example, in a special issue of Science and Public Policy

(December 2004), Josephine Stein, a senior social-science research fellow at University of East London, in trying to answer the question "Is there a European Knowledge System?" first had to establish the s/t boundaries of Europe. She argues that there is a European Knowledge System and its boundaries extend beyond the EC "to incorporate the 25 Member States of the EU and the additional EFTA (European Free Trade Association) states (Iceland, Liechtenstein, Norway, Switzerland), and to extend this as appropriate to the three EU candidate states for which association agreements exist; Israel, which participates fully in the Framework Programme and is a member, associate member or observer in numerous European S&T organisations; countries under the Mediterranean partnership" (Stein 2004), p. 439). Here we can see that dwelling into the question of boundaries, i.e., not taking 'country' or 'region' for granted, quickly yields interesting revelations. In Stein's case, the boundaries of the European knowledge system expanded well beyond the EU.

Conceptually, where and how to draw the boundaries of s/t activities remains a challenge for social science research of science policy. Spatial boundaries have been taken for granted in most s/t policy analyses even though new political, economic, and ideological arrangements have begun to question the stability of existing geographic boundaries. As new arrangements emerge and evolve, new boundaries for the analysis of s/t should be considered. For example, it might make more sense to analyze s/t activities, expenditures, and policies along the Brasil-Cuba-Venezuela axis, where a political commitment for s/t collaboration is emerging, than within the geographic boundaries of Latin America.

But more than calling attention to drawing spatial boundaries around a country, region, or axis, within which s/t statistics can be gathered and analyzed, I am also calling attention to the concept of 'country', as the territorial space within which a 'nation' as a cultural construct has come to exist. Hence 'country' is both spatial and cultural.[31]

Conceptually, I would like to call for particular attention to the cultural element of country, i.e., the nation or 'we' (Anderson 1991) and how the 'we' is defined, by whom, and under what circumstances, and its relationship to institutions, calls for, and decision-making about s/t. Furthermore, key defining moments in the history of a country significantly shape s/t institutions and policy. As William Halffman, a s/t policy scholar in the Netherlands, reminds us, "the basic premises of the legal system, the presence of a well-organized civil society, a centralized state, or a corps of career civil servants are thus seen as key determinants of the national type of decision making. The fundamental patterns that were developed for such institutions are sought in key episodes in history, such as the American or French Revolution, the Napoleonic codification of Dutch law (van Waarden, 1992; Grimm, 1986), or the specific political uncertainties over leadership succession and political conditions at the end of the 19th century"(Halffman 2005), p. 462). Halffman's focus seems to be on key historical developments of the State and their role on decision-making. But how about the history of the meaning of 'nation' and its role in shaping s/t policy?

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[31] It is also political as the territory within which the State exercises sovereignty. But since the political dimension has been the focus of much of political-science analysis, I will focus here on cultural dimensions.

In the book *Defending the Nation: US policymaking to create s/e from Sputnik to the 'War against Terrorism'*, I analyze how images of the nation under threat shape s/e education policy. For example, in the 1960s, the image of the American nation under threat by Soviet science shaped policymaking that positioned NSF as the institutional mechanism to make the elite scientists required for the "Age of Science." Meanwhile, in the 1970s an image of American nation facing domestic social and environmental problems shaped policymaking to educate scientists and engineers to solve domestic social and environmental problems. In the 1980s, an image of the American nation under the threat of Japanese competitiveness shaped the programs for large numbers of scientists and engineers (Lucena 2005). This is only one way of conceptualizing the meaning of 'nation' in shaping policymaking.

Other conceptualizations are possible. For example, in our research on engineering across nations, we have found that as soon as a dominant definition of the 'we' emerges, proposals to move the 'we' forward also emerge. These proposals are what we call 'images of progress' (Downey and Lucena 2004). Very often these 'images of progress' include calls for the creation of institutions and specialized people in s/t, and often for specific directions and resource allocation of s/t. For example, in Brasil the image of progress in that country led, first, the Emperor to create two engineering schools, then state governments to create regional engineering schools during the Republic, the authoritarian government of Getulio Vargas to create a national school and laboratory for national industry, and later the military regime to create graduate and research institutions (Lucena 2006). Clearly, the historical development of s/t in Brasil, conceptualized around its own image of progress, is very different from that in Argentina, Colombia, or Mexico. Hence it makes more sense to research, analyze, and compare differences among s/t developments and policies across countries than to group them around the traditional boundaries of 'Latin America.'

Social-science research opportunities

In light of the conceptual challenges of both spatial and cultural boundaries for social science research of s/t policy, I propose here a key area for future research: *Historical development of countries, as spatial-cultural entities, and the shaping of decision-making for s/t.* That is research on how historical contingencies shaped particular s/t institutions, practices, levels of funding, s/t relationship with state and the wider political economy. This type of research might make it difficult to group countries by continents as it is still done in some NSF publications and s/t policy analysis (e.g., (Velho 2005). Maybe with the exception of Europe which has developed regional decision-making mechanisms and budgets for the development of s/t, continents should not be grouped for comparisons. Other types of country-to-country grouping might make more sense. Even European s/t policy might have to be understood in relationship with national histories of member countries. Furthermore, focus on historical development of countries and cultural images of progress might help explain continuities and discontinuities that social constructivist accounts cannot explain. Social constructivism is helpful in identifying relevant social groups, interpretive flexibility, and closure but is limiting in explaining long-lasting continuities that emerge when, for example, images of progress challenge different generations of decision-makers.

There are, however, some potential limitations to the research proposed here. As Hallfman reminds us, "differences within the same country cannot be explained if the national context is the major explanatory factor. How can we explain more subtle differences between American regulatory regimes for toxic substances and pesticides when they both operate within the same macro-institutions? What if some regimes between countries resemble each other more than contiguous national regimes? *Ad hoc* explanations are then the only way out" (Halffman 2005), p. 463). Hence attention must also be paid to differences among institutions, regulatory regimes, decision-making groups, etc. within the same country.

