Division of Social & Economic Sciences

Program Responses to the 2007 COV Report

Includes Responses to the COV Reports for the Disciplinary Programs of SES:

Decision, Risk & Management Sciences
Economics
Innovation & Organizational Change
Law and Social Science
Methodology, Measurement, and Statistics
Political Science
Science & Society
Sociology

Directorate for Social, Behavioral and Economic Sciences
Division of Social and Economic Sciences
Program Responses to the 2007 COV Report

A Committee of Visitors (COV) convened March 8-10, 2007 to review the disciplinary programs in the Division of Social and Economic Sciences (SES): Decision, Risk, and Management Sciences Program, Economics Program, Innovation and Organizational Change Program, Law and Social Science Program, Methodology, Measurement and Statistics Program, Political Science Program, Science and Society Program, Sociology Program. In attendance were 25 members, four members of the Social and Behavioral Sciences Advisory Committee, including one COV Chair, who met in plenary and in program-focused and cross-disciplinary sessions at different times of the meeting, as well as the current SES Program Officers who joined the COV for selected portions of the meeting… SBE Assistant Director David Lightfoot, SES Division Director Edward J. Hackett, and SES Senior Science Advisor Frank P. Scioli addressed the COV to brief the members on selected issues including the Government Performance and Results Act, the Directorate and Divisional structure, and conflicts of interest.

The Division of Social and Economic Sciences is extremely grateful for the input provided by the Committee of Visitors, and especially for the leadership of John King in chairing this process. Endorsement of the efforts of the Program Directors and of the portfolio of funded projects is reassuring.

This document provides an overview of the process provided by John King, SBE Advisory Committee Member and Chair of the COV, and addresses, on a point-by-point basis, specific comments and recommendations provided by each of the Program COV members.

SUMMARY REPORT FROM
SBE ADVISORY COMMITTEE MEMBERS ON THE SES COV
John Leslie King (Chair)
Cecilia Conrad
Shari Diamond
Guillermina Jasso
March 15, 2007

Background

Four members of the Advisory Committee (AC) of the Social, Behavioral and Economic
Sciences (SBE) Directorate participated in the Committee of Visitors (COV) meeting for
the Social and Economic Sciences (SES) Division, held at NSF March 8-10, 2007. They
joined a group of 25 scholars working in teams conducting COV reviews for each of
eight SES programs: Decision, Risk and Management Sciences (DRMS); Economics
(Econ); Innovation and Organizational Change (IOC); Law and Social Science (L&SS);
Methodology, Measurement and Statistics (MMS); Political Science (PoliSci); Science
and Society (S&S); and Sociology (Socio). The four members from the SBE AC worked
among the program COV groups on May 8 and 9, read the reports of those groups on
May 10, and produced this report.

This report is not a comprehensive summary or recapitulation of the individual program
COV reports; each of those reports constitutes a stand-alone document for use of
program, division and directorate leadership. The purpose of this report is to provide an
overall assessment of the SES Division based on a cross-cutting examination of the
program COV reports, deliberations by the program COV members, and discussions
among the four SBE AC members and the leadership of the SES Division and the SBE
Directorate.

Overall Assessment

There are two broad purposes of this COV. The first is to assess the integrity and
efficiency of the SES Division with respect to its baseline responsibilities of soliciting
and appropriately reviewing proposals, making research awards that are likely to produce
useful results, and meeting specific NSF goals such as broadening participation in
scholarly research by geographic region and type of institution, as well as among
individuals (e.g., race/ethnicity, gender). These are covered in the questions provided in
parts A.1 through A.5 of the template given to each COV (See Table 1). The other is to
assess the SES Division with respect to its performance in achieving the overall goal of
producing knowledge beneficial to science and the national welfare. Information on this
goal is provided by answers to questions in parts B and C of the template, as well as
commentary accompanying the overall reports and discussions among program COV
members, the SBE AC COV members, and the SES and SBE staff during the COV
process.

Table 1: COV Chair's Summary of Program COV Reports								
	Econ	DRMS	IOC	MMS	S&S	L&SS	PoliSci	Socio
Part A.1								
1	Y	Y	Y	Y	Y	*	Y	Y
2	Y	Y	Y	Y	Y	*	Y	Y
3	Y	Y2	Y	Y1	Y	*	Y	Y
4	Y	Y1	Y	Y	Y	*	Y	Y
5	Y	Y	Y	Y	Y	*	Y	Y
6	Y	Y	Y	Y	Y	*	Y	Y
7	NC	NC	C	C	C	*	NC	C
Part A.2								
1	Y	N	Y	Y1	Y	Y	Y1	Y
2	Y	NC	Y	Y	Y	Y	Y	Y
3	Y	NC	Y	Y	Y	Y	Y	Y
4	NC	NC	C	C	C	C	C	C
Part A.3								
1	Y	Y	Y	Y	Y	Y	Y	Y
2	Y	Y	Y	Y	Y	Y	Y	Y
3	Y	Y	Y	Y	Y	Y	Y	Y
4	Y	Y	DNA	Y	Y	Y	Y	NA
5	NC	C	C	NC	C	NC	NC	C
Part A.4								
1	Y	Y	Y	Y	Y	Y	Y	Y
2	Y	N1	Y	Y	Y	Y	Y	Y
3	Y	Y	Y	Y1	Y	N1	Y	Y
4	Y	Y	Y	Y	Y	Y	Y	Y
5	Y	Y1	Y	Y	Y	Y	Y	NA
6	Y	Y	Y	Y	Y	Y	Y	Y
7	Y	Y	Y	Y	Y	Y	Y	Y
8	Y	Y	Y	Y	Y	Y	Y	Y
9	Y	Y	Y	Y	Y	Y	Y	NA
10	Y	Y1	Y	Y	Y	Y	Y	Y
11	Y	Y	Y1	Y	Y	Y	Y	Y
12	Y	Y1	Y1	Y	Y	Y	NC	Y
13	NC	C	NC	NC	C	NC	NC	C
Part A.5								
1	Exc.	Exc.	VG	Exc.	VG	VG	Exc.	VG
2	NC	C	C	C	C	C	C	C
3	NC	C	C	C	C	C	C	C
4	NC	C	C	C	C	C	C	C

Legend	Y	Yes		N	No
	Y1	Yes, with exceptions		N1	No, with exceptions
	Y2	Often, not always		DNA	Data Not Available
	NA	Not applicable			
	C	Comment provided (NC is no comment)			
	*	Commentary rather than specific answers to questions			

As Table 1 shows, the program COV teams find the SES Division to be doing an excellent job with respect to integrity and efficiency. There are some exceptions to a uniformly positive assessment, but a careful reading of the actual comments provided by the program COV teams shows these exceptions to reflect either peculiar characteristics of a given program (e.g., a challenge resulting from a program's special mandate), or general concerns raised in one way or another by all programs (e.g., a need for increases in funding in order to accomplish goals). None of the project COV teams expressed integrity or efficiency issues that warrant remedial attention. In all, the SES Division is doing an outstanding job in this regard.

That said, there is room for improvement in the SES Division. The remainder of this report is devoted to three broad areas of concern identified by the SBE AC COV members.

Areas of Concern

Rather than reflecting deficiencies, the concerns expressed below represent conditions that create missed opportunities for SES, SBE, NSF, and the nation. SES can be contributing much more significantly. We (the members of the SBE AC on the COV team) recognize that these concerns are not easy to address. If they were, they would already have been resolved. Moreover, some of them will require considerable time and effort to address. Our intention is to alert the leadership of SES, SBE and NSF, and to encourage action on these matters.

There are three general areas of concern: administrative strategy, scientific challenges and impacts, and SES in the larger SBE and NSF context.

Administrative Strategy

The program COV teams uniformly compliment the quality of the administration of SES programs. In this, they echo the reports of previous COV teams. Unfortunately, the current review also reinforces earlier observations that administrative resources are insufficient to deal with the workload that SES programs *currently* face. We go beyond this assessment to suggest that these resources are particularly inadequate for the workload that the programs *ought to* face in order to rise to their full potential. NSF is justifiably proud that the vast majority of its funding goes directly to research and related activity, and only a small fraction goes to administration. However, there is a fine line between virtue and vice in this matter, and we believe that SES has crossed that line in the wrong direction.

No self-respecting researcher is going to make an appeal for administration over research, but as the expertise available in numerous SES programs clearly suggests, under-investment in administrative capacity is a bad administrative strategy. This is especially true when demands for administrative attention are rising in a non-linear manner relative

to the core work (e.g., research itself) and the opportunities ahead require serious strategic attention that only administrative leaders can provide.

We believe there are two reasons for this situation. One is the rising demand for compliance with expectations regarding the handling of solicitations, proposals, awards, and post-award reviews. As new expectations have been added (e.g., for investigator attention to diversity, K-12 participation, undergraduate research involvement), more administrative effort is required to meet these expectations throughout the life-cycle. The other is the additional administrative workload faced by program officers and others as it becomes more difficult to secure a sufficiently broad and deep set of proposal reviews. As the funding rate drops, the psychological contracts between the SES program officers and the research community become more fragile. Those writing proposals are also those who are called on to review proposals. When the likelihood of getting an award declines, the number of researchers writing proposals declines, and the number of people willing to participate in review declines. Increasing the percentage of proposals funded might reverse this trend, but doing so requires administrative effort as well as funding increases. In the meantime, a vicious circle is created in which program officers have to work harder simply to keep up. This is demoralizing to permanent staff and works against attracting the strongest people into rotator positions.

The other serious problem with the overload situation is missed opportunities that inevitably follow. NSF rightly prides itself on being a "bottom-up" enterprise that gathers the best ideas from the distributed community of scholars through various mechanisms (e.g., COVs, workshops) and synthesizes these ideas into new programmatic initiatives that improve the science being done. However, this community is also amorphous, and by itself cannot produce programmatic initiatives that stimulate new scientific endeavor *and* conform to the necessary protocols required by NSF. Such work must be done by the expert staff in the programs, divisions and directorates. If those people are preoccupied simply trying to keep up with the administrative workload, they cannot devote the necessary energy to collecting and synthesizing ideas to produce new programmatic visions and transforming them into reality. Something has to give, and in most cases it will be innovation. As we suggest below, this is a bad time for the SES Division to be hampered with respect to innovation.

Contrary to conventional wisdom, the low percentage of SES funding spent on administration does not necessarily encourage high efficiency. In our view, SES is already at a point where insufficient support for administration is hampering the nation's ability to get a full return from the research investment now being made in SES programs. This should be addressed immediately. Fortunately, SES itself contains programs with expertise in such matters. It might be a good idea to invest some SES resources in mobilizing and applying that expertise to the work of SES itself.

One other area of administrative strategy is the establishment and maintenance of an effective balance between permanent and rotator staff within SES. Permanent staff bring the advantages of organizational memory and institutional capability, not to mention many years of experience and lore in the sciences of their own expertise. However,

permanent staff seldom have the opportunity to be directly involved in research on a regular basis themselves: they are vicarious researchers. Rotators, in contrast, are usually active researchers who come in from ongoing research programs at their respective institutions. They have a good grasp of the state of their fields, bring new ideas to SES, and also have their own social networks that can help broaden the social networks of the division. However, they have a limited understanding of how NSF works and they are usually not in place long enough to learn what they really need to know before they are planning to leave. The right balance of permanent staff to rotators is a vital goal of administrative strategy for SES. We cannot suggest exactly what that balance should be or how it might be achieved across the SES programs, but we encourage the SES Division and the SBE Directorate to consider how to achieve the appropriate balance, not only across programs, but also within programs.

Scientific Challenges and Impacts

Individual program COV reports make many useful observations about challenges and impacts at the program level. We have screened these observations and chosen for discussion a set of issues that also were raised in our conversations with program COV team members and with staff from the SES Division and the SBE Directorate. For each of the issues we discuss both challenges and impacts. We have five focal concerns under challenges and impacts: mechanics of disciplinary and cross-disciplinary research; broader and deeper advancement in science; theory, method and measurement; the role of oral tradition; and the need for training the next generation.

Mechanics of disciplinary and cross-disciplinary research. It was noted by more than one program COV team that research with roots in particular disciplinary traditions can be found not only in the core disciplinary programs (e.g., economics, political science) but also in so-called "cross-cutting" endeavors. These include cross-cutting programs (e.g., DRMS) and much broader NSF-wide initiatives such as Human and Social Dynamics. These cross-cutting programs and initiatives are a relatively recent innovation in NSF, and they are important to bolstering NSF's ability to bring scientific expertise to bear on topics important to national welfare. In addition, the funding mechanisms of NSF-wide cross-cutting initiatives can have important downstream impact on disciplinary funding, as support for the original cross-cutting initiatives reverts to base funding in the directorates and divisions. There was concern, therefore, that the COV mechanism for reviewing disciplinary programs does not allow for close examination of related disciplinary work going on in cross-cutting programs and initiatives. Typically, there is one COV to look at the disciplinary area, and another to look at the cross-cutting program. It is difficult to determine the quality and impact of the work in a given discipline across all the programs where such work is being done. Much has been said about the problems of disciplinary stovepipes but it is possible as well to create problems from cross-disciplinary stovepipes. Mechanisms are needed to avoid both vertical and horizontal stovepipes, or at least to link them together.