Conceptual shift II: Attention to engineering

Beginning in the 1980s, national concerns over competitiveness with Japan and more recent concerns over competitiveness at a global scale have made engineering more visible in both popular and policy-making circles (Business-Higher Education Forum 1982; MIT Commission on Industrial Productivity 1989; Schmitt 1990; National Academy of Engineering 2004). Also since the 1980s engineers have come to occupy key positions as directors or deputy directors of NSF (Bloch, Bordogna, Bement) and NSB chairmen (Branscomb, Schmitt). The visibility of engineering in policymaking, given its importance for US competitiveness, has not dimished. For example, NSB's 2006 S/E Indicators highlight the importance of technology in global competitiveness and present an alarming picture of how US competitiveness in high technology is eroding to Asia and Europe.

Ironically, engineering has occupied a secondary position in most STS and science-policy scholarship. This might be due to STS positioning with respect to dominant popular theorizing about science, engineering, and technology. In this dominant theorizing, science discovers the truths (upstream) and engineers applies those truths to control nature through technology (downstream) (Downey 2006), p. 10). As a result, 'engineering' has remained marginal as an object of analysis in both the social studies of science and technology and history of technology. In the last 3 years, a growing number of sessions at both 4S and SHOT meetings on engineers have been sponsored by an emerging group of interdisciplinary scholars now organized under INES but most STS analytical and empirical work on engineering remains peripheral. So shouldn't STS and s/t policy researchers be paying more attention to 'engineering' not as downstream from science but as a site of discovery, mediation, identity formation, negotiation, etc., particularly given its current importance for US competitiveness?

Social-science research opportunities

Within the larger research topic of 'engineering,' engineering education emerges as a site of potential interest to STS and s/e education policy researchers. In engineering education, some of the most significant educational reforms in s/e are being proposed, international collaborations at all levels are being created, and long-lasting policy challenges remain (e.g., engineering has the lowest representation of women and minorities of any s/e field). For example, most of the emphasis on increasing representation of w/m in engineering education has been on 'add-on' diversity programs with the goal of increasing numbers. After millions of dollars and many years of this type of efforts, success remains limited. As Busch-Vishniac and Jaroz, in their influential article "Can Diversity in the Undergraduate Engineering Population Be Enhanced Through Curricular Change?" argue "[few attempts have been made to revise the engineering curriculum itself so as to promote diversity, even though it is clear that the curriculum is central to what defines an engineering education. Instead, most diversity initiatives . . . have tried using "addons" or minor changes to rectify the situation…the curriculum is fundamentally flawed" (Busch-Vishniac and Jarosz 2004), p. 256). What goes on in the creation, dissemination, and learning of engineering curricula that keeps engineering education mainly a white-male environment? Should policymakers be turning their attention to knowledge content in their efforts to increase representation of underrepresented groups in s/e?

In international engineering education, key policy questions remain unanswered. For example, the Bologna process aims at creating a European common area of higher education. This process has been motivated by the desire to enhance mobility of students and professionals across Europe and by the goal of making Europe a highly competitive environment for knowledge creation. Similarly, other frameworks for mobility of engineering students and professionals are being developed in the Americas (e.g., Engineer of the Americas, Iberoamerican engineer). Yet we do not know much about the implications of these processes for US competitiveness and the future of US s/e education. What are the most effective ways for US engineering education to respond to these emerging mobility frameworks? An upcoming conference sponsored by NSF and Sigma Xi to figure out best practices to globally engage the US S&E workforce indicates that answers still remain at large.

Ironically, at the same time that engineering has risen in importance to US competitiveness, the engineering profession appears to be in danger of losing control of technology to other professionals (Downey 2005). First, engineers are now in competition with scientists for the R&D of new technologies in biotech, infotech, and nanotech as evidenced by the increasing number of patents issued to scientists. Second, offshore outsourcing of engineering work places US engineers in direct competition with Chinese, Indian engineers. Third, the 'dematerialization' of engineering work, which takes place as more engineering activity is computer modeled and simulated, places engineering in competition with computer scientists and comp workers. Fourth, the institutionalization of team-work places engineers in competition with non-engineers for the solution of technical problems. Finally, formally educated engineers are in competition with 'non-engineers' for problem-solving in and leading of technical teams. Nowadays, 25% of those reported practicing engineering have no formal education in engineering (Lucena 2003).

Yet we do not know much about this shift in jurisdiction over technological development. What are the dynamics of interdisciplinary work between scientists and engineers in the creation of new technologies? How are these dynamics shaping the development rate, economic value, and social relevance of knowledge and products? Where are the non-engineers in engineering coming from? What are the characteristics of technologies developed, made, and maintained by non-engineers? What are the development rate, economic value, and social relevance of knowledge/products created by non-engineers?

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U.S. Science and Technology Policy in Global Context[32]

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In recent decades, world leadership has been a major touchstone of U.S. science and technology policies. NSF's vision statement, for example, says that NSF investments "will catalyze the strong progress in science and engineering needed to establish world leadership," and its strategic plan identifies "maintaining U.S. leadership in key fields" as the goal of international collaboration in science and engineering.[33] The American Competitiveness Initiative is aimed at "keeping our competitive edge" and "continued leadership in innovation, exploration, and ingenuity."[34]

But the global division of labor is shifting. For the first time in history, the last two decades have seen mutual trade in manufactured goods between affluent and rising economies. The core economies of the industrial age are turning into service economies. Continued U.S. prosperity depends on finding national economic roles that fit our strengths and capabilities and are valuable globally. The distinction between "developed" and "developing" countries -- those that have arrived and those that are on their way -- is outdated. We are all developing together into new economic patterns, and the United States faces choices about its development path like every other country in the world.

Science and technology are a part of the shifting global picture. In every economic area around the world – agriculture, manufacturing, services, and mineral extraction – the introduction of new techniques and technologies is strongly associated with being able to sell goods or services and thus "compete" for both local and global markets. Furthermore, whole new sectors of the world economy, many believe, will be stimulated by new science-based technological capabilities. Those who lead in introducing those new technologies will gather wealth; those who follow will not. Every country would like to benefit from those new technological waves. As scientific capability builds up in the rest of the world, the United States might not be at the front of all these new developments.

A decade ago, "leadership" meant dominance. But as other nations develop economically and build their science and technology capacities, dominance is no longer a realistic goal for any nation. One possible response for U.S. S&T leadership is to redouble efforts and try to "stay ahead" of the rest of the world. An alternative response, however, is beginning to be expressed. Our objectives could be defined in relation to our own conditions, not someone else's; we could aim to "survive as a nation and prosper as a people," in a world in which "winning does not mean being Number One."[35]

The U.S. S&T policy research community can help in the search for such alternative formulations of an evolving role for U.S. science and technology in the world through a number of channels, including

1. developing more multi-dimensional theories and models of the roles of science and technology in global economic, social, and governance systems;

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[34] http://www.whitehouse.gov/stateoftheunion/2006/aci/
[35] http://www.aaas.org/news/releases/2006/0503hamre.shtml

2. providing a rich base of description and insight into the co-evolution of science and technology outside the U.S. and Europe, paying particular attention to the variety of forms of knowledge that contribute to local, national, and global development, and the voices of actors outside the established institutions of S&T policy decision-making;
3. breaking the silences of conventional S&T policy discussions, and helping to open discussion about important but neglected topics that relate science and technology to global development.

This essay will touch on each of these potential contributions in turn.[36]

Theories and Models

By far the dominant theories of global development that enter S&T policy discussions come from economics. Neoclassical theories of economic growth attribute growth over and above what is expected on the basis of capital and labor to improvements in technology. This view is alive and well in the many analyses of "total factor productivity" or the "Solow residual." Under this theory, because of the spread of technology and diminishing returns to capital, countries with lower levels of income are expected to catch up over time to those with higher incomes. New Growth Theory makes technology and knowledge themselves a force of production, on which returns do not diminish. Thus, countries with knowledge capital can accelerate growth indefinitely. "Catch-up" is much harder, and the accumulation of knowledge capital is the key. Human capital (which embodies knowledge in people) is a one of the key in New Growth.

Also from economics comes the work on innovation systems (national, regional, sectoral). An innovation system consists of a set of actors (usually government, research, and business), their relationships, and the process by which the system learns. Since learning is a version of knowledge accumulation, the concept of innovation systems helps to fill in some of the implementation details of New Growth Theory. Sectoral systems of innovation stretch across national boundaries, so this concept can be used to study global innovation processes.

Both growth theories and innovation system models are widely used in science and technology policy formation around the world. But as social theories, they are quite narrowly focused. On the one hand, they tend to ignore power and the possible distorting influence of power on economic processes. And on the other, they tend to ignore people, paying scant attention to human development issues while focusing on the activities of businesses and the supporting role of governments.

More multi-dimensional theories of global change that take technology into account, however, are scarce. There is Castells' monumental work on the Information Society, which analyzes dynamics on the global scale, tracing the influence of information technology far and wide, and including both people and power. But there is also room for much more work on understanding the global forces and patterns. In particular, we need a concerted effort that goes beyond mere critique of globalization to identify levers and opportunities for shaping its dynamics in positive ways.

Co-evolution, knowledges, and voices

The STS literature specializes in providing nuanced original accounts of the co-evolution of sciences, technologies, and societies. From the STS literature, we learn how power and privilege get inside scientific knowledge, through dynamics of gender and class, and we see how

[36] My treatment of the literature here draws on a draft chapter for the forthcoming second edition of the Handbook of Science and Technology Studies, co-authored with Sonia Gatchair, Kyung-Sup Kim, Gonzalo Ordonez, and Anupit Supnithadnaporn. I thank them for their insights and contributions.

those can they be reproduced through the institutional dynamics of science and technology. Yet the STS literature itself has privileged the sciences and technologies of the global North by paying very little attention to these co-evolutionary processes outside Europe and the United States. Our recent review found very few articles in Social Studies of Science or Science, Technology, & Human Values that paid attention to co-evolutionary processes in countries of the global South. If STS insights are going to inform the understanding of the place of U.S. S&T in the world, the field itself will need to become more global.

Conventional S&T policy focuses on formal, institutional science and on science-based technologies, and does not recognize the value of the vast array of other knowledges that science studies identify and respect. Yet these are crucial for the global development process. In our review, we identify the focus on knowledges as the strength of the STS literature on developing countries:

> … the STS literature implicitly portrays globalization as a process of knowledge confrontations. "Professional" or "scientific" knowledge carries the privilege of the North into the definitions that shape life in the South. It tangles with other ways of framing and addressing issues, particularly those rooted in the knowledge of poor or indigenous people. By treating the various forms of knowledge symmetrically, the STS approach draws attention to the asymmetries in power that privilege one form of knowledge over another. STS stories include a broad set of actors, especially highlighting civil society and marginalized groups, and features their categories and knowledge. The STS literature thus highlights certain questions with regard to development projects: Whose project is it? What knowledge do the various actors bring to the interaction? Whose knowledge gets respect and deference? What are the outcomes of the project for the everyday lives of the people involved? (from forthcoming chapter for STS Handbook)

"Indigenous knowledge" provides an example of the difference in perspectives. In conventional S&T policy discussions, the knowledge of indigenous people about their natural environments is treated as a commercial opportunity, a site for the establishment of intellectual property rights. In the STS perspective, however, the symmetric treatment of indigenous knowledge highlights its value in innovation and learning processes, and forms the basis for looking for the many other forms of non-professionalized knowledge that can also contribute to these learning processes.

Silences

Conventional S&T policy discourse embraces certain polite topics of conversation and avoids others. One of the contributions of a policy research community can be to bring important but uncomfortable topics into the conversation. This is our responsibility as intellectuals and as human beings. Let me mention two examples.