Broader and deeper impact. Several of the program COV teams observed that the "broader impact" criterion for evaluating proposals and the results of SES-sponsored

research is problematic. Investigators and reviewers do not seem to have, or at least use, a common framework or vocabulary for describing broader impacts, making it difficult to ascertain and to communicate what the broader impacts of the research might be. This suggests a larger, more generic problem with framing the purpose as well as the payoffs of SES sponsored research. At some level, every research project contributes broadly, even if only to the immediately adjacent lines of research of which it is a part. And some projects contribute very broadly, not only to knowledge and capability within their own branch of science, but to all of the sciences. The question of what research projects *ought to contribute* seems unresolved, and if that is so, it will be difficult to evaluate their merits. We need a careful examination of how SES sponsored research projects might contribute to the overall scientific enterprise, and mechanisms for evaluating whether the impacts actually materialize in post-project assessments. Clearer guidelines should result in an improvement in the attention investigators give to this question in their proposals, and in the attention the reviewers give to this question in the review process.

Beyond the question of broader impact, there is a corollary concern with what we call deeper impact. By this we mean impact on the core capability and capacity of the SES fields themselves, especially with respect to building the full range of complements needed for the fields. One example of this has to do with the relationship between theoretical work and empirical work. Some fields have been aggressive in developing both theoretical and empirical dimensions of their science, and the expectation that theory will contribute to empirical work and vice-versa is deeply embedded in routine practice. However, other fields have not developed such conventions, and either theory or empirical work dominates, sometimes to the near exclusion of the other. This report is far too short for a discourse on the connections between theoretical and empirical work and their contributions to the advancement of scientific knowledge; suffice it to say that the challenge is to have an appropriate balance of theoretical and empirical work in any given field of inquiry, with the two explicitly interconnected. We suggest that the SES Division undertake a careful assessment of this balance in all of its fields, with the intention of achieving a desirable balance in each field. We note that the Empirical Implications of Theoretical Models (EITM) initiative in Political Science is exemplary in this regard. Further, we add that many scholars of scientific practice are now adding a third complement to the tradition of theory and empirical work: computational approaches. This is one of the tenets at the heart of the NSF-wide focus on Cyberinfrastructure. There is ample evidence that computational approaches are already altering the character and quality of research in SES fields, sometimes in fundamental ways. This trend will continue, and probably accelerate. We suggest adding computational approaches to the assessment of balance recommended here.

Theory, method and measurement. The long-term importance of the complements – theory, empirical work, computational approaches – to the welfare of the SES fields cannot be overstated. In addition to the balancing noted above, it is important to build and sustain focused efforts aimed at creating, assessing, and promulgating theory, method and measurement capability in the fields. The Methodology, Measurement and Statistics (MMS) Program focuses appropriately on understanding, developing and enriching the methodological infrastructure relied on by all of the other programs in the division and

beyond. As a secondary funder of many proposals, MMS has a unique perspective on the research and the methodological needs of researchers in SES. While retaining the current concentration on methodological development, MMS is well-positioned to assist in planning ways to stimulate and integrate theory and method in all SES fields. This expanded role would necessarily require additional resources for MMS. We do not suggest that this planning effort be reserved for MMS. The MMS program might be a focal point of such work, but the overall mission should be present in all SES programs. Moreover, this challenge should not be limited to SES. The SES Division has the potential (and, we would argue, the duty) to take on this larger role for the SBE Directorate, and where appropriate, for NSF and other research funding agencies.

Oral tradition. No one can question the vital role of the letters in the advancement of science. The ability to publish scientific findings broadly has been a hallmark of science since the founding of The Royal Society and the creation of the first scientific journals in the middle of the 17th Century. It is increasingly clear, however, that an exclusive focus on text-based communication overlooks a vital role of oral tradition in the conduct of science at all levels. The challenge is to develop mechanisms for transforming lore into written record, both within core disciplinary programs (and here permanent NSF program officers may be especially valuable) and more generally in the Science and Society program.

Training the next generation. The SES fields have a long tradition of doctoral education and consequent "on the job" development of researcher talent at the post-doctoral level. However, we believe that there is much that can and should be done to strengthen this in light of the opportunities and challenges facing the SES fields. Different commentators will bring different perspectives to this issue; from our perspective, we see two specific needs. One is for stronger preparation in quantitative skills across the SES fields, starting with mathematics and extending into the wide array of quantitative techniques that have become so important to advancing these sciences. This is important even without the rising significance of computational approaches to research, but clearly computational approaches are very dependent on strong quantitative skills. The other need is for more post-doctoral education of the sort common in the natural sciences and engineering. Such appointments give young researchers the opportunity to work closely with established researchers in ongoing research programs, with fewer distractions of teaching and service than common to assistant professorships. This change will obviously require considerable time and effort to accomplish, given that few of the SES fields have anything like this tradition at present. Nevertheless, we believe it is an important goal and effort toward it should begin.

SES In the Larger SBE and NSF Context

The SES Division is under-leveraged, both within the SBE Directorate and within the NSF itself. By that we mean that the SES fields have a great deal to contribute to the welfare of the nation, but much of that potential goes untapped. An interesting example of this arose in some of the program COV discussions, where it was noted that the American Competitiveness Initiative does not make much sense without the inclusion of

economics, sociology and other sociobehavioral sciences, yet these are conspicuously absent from the scientific research aspects of the ACI as currently articulated. The SES Division contains many areas of expertise that are important to the national welfare. Some of these opportunities are represented by multi-disciplinary, cross-cutting programs found within NSF; many others can be imagined without difficulty. How did this under-leveraged condition arise, and why does it persist?

There are undoubtedly many explanations for the current situation, starting with the most consistent point made among the program COV teams in this and in previous years: insufficient funding for SES programs. This argument is simple: insufficient funding makes it impossible to cover what each field thinks vital for its own interests, so broader interests are out of reach altogether. Every program review concludes with the plea for more funding. This is true not only in SES, but throughout the NSF. When every program asks for more funding on the grounds that good research is going unfunded, the net effect is all noise and no signal. Only by doing work that serves the broader interest can additional funding to build the core fields be obtained over the long run. The most significant increases in NSF funding in recent years have come through mobilization of the sciences – usually multiple fields of the sciences – around concerns of national importance. Eventually, the funding for these cross-cutting programs goes back into the directorates and divisions, to be allocated to programs. This builds strengths in the core programs. If done right, it also should build strengths within the core programs to work across boundaries and mobilize to address other issues of national concern.

The SES fields have a great deal to offer across many areas of national need. This fact alone creates great opportunities. Exploiting those opportunities requires a sophisticated strategy that, while building on the old notion of basic vs. applied research, goes beyond it to build a new and rich understanding of the interdependency of fundamental and practical knowledge. As progress is made in defining opportunities and strategizing to seek additional funding by addressing problems of national significance, equivalent strategic effort must be put into using new resources to build competence and knowledge in the core disciplines. With one must come the other.