One is the area I have been working on over the last five years or so, inequality. This topic has traditionally not been mentioned in science and technology policy discourse.[37] Yet the work that my students and I have done shows that there are many relationships between issues of socioeconomic, gender, and ethnic inequalities and such sub-areas of S&T policy as research,

[37] One of the few voices in power to raise issues of the benefits of the science and technology for the poor was the late Congressman George Brown, in part under the influence of workshop participant Dan Sarewitz, who is himself also an exception to the general pattern I identify here.

innovation, and human resource policies.[38] On the global scale, the growing emphasis on market orientation in S&T policies in both the U.S. and Europe are certainly contributing to global inequalities, and new intellectual property regimes are exacerbating the situation. No one believes that inequalities can continue to grow in a sustainable global order. There are vitally important problems here, and too few of us voicing and analyzing them.

Another highly significant silence surrounds military technology. Well over half of U.S. public R&D expenditure is in the Department of Defense. It is impossible to picture a transformed view of the role of U.S. innovation in the world that leaves that pattern of expenditure in place. Yet very little effort in social studies of science and science policy studies is devoted to tracing the structure or implications of this investment.

Directions

Current world conditions make it clear that "U.S. leadership" in science and technology needs to be redefined. The best recent statement I have seen on new directions is National Innovation Initiative report issued by the Council on Competitiveness.[39] Its vision is of the U.S. as an Innovation Society, not just an Innovation Economy, a place where everyone innovates. The U.S. Innovation Society will take its place in the global growth by helping to address global challenges, in this vision. The NII report shows that leaders in business, academe, and labor are arguing that the United States needs to start to provide leadership in spreading global prosperity in a socially and politically sustainable world. It is possible to shift our concept of productivity, from producing more goods with less labor, to producing a better life for everyone with decent work for all. The social sciences surely have a great deal to contribute to that effort as well.

[38] See "Distributional Effects of Science and Technology-Based Economic Development Strategies at State Level in the United States," Susan E. Cozzens, Kamau Bobb, Kendall Deas, Sonia Gatchair, Albert George, and Gonzalo Ordonez, Science and Public Policy, February, 2005; "Measuring the relationship between high technology development strategies and wage inequality," Susan E. Cozzens and Kamau Bobb, Scientometrics 58 (2): 351-368, 2003; and "Evaluating the Distributional Consequences of Science and Technology Policies and Programs," Susan E. Cozzens, Kamau Bobb, and Isabel Bortagaray, Research Evaluation, 11 (August): 101-107, 2002.

[39] www.compete.org

Session 7: How Can the Social Sciences Inform Science Policy?

Stick with Advice Inoffensive to Science Policy Influentials?

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"What are the most fruitful ways for social science to inform science policy?," Session 7 speakers are asked. Perhaps the most basic lesson the social sciences have to teach is that social processes are problem-ridden; institutions take on lives of their own, for example, as by serving insiders at the expense of the supposed beneficiaries. Grasping that insight would open the way for probing decision making about science in a more fundamental way than anything I've seen hinted at in the workshop materials. Is it acceptable here to ask hard questions about NSF?

If so, we might start from psychologists' recognition that everyone has perceptual lenses and mental schemas that distort and filter, and that these tend to be shared with others in one's institutional/cultural setting. What systematic biases and misperceptions tend to be found among science policy influentials, and how might a civilization protect against the worst consequences of these? Am I right in perceiving some of our workshop materials as laden with conventional myths about science policy, myths that have been thoroughly debunked by STS scholars? If so, what is most needed from social science is not a bit more study of this or that facet of science or science policy, but a challenge to institutionalized science-think. Workshop participants will squander an opportunity to counteract insider biases if we (pretend to?) accept science policy framings infused with mythperceptions.

Whoever frames a debate or sets an agenda wields power, a commonsensical notion refined by several disciplines represented here. Science policy agendas are set without meaningful representation of most of humanity, and this prevailing mobilization of bias leads to overemphasizing some aims at the expense of others that are equally or more deserving. For example, chemistry funding deliberations systematically underrepresent critics of contemporary chemistry, including those who advocate benign-by-design "green" chemicals.

When public choices fail to take advantage of the potential intelligence of democracy, relevant considerations are neglected and imprudent actions become more frequent. Consider as an example what I take to be the main thrust of science policy as now pursued: ***MORE***. NSF insiders are not likely to question funding increases, of course, yet there is a good argument to be made against the notion that more research is an unalloyed good. Recent concerns about health consequences of nanoparticles illustrate why slower sometimes is better, a caution I would like to see applied to the entire nano-bio-info convergence. I hope that those pushing a rapid pace are simply ignorant of the huge swath of social science pertaining to human psychosocial and cultural fragility, but I suspect most simply do not care. Whatever the explanation, imprudence is near the heart of contemporary science policy, and it contradicts much of what STS scholars know about how to proceed intelligently in the face of uncertainty.

More generally, workshop participants ought to be asking whether expertise funded by NSF is being deployed *appropriately* to develop *equitably usable knowledge*. Not only am I skeptical that this is the modal practice, I doubt very much that NSF is organized to facilitate real probing of major shortcomings. Other workshop participants obviously may disagree, and my intention is not to argue for my views but to ask that we at least discuss the problems and possibilities of *organizational learning*. How might a science policy organization be structured so as to encourage deep, systematic, sophisticated probing of its own shortcomings?

Such an endeavor would require coming to grips with what public policy scholars refer to as the *limits of analysis* and the inevitably *partisan* character of knowledge in action. Better understanding per se does not translate automatically into improved policy or outcomes, in part because analysis rarely if ever resolves complex disputes. Deliberate change generally requires proposals that ultimately win assent from a majority coalition, proposals never fully justified on the basis of analysis. I wonder how many of us could agree on research directions strategically aimed at actually ameliorating serious shortcomings in science, science policy, and technological innovation. Maintaining the polite fiction that the workshop is aiming to "understand" the whole science/science policy system has the advantage of offending no one, I admit; but coming up with usable, important recommendations requires a rather different approach.