There are many chicken-and-egg problems in the course we recommend. It is hard to know exactly where to begin, and how to bootstrap the processes required to get the whole effort going. We again make note of the fact that SES itself has a remarkable wealth of talent to aid in this challenging endeavor. Within the SES programs lie expertise in economics, social organization, politics, law, innovation, methodology, philosophy, and many other fields related to the challenges. The challenges articulated here are daunting, but they are not beyond the reach of the fields represented in SES.

Response to the 2007 Report by the Committee of Visitors

Decision, Risk and Management Sciences Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The members of the 2007 Committee of Visitors (COV) for the Decision, Risk and Management Sciences Program – Valerie Reyna, Cornell University (chair); Vicki Bier, University of Wisconsin, and Michael Dougherty, University of Maryland – met at the National Science Foundation March 8-10, 2007 to review the integrity and efficiency of the program's management practices and to assess and advise on all matters, including the program's portfolio of investments.

This response document provides a verbatim quotation of the COV's recommendation, followed by our response.

Recommendation

As an aside, multiple ratings from a single reviewer (e.g., Excellent/Very Good) are classified as "R" in the review record, making this document more difficult to use than it might have been. We would recommend revising the review record so that it can accommodate multiple ratings.

Agree

NSF does not compel reviewers to assign a single rating, and reviewers use multiple ratings to create an intermediate category, to reflect a contingent evaluation, or to express different views of different parts of a proposal. Changing reviewer instructions and options would require action at the level of the Foundation as a whole, so the program will convey this comment to those responsible for oversight of review procedures.

Recommendation

The review criteria as currently implemented are too vague, without the addition of the information in footnotes 7 and 8 (as cited in the DRMS report to the COV). We propose adding those questions back into the review form.

In addition, we propose adding the following check-off box (yes/no) to the review form: Does the proposed research program include undergraduates or graduate students in the conduct of research (i.e., not solely as research subjects)?

Educational research is a fundamental mission of EHR. We propose that NSF explore educational funding of teaching workshops and other educational initiatives involving DRMS scientists by drawing on EHR resources. DRMS research offers an opportunity to attract women and minority students into rigorous scientific research careers. For example, research on racial prejudice, environmental quality, and other social real-world social problems are attractive to many young students. Targeting promising young students through EHR funded initiatives would help establish the next generation of scientists.

Partially Agree

NSF supports fundamental research that leads to the betterment of society as a whole, and NSF regularly offers evidence of this through the budget process, "highlights" reports, and other government accountability measures. Criterion 2 of the NSF merit review process, which asks PIs to indicate the broader impacts of their research, alerts researchers to this aspect of the Foundation's mission, compels them to address such matters in the proposal, and asks reviewers to evaluate the proposed plans. Education, outreach, and other such activities may be among the broader impacts of a proposed research project, but are not mandated. Changing any of the matters of concern in this passage of the COV report would require changes in NSF policy and in the policy environment in which the Foundation operates. We will discuss the COV's concerns in appropriate venues.

Recommendation

The statistics in the Report to COV were extremely helpful in documenting and justifying the geographical and institutional diversity of awards. It is clear that awards reflect applications, and representation of particular states or institutions fairly reflects the quality of applications. It would be desirable to increase the quality of applications from different geographical areas and from non-traditional institutions, but quality should remain paramount.

Agree

DRMS and other NSF programs address such matters through various sorts of outreach. Funds for such activities are limited, but the program will continue its efforts in these regards.

Recommendation

Membership on the panel is one-time only and very short (only two years), providing an undue constraint on who can be asked to be on the panel. Quality of panel members is high overall, but this is due to Herculean efforts by the program directors. In the interest of maintaining quality panelists, DRMS staff should consider lengthening

terms to three years, allow people to serve two or more non-contiguous terms, and/or allowing for terms of different lengths.

Mostly Disagree

We have limited panel membership to 2 years to balance experience with change. Also, we recognize the huge time commitment panel service entails, although there are many panelists we would have been delighted to have had serve on the panel longer or to return to serve a second time. That said, maintaining the current procedures as the rule and deviating only occasionally has the virtue of assuring that the panel remains fresh and avoids even the appearance that the program has been "captured" by certain theoretical views or schools of thought. In light of the diversity of DRMS proposals, there are many senior scholars in many fields qualified to serve on the panel. Still, faced with short-term emergencies, we have asked former panelists to return for one competition.

Recommendation

High quality (ideally permanent rather than rotating) program directors are essential in achieving these impressive results. We would encourage the program directors to adhere somewhat less strictly to a numerical quota for panelists in particular areas. It would be desirable to increase the quality of applications from different geographical areas and from non-traditional institutions, but quality should remain paramount.

Agree

There are no numerical quotas. Still, we agree with the COV that quality must remain paramount.

Recommendation

Decrease the number of young reviewers. They are likely to have less time to review proposals and lack relevant experience. A few such qualified reviewers are appropriate to give them experience in preparing their own proposals in the future. As the pendulum has swung away nationally from quality considerations for reviewers and panelists (focusing on a number of important, but less crucial characteristics), it becomes ever more important to prioritize expertise as a criterion for selecting reviewers and panelists.

Disagree

In our collective experience, it is our more senior reviewers who are overwhelmed with review requests. Moreover, while some of the reviews we receive from junior faculty are poor, many are extensive and thoughtful. Finally, the reviewing experience is a good way

to educate newer scholars about the DRMS program. Given that we are successful at obtaining sufficient numbers of external reviews on proposals, any small risk associated with including some (though certainly not all) junior people in the review process strikes us as minimal.

Recommendation

Latent Semantic Analysis can be used to select reviewers within DRMS, and identify reviewers outside of DRMS whose expertise is relevant to DRMS proposals. A sample reference can be found below. Dumais, S. T. and Nielsen, J. (1992), "Automating the assignment of submitted manuscripts to reviewers." In N. Belkin, P. Ingwersen, and A. M. Pejtersen (Eds.), SIGIR'92: Proceedings of the 15th Annual International ACM SIGIR Conference on Research and Development in Information Retrieval. ACM Press, pp.233-244. --- using LSI to match reviewers and papers

Agree

This is an interesting idea we intend to explore.

Are awards appropriate in size and duration for the scope of the projects?

No, in general. The problem is particularly severe for highly interdisciplinary work; it is difficult to recruit a group of top-notch researchers from a variety of fields and get them to commit substantial time to a joint project for the size of awards typically available within the program. Consideration should be given to the duration of awards, because three years or less is insufficient to reap the full benefits of an investment. Specifically, startup activities typically occupy much of the first year, and the search for new funding must begin in the third year, leaving less time for active scholarship. Therefore, we recommend that awards be lengthened to four or five years, at least for senior researchers with strong proposals and established track records.

Agree and Disagree

We agree with that size of awards should increase as highly interdisciplinary work is often expensive. We also agree with the COV that "consideration should be given to the duration of awards," but disagree with any sort of blanket extension of awards. The primary reason DRMS funds few proposals for more than 3 years is that PI's rarely request more than 3 years of funding. When we have received proposals that request more than 3 years, in most cases reviewers and panels are reluctant to recommend funding beyond 3 years because the success of work in the later years is so dependent on results from the earlier years of the project. If the work from the early years is panning out so that additional work is warranted, the writing of a new proposal should not be onerous.

Recommendation

 DRMS contributes substantially to proposals submitted to other programs. However, those programs contribute substantially less to DRMS proposals (see pages 14-15 of the report to the committee). We would applaud efforts by NSF leadership to redress this imbalance.

Partially Agree

This is not a matter for NSF leadership to redress, but reflects agreements among freely-acting program officers. We were somewhat surprised when we saw how much of our program funds we exported to other programs (e.g., Economics, Social Psychology) in comparison with the paucity of funds imported from those programs to support grants to proposals with DRMS as the lead program. As DRMS is an interdisciplinary program, however, it seems to us that it is probably a sensible outcome – traditional disciplines may be more inclined to fund risky, integrative, or potentially transformative research when there is material participation (through review and funding) by DRMS.

Recommendation

Does the program portfolio have an appropriate balance of:
    Funding for centers, groups and awards to individuals?

    The program portfolio is heavily weighted towards awards to individuals, with a small but significant number of collaborative research awards to groups that are spread across multiple institutions. This is appropriate given the limited budget of the program.

    However, some types of work can only be done by larger centers. Increasing the DRMS budget to accommodate more group and center proposals would offer the opportunity to increase high-quality, high-relevance interdisciplinary research. NSF-wide special initiatives would also be another mechanism through which groups of decision-making researchers could make valuable contributions.

Agree

For 3 years NSF has been supporting three centers funded under the Decision Making Under Uncertainty for Climate Change competition. Site visit teams recently concluded that all three centers were successful in fostering interdisciplinary work.

Recommendation

Does the program portfolio have an appropriate balance of:
      Awards to new investigators?

Comments: Based on the data presented on pages 18-19 of the report to the committee, grants to new investigators appear to make up roughly one third of the overall program awards. This seems if anything high relative to the population of researchers in the field; this is a valuable but high-risk investment in the future.

NSF also has the flexibility to give special consideration for small awards to new investigators who may not yet be ready to write a competitive proposal for a full-fledged, multi-year award.

Agree

We agree that the DRMS program is supporting new investigators and that this is a valuable investment in the future. NSF does grant programs the flexibility to reduce a multi-year award into a smaller award if warranted.

Recommendation

Does the program portfolio have an appropriate balance of:

Projects that integrate research and education?

Comments: Training of future scientists should be considered an essential educational mission. In this area, DRMS is doing well.

In the opinion of this particular committee, it is inappropriate to expect that research grants will in general integrate extensive additional involvement in education. Educational activities are obviously meritorious, but should be funded in their own right.

Agree

"Broader impacts" is one of NSF's two funding criteria and includes "education" as one of its five components. Some projects lend themselves readily to extensive additional involvement in education, but other do not. We agree with the COV that research grants in general need not integrate extensive additional involvement in education.

Recommendation

Does the program portfolio have an appropriate balance:

Across disciplines and subdisciplines of the activity and of emergency opportunities?

Comments: The 2004 Committee of Visitors stated that: "A large number of funded proposals concentrate on controlled experiments with well-specified hypotheses. In the sample folders that we looked at, controlled experiments were funded at a higher rate than other methods such as interviews, direct observation, content analysis of documents, surveys, or econometric analyses of archival data. It may be desirable to take more risks with non-experimental projects, and to ensure representation on the panel and in the reviewer pool of respected researchers with a broader set of approaches."

While an appropriate balance across disciplines and methodologies is obviously a good idea, methodological rigor should not be compromised. Therefore, the 2007 Committee strongly takes issue with the statements about methodology in the 2004 report.

Agree and Disagree

We agree that methodological rigor should not be compromised, but disagree with the implication that controlled experiments are more sound methodologically than work that employs other methods.

Recommendation

Does the program portfolio have appropriate participation of underrepresented groups?

Comments: The representation of awards overall is commensurate with the pool of applicants. The representation of women as principal investigators or co-principal investigators is similar to NSF-wide statistics, which is about 30%. Minorities submit very few applications to this program (only 7% on average); however, the funding rate does not differ from that of other proposals.

NSF leadership should explore additional opportunities to increase the pool of rigorously (e.g., quantitatively) trained minority investigators. Graduate research fellowships and minority post-doctoral fellowships require candidates to initiate the proposal at relatively early stages of their careers. Supplementary funding on existing grants would provide a mechanism for PIs to add minority graduate students, postdoctoral fellows, and collaborators more efficiently and perhaps at less cost.

Agree

Broadening participation in all STEM fields is a Foundation-wide concern, and DRMS and other programs in SES will be energetic in their efforts to do so, employing mechanisms currently available and developing new ones. Moreover, there is interest at NSF in expanding the research foundations of broadening participation.

Recommendation

Is the program relevant to national priorities, agency mission, relevant fields and other customer needs? Include citations of relevant external reports.

Comments: DRMS research is of core relevance to issues of national security (such as terrorism), environmental quality, economic competitiveness, medical decision making, and other major national priorities. In fact, DRMS research is of high scientific importance as well as practical relevance. Because of the latter, DRMS should be a high priority for projects that translate the fruits of research into practical applications. Creative thought should be given to ways of marketing DRMS research results to the various practitioner communities and mission agencies. The Society for Judgment and Decision Making has endorsed this position, as exemplified in **Psychological Science in the Public Interest** and **Rising above the Gathering Storm** (about the importance of science to economic prosperity).

There are a number of agencies disseminating research funds that do not use adequate peer review. DRMS should explore opportunities to undertake the peer-review process on behalf of these agencies through memos of understanding.

Partially Agree

We agree wholeheartedly that DRMS funded research is of significant practical relevance as well as scientific importance. We also believe that program directors have the ability to synthesize and disseminate the results of DRMS research to practitioner communities and mission agencies, and would welcome the opportunity to do more of this. The synthesis of research and tailoring findings to specific communities, however, generally require significant commitments of time that the program directors simply do not have available without curtailing their current programmatic and extra-programmatic responsibilities.

We also agree that assisting other government agencies in funding research is an excellent idea, one the program directors' expertise could enable and one we have pursued to some extent in the past (e.g., the Decision Making and Valuation for Environmental Policy solicitations with the US Environmental Protection Agency). Currently DRMS is working with the National Oceanic and Atmospheric Administration in implementing a joint solicitation that would also involve other directorates at NSF. Once again, however, as a practical matter, severe time constraints limit the ability of DRMS program staff to assist with the administrative needs of other federal agencies.