Implicit in the foregoing is the specter of social conflict. Looming large in the social sciences, conflict all but disappears in the workshop framing and in most other NSF discourse. I find that somewhat odd considering how many STS scholars have relied in one way or another on the everyday observation that facts and values intertwine inextricably. Anything to do with policy is inherently partisan, moreover, in the sense of promoting some version of the public interest that is not universally agreed upon. It is logically impossible for workshop participants to offer advice about improving science policy research without a stance on what would constitute "better;" there is no such thing as a knowledge shortfall without goals to be served by improved understanding; and efforts to specify such aims will be inherently partisan. Knowledge (and lack of it) matters, in other words, but usually as refracted through latent social conflict. All workshop participants know that; why don't we try to teach science policy participants to face up to partisan conflict?

Finally, social scientists know quite a bit about how institutions (mal)function, yet institution-oriented questions on the workshop agenda are not sharply focused. I assume we have insights about organizational culture(s) at NSF, about capture of the agency by those receiving funds, and about problems arising from intra-institutional machinations among the House Science Committee, mission agencies, and NSF. Others can correct me if they know better, but outside of some commercialization initiatives I have not seen much evidence of sophisticated thinking about institutional matters in science policy. Which leads me to wonder, "What are the main institutional forces shaping NSF administrators' disincentives to think social scientifically?"

In sum, I believe that it is intellectually indefensible to assume that U.S. R&D policy is so clearly on the right track that discussions such as ours can focus just on minor tweaking. I do not believe that shaping science and civilization will be significantly improved by doing a bit more science policy research in order to come up with some specific new "knowledge." For the core problems in science policy are the same ones that trouble the rest of our civilization: misperception and nonlearning, mobilizations of bias, badly designed institutions, and other deeply embedded aspects of contemporary power structures and governing mentalities.

I therefore urge workshop participants to frame our report so as to challenge rather than coddle science policy influentials. If we skip over the unpleasant fact that mainstream NSF discourses and practices tend to be nationalistic more than global, elite-serving more than populist, science-serving more than outcomes-oriented, and technocratic more than democratic, we will be throwing out a large fraction of what social scientists have to contribute to fairer and more effective science policy processes.

The Study of Public Reasoning

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A key area where the social sciences can contribute both theoretically and pragmatically to the science of science policy is via *the study of public reasoning*.

Perhaps most obviously, the study of public reasoning in the social sciences examines how science advice is brought to bear on policy decisions. Science policy has long been classified in a binary fashion—policy-for-science and science-for-policy—with the former receiving the lion's share of attention. As has become clear recently, however, the nation's ability to deploy science credibly in support of policy decisions is an essential element in securing broader public trust in democratic governance (Ezrahi 1990). Moreover, securing credible science advice has proven neither simple nor straightforward in many cases (Jasanoff 1990), leading to the elaboration of extensive policies governing the information and knowledge flows entering the policy process. Not only do these policies establish rules and procedures for the scientific advisory process itself, but they also often establish standards for knowledge production (as in NIH and FDA guidelines and auditing of clinical trials) and even pursue research and inquiry internal to regulatory processes (as in EPA laboratories and external grant programs designed to serve EPA rulemaking). In terms of broad legislation in the United States, for example, relevant policies include the Administrative Procedures Act, the Federal Advisory Committee Act, the Data Quality Act, the Freedom of Information Act, recent OMB guidelines for peer review, etc. These policies enable and constrain the creation, dissemination, review, and use of scientific and expert advice in policy contexts. A science of science policy must therefore grapple persuasively with these dimensions of policy-for-regulatory-science if it is to provide a strong foundation for future policymaking.

Public reasoning, of course, is broader than simply the provision of scientific or expert advice. Science advice is one element in a diverse array of formal and informal means by which societies identify, frame, evaluate, and make sense of the policy challenges they face. Also important are the policy sciences (e.g., cost benefit analysis, policy analysis), statistical data collection and analysis (e.g., inflation, crime, demographics), lay and indigenous knowledges, cultural narratives, legal constructions of facts, and many more elements of what has also been called *civic epistemologies*—the standards of evidence, forms of expertise, norms of warrant and review, and styles of reasoning that characterize the formation, deliberation, and implementation of public policy. Science advice is thus but one part of a more complex, dynamic interplay of social ideas that shapes the ultimate winnowing and selection of policy choices, as well as public reactions to major policy events and crises. Here, too, the social sciences are prepared to contribute in important ways to understanding this dynamic interplay of ideas and, especially the role of science and expertise in relation to other, competing models of knowledge and rationality.

In what follows I describe three key contributions where I think social science research on public reasoning has made significant contributions to science policy and is prepared to continue to do so. These include (1) the identification, analysis, and potential resolution of multiple, conflicting styles of reasoning; (2) the design and analysis of reflexive governance; and (3) the strengthening of democracy in national and global contexts.

Resolving conflicting styles of reasoning

In recent decades, myriad public policy choices have given rise to intense disputes over the truth status of knowledge claims in international affairs. Diverging interpretations of evidence and analysis have occurred over the risks of climate change and biodiversity loss, the status of Iraqi, North Korean, and Iranian nuclear facilities, the safety of trade in genetically modified organisms, and the adequacy of plans for containing outbreaks of emerging diseases, to name only a few. In this respect, global politics increasingly mirrors its domestic counterpart, for which disputes over knowledge claims have become an endemic element of modern and postmodern politics (see, e.g., Nelkin 1984; Epstein 1996).

An important element of these conflicts results from significant divergences in what the philosopher Ian Hacking calls *styles of reasoning*. Hacking's work focuses on competing modalities of reasoning within the scientific community: interlocking forms and standards of evidence, problem framing, and logical analysis (Hacking 2002). Building on similar ideas, research on comparative politics and regulation has also identified distinctive styles of reasoning operating in the ways that cultures frame and analyze risk (Jasanoff 2005, 1995a, 1986; Parthasarathy 2004; Daemmrich and Krucken 2000; Rayner and Malone 1998; Krimsky and Plough 1988). At play are divergent normative and institutional frameworks for integrating scientific and expert advice into distinctive constitutional politics. Institutions and cultures vary in how they define who counts as an expert for the purpose of advising policy, requisite standards of evidence and proof, how problems get framed, and the treatment of uncertainty. Brought into dialogue in global politics, divergent national styles of reasoning (embedded in national political cultures) become seeds of international conflict in an era of globalization.

Social science research can help science policy confront the increasingly numerous challenges cultural divergences in styles of reasoning pose for international governance. Analyses of conflicting styles of reasoning can help explain why particular disputes arise and how they get resolved (or not) within emerging institutions and frameworks for international governance (Miller forthcoming, b). They can also contribute to identifying and evaluating novel approaches for overcoming such conflicts and for establishing robust approaches to global reasoning that can speak credibly to myriad, heterogeneous publics in global society (Miller 2005, forthcoming, a; Jasanoff 1998). Relevant work would be positioned, for example, to assist the World Trade Organization in resolving differences between U.S. and European risk analysis frameworks (Winickoff et al. 2005, 2004) or to help the International Atomic Energy Agency and major powers develop more sophisticated mechanisms for validating competing claims regarding nuclear non-proliferation treaty violations.

The design of reflexive governance

The U.S. Army Corps of Engineers internal report on the impact of Hurricane Katrina on New Orleans levies, as well as similar reports by other agencies and groups, documented a widespread series of failures in both the initial design and construction of the levies as well as the systematic monitoring of the levy infrastructure over time. Supplemented by historical and sociological accounts of other natural disasters and technological accidents, as well as policy failures, these reports suggest significant problems in contemporary governance in how knowledge concerning the creation and monitoring of scientific and technological systems is constructed, synthesized, validated, and applied (see, e.g., Goldman 2005; Scott 1998; Jasanoff 1994; Perrow 1986; Wynne 1982). These problems highlight an additional important contribution that social science research on modes of reasoning can make by analyzing the modes of reasoning underpinning policy decisionmaking in the governance of science and technology. Just as importantly, they suggest the need for a more systematic effort to apply social science research to the challenge of designing more *reflexive forms of governance* that systematically attend to their own epistemological and ontological frameworks and the knowledge framings, tacit assumptions, normative commitments, and power relationships they embed.

In the case of Katrina, for example, a more reflexive Corps of Engineers might have recognized a number of limitations in its own analyses. For example, more sophisticated reflection on the Corps' monitoring of New Orleans' levies might have revealed both the inadequacies of the monitoring system—and, if corrected, might also have revealed the structural problems left over from poor construction (which might themselves have been identified and repaired at an earlier stage if the oversight of the construction had been carried out with a more fine-tuned sensibility to the vagaries and uncertainties of technological systems). The Corps might also have been more carefully attuned to the potential limitations of its framing of the problem of storm surge and might have identified the need for further study of what might happen if the water entered the levy system from a different direction than expected. Or, the Corps (or perhaps more appropriately the Federal Emergency Management Agency) might have realized the ways in which levy technologies, class, and race were combining to create particularly vulnerable communities within New Orleans and to prepare to address those vulnerabilities.

Reflexivity is a term borrowed from the sociology of scientific knowledge literature that emphasizes the capacity of social arrangements to recognize, acknowledge, and factor into knowledge claims the limitations, assumptions, and power relationships of any given epistemological framework—to reflect, in other words, on one's own knowledge-making processes in the act of making and applying knowledge. Reflexive governance thus implies the capacity to reflect on the styles of public reasoning at play in policy choices and to make allowances for the constructed character of that reasoning as policy issues are identified, defined, assessed, and resolved. As understood here, reflexive governance applies both to particular institutional decision-making contexts where various forms of expert knowledge are applied as well as to the broader constellations of public decision- making processes in contemporary societies, including not only executive agencies but also legislative and judicial bodies and civil society. Obviously, the functioning of reflexivity will be different at these different scales, however.

In addition to the large-scale technological systems discussed above, the social dislocations associated with new and emerging technologies are another context in which the study of reflexive governance could offer important contributions. New and emerging technologies often raise complex notions of risk that cannot be fully captured by existing risk analytic methods. They also often pose challenges to more fundamental notions in society, such as responsibility, privacy, agency, and causality. Understanding at many intersecting levels of society how publics acquire, collate, and assess information about new and emerging technologies would be of considerable value in the formulation of science policy. So, too, would analyses of existing processes for managing technological change, such as those embodied in the U.S. Food and Drug Administration and Environmental Protection Agency, as well as the less structured frameworks of legal interpretation and precedent for resolving technological conflict (see, e.g., Jasanoff 1995b) or a variety of still more informal avenues of social mobilization and critique.

How does reflexivity work in these institutions, in different issue area and cultural contexts? What, in other words, does it mean for governance to be reflexive? How would one recognize it action? How do reflexive elements of governance work?

From such studies, it would be possible to begin to develop insights into the design of reflexive governance institutions (Miller 2003, 2004a), drawing not only on the social study of knowledge systems but also on literatures in organizational learning and institutional change. Such research could address challenges such as the systematic institutionalization of critical review into knowledge-making processes; the identification of gaps between expected and achieved outcomes; the design of processes for scanning society for changes that may challenge existing framings and modalities of reasoning; identifying, understanding, and resolving conflicts among styles of reasoning; systematic comparison as a learning model; sampling public values and perspectives and incorporating public consultation; dispute resolution (and also, maybe, the fostering of appropriate forms of dispute); acknowleding and managing uncertainty; and building capacity to respond to novelty and change.