Recommendation

Responsiveness of the program to emerging research and education opportunities.

Comments: DRMS has frequently been at the forefront of emerging research trends. As noted above, this should not come of expense of methodological rigor.

One interesting suggestion is to use data mining techniques such as latent semantic analysis and topics analysis to identify emerging trends in research before the researchers themselves may become aware of them. This would help the program officers characterize the nature of what is being funded, identify intersections between fields, stimulate new areas of collaboration, and advocate more effectively for new funding for emerging areas.

Agree

DRMS program directors are in a unique position to detect emerging trends in research to the extent that they frequently see research projects well before the ideas driving those projects are widely disseminated in the academic literature. Similarly, we are in a position to identify trends that are occurring across disciplines. Historically, these connections have been made on an informal basis in the course of program directors conducting their jobs and as a by-product of the panel meetings. The data mining technique of latent semantic analysis represents an exciting formal approach to identifying these trends and one we intend to explore.

Response to the 2007 Report by the Committee of Visitors

Economics Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The Committee of Visitors (COV) – Drs. John Haltiwanger (University of Maryland), Kenneth Rogoff (Harvard University), Richard Portes (London Business School) and Luis Fernandez (Oberlin) – convened on March 8-10, 2007 to review the Economics Program. Cecilia Conrad (Pomona) represented the SBE Advisory Panel for the Economic, Decision and Management Sciences COV cluster. The Economics Program Directors – Daniel Newlon, George von Furstenberg and Vincy Fon – joined the COV as requested for portions of the meeting. The COV addressed two sets of topics: (1) oversight concerning the management of the Program and (2) evaluation of the outputs and outcomes of NSF investments in the Economics Program.

Summary Observations: The COV concluded that the "Economics Program at the NSF continues to do a superb job in helping stimulate important new research in economics. The NSF continues to play a core role in supporting economics research that is transforming both the discipline and society. The excellence and professionalism of the NSF Economics Program staff is extraordinary, the program is in good health from this perspective."

Recommendation

"The main problems in Economics stem from a dismaying long-term trend decline in inflation-adjusted funding, a fact that has already been underscored by the previous two COVs in 2001 and 2004. As a result, the Economics Program has had to turn down very promising proposals. The success rate is now hovering around 20%, well below long-term averages despite declining numbers of proposals (decline of around 10%). The success rate has recovered some from a few years ago but it is not clear this represents an improvement given the decline in the number of proposals and the continuing problems with salary caps and RA amount caps in awards. An open question is whether the low success rate as well as low award amounts have caused PIs to be discouraged from submitting proposals to NSF.
 The 2004 COV sought to remedy this situation by recommending a separate division for Economics within the NSF. That idea was rejected by the NSF, but as far as we can tell, no alternative approach was adopted to remedy the core problem. Given the influence of basic economic research in universities, in policymaking, as well as in finance and business, it is puzzling indeed that the size of the NSF program in economics is so minuscule, $20 million dollars per year."

PROGRAM STAFF PARTIALLY AGREES:

All fields of science in the Division have made similar claims about budgets and rightly so, but program staff and division leadership see deep disadvantages in arguing for the budget of one program versus another. Given the rich intellectual and social connections among fields of science, strategies that increase all budgets within the division and directorate are indicated. To that end program officers and division leadership will continue to pursue strategies that increase funding for interdisciplinary initiatives involving our fields and that increase the base budgets of programs.

Recommendation
"The fact that there are no economists in the senior management of NSF has implications not only for how the current NSF 'pie' is divided, but also for how the NSF advocates for economic research with Congress. Advances in economics, many funded over the years by the NSF, have played a huge role in shaping public policy and innovation in today's era of globalization. US dominance in financial globalization, for example, builds integrally on innovative Nobel Prize winning work on options pricing ('rocket-science finance'), the core of which was funded over the years by the NSF. We acknowledge that some of the blame lies with economists for not advocating and lobbying in Congress as other disciplines do, but NSF structure must also be partly to blame.
A striking case in point is the NSF's recent competitiveness initiative. It is very troubling to the COV that the competitiveness initiative does not include input from economics whatsoever. 'Competitiveness' is fundamentally an economic concept. Over the years, countries that proceeded with 'competitiveness initiatives' without the checks and balances of proper economic analysis have wasted tens of billions of dollars. Economists who study international trade, industrial organization, and productivity growth have developed key insights on how to think about these problems, what works, and what doesn't. Parenthetically, among all of America's industries, financial services is perhaps the largest, most competitive and innovative, and produces the most income. Despite growing challenges from Europe and elsewhere, this will probably continue to be the case for years to come. NSF-funded economics research has long played a role in keeping the US on the forefront of this industry. So from many angles, having a 'competitiveness' program that excludes economics is simply absurd."

PARTIALLY AGREE:
 The disciplinary affiliations of NSF's upper management have at best a modest influence on program budgets and initiatives. The current director and deputy director of the Foundation are an engineer and a biologist, respectively, yet proposal success rates are relatively low in the engineering directorate, the biology directorate received a modest budget increase in FY2008, and the mathematics and physical sciences budget increased substantially**.** The management of government-wide initiatives such as the $5.9 billion American Competitiveness Initiative is beyond the scope of this COV report, and outside the authority if NSF—these are matters for the President and the Congress – but, we agree it would be helpful to have social scientists engaged earlier and at a higher level in major research initiatives. like this one. Economists were not entirely excluded from the national competitiveness initiative, and were substantially involved in the NSF part of it. Economists participated in the workshops that shaped the prospectus for the $10 million

Science of Science and Innovation Policy (SciSIP) solicitation and SciSIP is headed by a former Economics Program Director.   Economists also participate in other relevant initiatives and special solicitations such as Cyberinfrastructure and Human and Social Dynamics. We urge engaging social and behavioral scientists at earlier stages and higher levels in all initiatives, both within and outside NSF, when their involvement would enhance depth, breadth and public impact.

Recommendation

 "A final point we would like to emphasize deals with the COV process itself. The funding for the core Economics program, which we have been asked to review, has been relatively flat for many years. We understand that much of the marginal increase in funding for economists has come through inter-disciplinary initiatives such as Human and Social Dynamics.   Yet, the COV process does not permit any comparison of the marginal project rejected by Economics, and the marginal project funded by one of the initiatives. We do not have the data or the files on the initiatives to make any comparisons. We can only point out that most of the transformative and key ideas that have come out of the Economics profession in recent decades have come through the core Economics Program of the NSF. We suspect this will be true in the future, although we are prepared to be convinced otherwise. Certainly, the COV process should be designed so that fields can give judgments on this issue. Thus, we recommend that future COVs have access to decisions on economics proposals across all of NSF."

PARTIALLY AGREE:

As currently structured the COV process is concerned with the workings and performance of programs, not disciplines, so the Human and Social Dynamics Program is beyond the purview of this COV.   The COV process is currently being re-evaluated by NSF and might be changed in the future in this and other ways. As for the origins of transformative ideas, since such ideas are themselves hard to recognize, particularly in the short term, it is difficult to evaluate assertions about where they originate and how the may be induced.

Recommendation
"Some concern is raised by the fall in the mail review "hit rate" (proportion of requested reviews that are completed). We recommend that the Program Directors should seek to identify the causes and consequences of this decline."

PARTIALLY AGREE:

 The concerns of the COV about the decline in the hit rate in FY2006 are premature. The hit rates in the preceding two years (75% in FY2004 and 87% in FY2005) were extraordinarily high and reflect successful efforts by the Economics Program staff to encourage reviewers to answer review requests.   The hit rate declined in FY2006

because the Economics Program staff stopped pursuing overdue review requests for proposals that had a sufficient number of mail reviews. But the quality and number of reviews received are more important to the evaluation process than the hit rate and the COV found the quality and number of reviews exceptionally good. Program staff still will monitor the mail review hit rate to make sure that it does not continue to decline and if it does, the economics community will be alerted to this problem.

Response to the 2007 Report by the Committee of Visitors

Innovation and Organizational Change Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The members of the 2007 Committee of Visitors for the Innovation and Organizational Change (IOC) program -- Professors Linda Argote, Carnegie Mellon University (Chair), Andrew King, Dartmouth University and Alan Meyer, University of Oregon – met at the National Science Foundation on 8-10 March 2007 to review the integrity and efficiency of the program's management and to assess and advise with regard to the program's portfolio of investments.

With regard to integrity and efficiency, the committee concluded that, overall, the program is being managed effectively. Their major comments and IOC's responses on these topics are:

Recommendation

A very positive change relative to the last COV report is the increased use of external reviewers. We applaud the program officer's success in implementing this recommendation. … Increased use of ad hoc reviewers improved the degree to which the review process targeted field experts and enhanced the quality of information available to the program officer.

> **Agree.** The IOC program director has made it a priority to increase the number of expert mail reviews per proposal. This not only improves the quality of information available to the panel and program officer for decision-making purposes, it also familiarizes more members of our research community, which has not traditionally been grants oriented, with NSF and the grant-making process.

Recommendation

We did note variability in the quality of the reviews. We also observed that some of the reviewers failed to actually provide a rating, or provided two ratings. As a result, their

ratings were marked as "R" in the data provided to COV members. We suggest that instructions to reviewers include more explicit directions.

> **Agree.** Consistently high quality reviews are an important goal for IOC. Each proposal needs to receive high quality assessment and each PI needs to receive high quality feedback. The program director will reassess IOC's reviewer solicitation letter and devise improvements to try to help ensure more consistently high quality reviews and more consistent tendency to provide ratings.

> The 'R' ratings that concern the COV occur when a reviewer chooses to rate a proposal as falling between Excellent and Very Good, or between Good and Fair, for instance, rather than as falling solidly within one category. They may also reflect contingencies in the reviewer's judgment or different evaluations of different parts of the proposal. Since reviewers and panelists consider it valuable to be able to send this sort of signal, and since "R" ratings occur in programs across the Foundation, the program officer will inquire whether our computer system might be adjusted to better capture these for reporting purposes.

Recommendation

The panel summaries provide neat encapsulation of the panel's analysis. In cases where opinions were mixed, the program officer provided more detailed information on the opinions and clarified about how differences were resolved. … One possible weakness is that for some summaries of winning proposals' evaluation were relatively brief.

> **Agree.** All of SES has made it a priority to ensure that PIs can understand, from their panel summaries, the issues that were of greatest importance to the panel. This is just as important for proposals that are funded as for proposals that are declined. As a PI proceeds with a research project, he or she should be attentive to the suggestions in the reviews and panel discussion that can strengthen the work. The program officer will remind panelists of this and will pay additional attention to ensure that panel summaries for funded proposals are as helpful as those for declined ones.

Recommendation

In 2006, no review took longer than 9 months, although the number of quick reviews (< 6 months) decreased.

> **Agree.** In 2006, the program officer had a large number of reviews that missed the six month deadline by a few days. It is the program director's intent that IOC should somewhat exceed NSF's internal 70% 6-month dwell time goal. More care will be taken to ensure this in future. IOC's program director has an additional

practice, though, that is pertinent to concerns about timely feedback. The program director informally notifies almost all PIs informally (via e-mail) of the program's recommendation within a week or two of the panel meeting (i.e., 3-4 months after submission), so they can make plans for their research programs as soon as possible. Our PIs are not kept hanging while we complete our internal paperwork and concurrence processes.

.

The committee was generally pleased with the quality of the research funded by IOC and with the role it has played in advancing important research streams. Their major comments and IOC responses on these topics are:

Recommendation

The quality of the work being funded by IOC is very high. It includes some of the best current research relating to organizations and innovation.

> **Agree.** Although IOC-funded research is of high caliber, in comparison with other programs we could be receiving a higher proportion of fundable proposals. Improving the quality of overall submissions is the highest priority for the program director. Significant outreach efforts targeted to achieve this goal have been made this year and will continue in the next several years. In addition, the program director is developing web-based materials to help researchers who are unfamiliar with the grants process to present their research ideas more effectively.

Recommendation

As the only NSF program that is targeted at understanding organization-level phenomena, IOC should be receiving proposals from a larger share of the most eminent senior scholars and the most promising young scholars. We recommend an outreach campaign to communicate with prospective grant applicants that might include:

- Promoting IOC to leaders of selected subunits of professional associations …
- Sending RFPs to editors and editorial board members of journals that publish research on organization and innovation…
- Continuing to fund small conferences…
- … encourage the program's grant recipients to evangelize on behalf of the program.
- Consider supporting doctoral student workshops at conferences, and providing seed funding for dissertation research.

Almost entirely agree. These all are helpful ideas. The actions recommended under the first, third and fourth bullet points have already begun.

With regard to the second, the program director appreciates this advice and will send materials to editors and editorial board members.

The committee's final bullet point has two parts, which we address separately. First, we will give thought to what kinds of graduate student workshops might be encouraged. On the presumption that graduate students interacting with faculty holds more promise for advancing the field than graduate students interacting with each other, IOC has typically used workshop funds to enable graduate students to interact at a high level with faculty rather than in graduate student venues in which they interact principally with each other. That said, there may be some types of student-focused workshops (e.g., workshops on emerging methods) IOC should encourage and fund. The program director will discuss this with the panel. Second, with regard to doctoral dissertation funding: SBE has a category of grant called a "doctoral dissertation improvement grant" that IOC has not previously offered. Given your assessment, we will discuss this with the panel and, most likely, take steps to add this category of grant to IOC's portfolio.