Strengthening democracy

The final contribution social scientists are poised to make to science policy through the study of public reasoning is to strengthening democratic forms of governance. Put simply, science and technology are central elements in liberal democracy (Ezrahi 1990; Kleinman 2000). Historically, science has played important roles in a wide range of settings in suipporting the emergence of democratic forms of civil society (Nyhart and Broman 2002) and has been embedded in the broad transformation of democratic politics wrought by the Progressive and New Deal states (Hays 1959), which brought experts into the organization of the U.S. federal government by the tens of thousands. In the latter half of the 20th century, this bureaucratization of expertise was supplemented by the extensive construction of scientific advisory committees that brought external expertise to bear on the use of technical knowledge in the nation's regulatory agencies (Hilgartner 2000; Jasanoff 1990).

While many theories of democracy recognize the contribution of expertise to deliberation and democratic decision-making, these frameworks rarely if ever incorporate the increasing depth of social science understanding of the construction of science and scientific advice. Theories of deliberation, for example, rarely explore in depth how various kinds of claims acquire crediblity in deliberative contexts and the implications of variations across domains of claims-making for democratic processes. Likewise, even in the aftermath of the Bush-Gore contested election in 2000, analyses of electoral stability and processes in mainstream democratic theory paid scant attention, at best, to the ways in which knowledge claims were made, constested, and resolved in electoral politics, thus missing critical elements and leading to problematic proposals for electoral reform (Miller 2004b; Lynch et al. 2001).

There is a broad need, therefore, to apply systematic social science insights to our understanding of contemporary democracies, which largely came to be centered in the 20th century on the politics, governance, management, and application of scientific knowledge and technological systems, from the regulatory politics of EPA and FDA to questions of defense and homeland security, the mass-production consumer economy, transportation, the Internet, health care, etc. This is especially true today, in the context of widespread conflict over the public status of science in both regulatory governance and, more broadly, in civic life. While conflicts over the politicization of science advice and the teaching of evolution in schools are hardly new, public debates over these issues remain caught up in fundamental misunderstandings of the role of science in contemporary democratic forms of governance.

Arguably even more importantly, there is a need to apply this research to the challenge of designing new forms of global governance. Although globalization is often understood primarily in economic terms, it is clear that it is also a deeply scientific and technological transformation of social relationships. Critical problems in contemporary international relations, from efforts to regulate climate change, nuclear proliferation, and emerging diseases to the Internet, cloning, and HIV/AIDS depend on elaborate efforts to systemically collect, analyze, and apply expert knowledge to the governance of global-scale technological systems (e.g., in energy production, transportation, forestry, pharmaceuticals, etc.). Precisely because these governing arrangements are novel, and thus still fragile and, in many cases, weak and ineffective, and lack the kind of systematic support provided in national governance by constitutional frameworks, there is considerable need for robust analyses of how these systems can be strengthened in ways that are consistent with democratic principles. Recent studies have illustrated the potential contributions social scientists can make in this regard, both with respect to the organization of international scientific advisory processes (Miller forthcoming, a) and, more generally, in the transformation of diplomatic processes toward greater deliberation (Miller forthcoming, b).

Training

By way of conclusion, I want to highlight the need not only for social science research on public reasoning but also the need for more systematic training opportunities in this field. While a handful of graduate training programs were established in the fields of science policy and science studies in the early 1990s, these programs train students in the study of public reasoning only at the margins. At the same time, there has been little subsequent progress in expanding the institutionalization of these fields since the early 1990s to other universities. For the most part, these topics also do not attract significant attention within the fields of political science or public affairs. Consequently, it is fair to say that there are currently no systematic graduate training programs in the study of processes of public reasoning.

Two kinds of graduate training programs would be of considerable value. The first would focus on PhD education at the intersection of science policy, science studies, and political science. Here, the ambition would be to prepare future academic researchers to participate in the ambitious tasks set out in the previous sections. The second kind of graduate program would focus on professional education at the Master's level, with an emphasis on translating research insights on public reasoning into concrete skills to be put to work in the management of technical agencies and policy settings where knowledge is brought to bear on policy choices. Relevant skills and knowledge would include such areas as knowledge systems analysis, technological systems analyis, design of reflexive governance institutions, theories of science and democracy, techniques of public consultation, and so forth.

At the professional level, a degree in the management of public reasoning would parallel existing degrees in public affairs and public policy. Public reasoning is a central element in all government policymaking. Yet, few if any public affairs programs offer in depth skills in applying insights from the last three decades of social science research on knowledge and expertise in the policy process to the making of public management and public policy decisions. To make headway, significant efforts need to be made to create a generation of public managers and administrators who will work to transform institutional processes of knowledge creation, integration, critique, and application from the inside and who are able to build effective networks with the social science research community to bring new ideas into the public management process.

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The Dismal Science and the Endless Frontier

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Introduction

John Marburger, the President's Science Advisor, recently has argued that more attention and resources need to be devoted to the "social science of science policy." He has also stated that science policy "is to a great extent a branch of economics, and its effective practice requires the kind of quantitative tools economic policy makers have available including a rich variety of econometric models, and a base of academic research."

As an academic economist studying science policy, I applaud this initiative. The application of economics to science policy is in my mind natural since–at least according to its common textbook definition –economics is the "study of the allocation of scarce resources." And, of course, much of science policy is about allocation decisions.

But I see two major obstacles in moving forward. One is on the supply side, reflecting limits to policy prescriptions from economic research. The second is on the demand side, and reflects the scientific community's collective–and I argue unscientific– devotion to the hypothesis that science best serve society when allocation decisions are based primarily on scientific interest. The most commonly employed output of academic economic research in science policy has been the "market failure" theory of scientific research, developed by Nelson (1959) and Arrow(1962). According to this framework, the informational outputs of scientific research have the two defining characteristics of a "public good." They are non-rival: multiple parties can use them simultaneously, and they are not depleted upon use. And they are difficult to appropriate: any given firm will only be able to capture a fraction of the total benefits from investments in scientific research. Because of these "spillovers," social returns from such investments exceed private returns, and firms will tend to under invest in the production of science relative to the social optimum. Accordingly, government intervention can improve upon the market outcome. There are various ways in which the government could intervene to improve upon the market outcome, one of which is government funding of science.

This "market failure" justification for government funding has been influential in the post war era. Indeed, it was anticipated by Vannevar Bush in *Science, The Endless Frontier*:

> There are areas of science in which the public interest is acute
> but which are likely to be cultivated inadequately if left without
> more support than will come from private sources (12)

and later:

> [B]asic research is essentially non-commercial in nature. It will
> not receive the attention it requires if left to industry.

Similar statements have been invoked in Congressional testimony for increased funding of science in the post war era, and have appeared in almost every volume of the *Economic Report of the President* since the early 1960s.

But while market failure theory provides support for the argument that government should fund some level of science, it does not automatically tell us how much should be funded, or what types of science should be funded. As Nelson (1987) has argued in a trenchant critique of the market failure approach, market failures are ubiquitous, and the extent of market failure in a particular context depends crucially on modeling assumptions about which there is generally little scientific consensus.

Moreover, even *ex post* evaluation of the impact of government funding of science is notoriously difficult. While this is true of economic research more generally, a range of measurement issues specific to science policy make evaluation difficulties even more pronounced in this context (Jaffe 2002; 1999; David et al.1992).

Accordingly, the market failure framework does not provide "off the shelf" policy prescriptions, but can be used to argue for (or against) funding just about any type of research. Of course, the notion that research results have interpretative flexibility is well– known in the "science for policy" literature, so it is not at all surprising that this is also true in the context of "social science for science policy."

I have argued elsewhere that the room for social construction of research outputs is greatest in contexts where it is difficult to get sharp feedback on interventions (Nelson, Peterhansl, and Sampat 2004), and I do believe that better data, and more creative empirical approaches (see Jaffe 2002; Garber and Romer 1994; David, Hall, and Toole 2000) could help ameliorate these issues. And this would be an important step on the road to a more scientific science policy.

But even if we addressed these measurement issues, a more fundamental issue would remain. Specifically, if we take the market failure approach seriously, we would want to fund research with the largest *spillover gaps* i.e. where the difference between social and private rates of return is greatest (Jaffe 1999). In part this would reflect the appropriability of different types of research (which, as an aside, are rarely discussed in the allocation process). But more importantly, comparing the size of spillover gaps would reflect the extent of (expected) social returns to various types of research. But as the case of human embryonic stem cell research illustrates, perceptions of the social benefits (and costs) of research reflect competing value judgments. In this case, and I suspect most others, there is no objective scientific answer to what type of research yields the highest social returns. Ultimately, we will generally find ourselves back in the messy and unscientific world of values and politics. But even if we could develop useful measures of the expected social (and private) returns to particular lines of research, I'm still not certain that it would be clear– sailing to a more scientific science policy. The reason is that the science policy community clings to what Dan Sarewitz has termed "the myth of unfettered research", or the notion that the best way for science to contribute to society is via doing research that is scientifically interesting.

The most famous and most influential statement of the notion that resources for science should be allocated primarily according to scientific interest in a field was, again, Vannevar Bush in *Science, The Endless Frontier*:

> Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity

for exploration of the unknown." Bush (1947, 12)

Together with Bush's assertion that science is economically and socially important and that the market won't fund it adequately, this characterization of science setup Bush's policy prescription: government funding of science without government control of research agendas.

This view of the best way to allocate resources has been surprisingly persistent. For example, Nobel Prize winning biologist Arthur Kornberg recently wrote:

> [P]lans are fundamentally flawed because discoveries are commonly serendipitous. The best plan over many decades has been no plan.

Or, consider former NIH director Harold Varmus's testimony to the Senate Appropriations Committee in 1999:

> Because science attempts to discover what is unknown, it is inherently unpredictable; in this sense, it is unlike most industries, which can employ well-established methods to generate planned amounts of known products. History has repeatedly shown the benefits of allowing a significant portion of our research activity to be governed by the imagination and productivity of individual scientists, not by a regimented plan for alleviating diseases we do not yet fully understand

The implicit causal hypothesis here is that allocating resources to research to areas that are scientifically interesting will maximize the social benefits from investments in science. What is striking about this hypothesis, in addition to its persistence– is how little empirical support it has. This hypothesis is almost always made with appeal to specific examples of scientific research that yielded unexpected, valuable, outputs. But of course, much (if not most) curiosity-driven research yields no valuable outputs: surely that should be accounted for in calculating a rate of return? Moreover, is there really any evidence that the benefits from this type of research exceed those from "planned" research? It strikes me that much of the body of economic research showing high social benefits from "basic" research is in fact based on measurements from mission oriented research, e.g., research funded by the NIH or Department of Defense (see Hall 1992 for a review).

Nor am I aware of a theory of knowledge from which this hypothesis derives. Why should it be the case that "scientifically interesting" research agendas are also most fruitful for advancing public goals? I suspect there is occasional overlap between scientific interest in a problem and its social importance, but to my knowledge there is no sociological, epistemological, or economic theory of science which suggests that such overlap should be the rule rather than the exception.

A more plausible version of this hypothesis is that scientific opportunity is one important variable to consider in allocation decisions. In other words, all else equal, one should fund lines of research that are scientifically more feasible. And certainly, scientists are likely in a better position to make judgments about scientific feasibility than are others, though probably not as qualified to make decisions about which problems are important to society. Another reasonable argument along these lines is that because of benefits from serendipity, policymakers should fund some “undirected” research.

But the hypothesis that science best serves society when allocation decisions are based purely on scientific merit, and the related hypothesis that useful science cannot be planned, has little scientific support. A collective rejection of this hypothesis by the scientific community would be an important first step on the road to a science policy informed by economics, or by the social sciences more generally.