Recommendation

IOC's domain is inherently multidisciplinary, and attracts proposals grounded in a broad range of social and behavioral science disciplines. The PD's practice of negotiating co-sponsorships with other NSF programs is a good practice that magnifies the impact of IOC's limited budget.

Agree. The program director will continue to seek co-review and co-funding opportunities for IOC and will remain receptive to these opportunities when suggested by others.

Recommendation

The program has an excellent track record of funding proposals from women investigators. We did not see grants to minority investigators (other than Asian), but this may be caused by the tendency of investigators to decline to provide information concerning group affiliation.

Agree. NSF is serious in its commitment to increasing involvement by scientists from underrepresented minority groups, but the inability to measure our progress

because of data problems is frustrating. The program officer is taking steps to ensure that we are seen as a welcoming institution to minority scientists. In addition to conducting outreach in venues that include researchers from underrepresented groups, the program officer sends all notices about pertinent new competitions and funding opportunities to the Diversity listservs of the relevant professional societies, not just to the topical listservs. Also, the program director seeks out minority researchers to serve as reviewers, in order to enhance our visibility with them, to enhance their familiarity with the grants process and to ensure that they find us approachable and consider us a potential funding source.

Recommendation

In terms of its internal practices, we encourage the IOC to give a larger number of smaller awards to young scholars rather than funding a small number of young scholars with CAREER awards.

> **Partially agree.** IOC, like all NSF programs, is required to participate in the CAREER program and will continue to do so. CAREER has recently undergone a COV of its own, which may lead to some changes in the aims and operation of the program. However that turns out, the program officer will also conduct outreach to encourage more young scholars to submit regular grants to advance their research agendas and careers. The IOC panel would most likely be enthusiastic to receive more such proposals, much as they are enthusiastic about RUIs and about proposals that support graduate students.

Recommendation

Investing in conferences has a very high return, so we encourage devoting more funds to conferences.

> **Agree.** IOC has been funding, on average, one conference per year. These are conferences that aim to advance the research agenda of some important, promising portion of the community. IOC funds are typically used to enable graduate students and junior faculty to participate. IOC requires that any conference it supports be open to many scholars, that conference materials are widely available and that the participants would not have some other natural opportunity to meet. Since funding for conferences is typically modest, the program can certainly accommodate one or two more per year without compromising its research portfolio. We are not as yet receiving this number of high quality workshop proposals, but we will be open to them.

Recommendation

The IOC program is exceptionally relevant to national priorities and NSF goals. The program focuses on innovation, which is essential for American competitiveness…. The IOC program is also central to other NSF initiatives, including the Science of Science and Innovation Policy, and the Dynamics of Coupled Natural and Human Systems. The cross-disciplinary, problem-solving orientation of the IOC program positions it to help solve many of the most important challenges of our time, including innovation, sustainability, the implementation and diffusion of new technologies, productivity, and organizational performance.

> **Agree.** IOC will remain active in these and all other NSF-wide activities that are appropriate for the program.

Response to the 2007 Report by the Committee of Visitors

Law and Social Sciences Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The members of the 2007 Committee of Visitors for the Law and Social Sciences Program – Professors Celesta Albonetti, University of Iowa, Steven Penrod, John Jay College of Criminal Justice, and, C. Neal Tate, Vanderbilt University – met at the National Science Foundation March 8-10, 2007 to review the integrity and efficiency of the program's management and to assess and advise with regard to the program's portfolio of investments. During their visit, the members of the committee produced a report of their findings. Below are responses to the major parts of the report.

The program director for NSF's Law and Social Sciences program is greatly pleased about the success of this COV. The director is especially delighted that COV members were unanimous in concluding that during the COV period, "the Program review process was efficient, fair and effective, and the program well managed" and that "the Program is meeting its obligations to promote sound science in the research it funds." The comments below are responses to the Committee's comments and recommendations and the actions that the program plans to take to address them.

Recommendation

With respect to one aspect of efficiency (A12), the COV noted that the 2003 COV reported that the time to decision (dwell time) for competitive proposals was averaging 4 months—that number has crept up in the period under review: from 5.2 months in 2004 to 5.8 months in 2006. The increase likely reflects, in part, the growing number of proposals that must be handled by the Program Officer—these totaled 142 in 2004 and 178 in 2006 (a 25% increase). The total number of proposals submitted during the period of 2004-2006 was 504 (478 competitive) as compared to 373 proposals in the 2001-2003 timeframe (a 35% increase). The lengthening dwell times and growth in numbers of proposals beg the question of whether the Program office is adequately staffed and whether dwell times might be reduced by providing the Program more support.

Agree

The program director agrees with the committee that there has been a significant increase in the number of proposals reviewed by the LSS program. It should be pointed out that the increase of 35% in proposal load between this COV and the 2004 COV does not reflect a large number of proposals co-reviewed with other SES and BCS programs. Generally speaking it would be expected that during the first year of a new program director's tenure, dwell time would suffer slightly as the officer learns the new job,

especially in a program without a permanent program officer to provide steady guidance. As the new program director gains in experience and understands how to efficiently process proposals for award and declination, and also appreciates the importance of dwell time to PIs and to the NSF as a whole, significant improvement in dwell time should be observed. The only spoiler for this rule is if there is continued increase in the number of proposals. While the program's dwell time experienced a slight increase (*from 5.2 months in 2004 to 5.8 months in 2006)*, there was indeed a comparatively greater increase in the proposal load. Furthermore, the nature of the research LSS funds frequently requires IRB approval and PIs often wait until they receive a positive response from LSS before beginning the IRB approval process. This can affect dwell time.

In addition, due to increases in the number of quality proposals being submitted, the program has had to request downward budget revisions in nearly every proposal funded. Addressing these requests take time and undoubtedly contributed to the slight increase in dwell time. But an increase in dwell time is an increase, nevertheless. Irrespective of any countervailing explanations, there is always room for improvement. The COV expressed concern about whether the program is being adequately staffed. Until recently, the answer to that concern would have been "No." Luckily, the program now obtains regular assistance from Kevin Gotham who splits his time across three programs in the SPS cluster. The program director has made some changes such as sending out mail reviews as soon as proposals come in and holding the advisory conference sooner in the cycle. These changes are designed to improve the program's dwell time.

Recommendation

 "The COV is concerned that substantial numbers of worthy proposals are not being funded but the COV does not have a basis for assessing whether this situation is consistent with the experience of other panels or whether the situation indicates that the LSS Program is not adequately funded."

Disagree

 The program recently instituted a panel rating system of "Must Fund" "Should Fund (+ or -)", "Could Fund" and "Do Not Fund." This rating system is more fine-grained and is designed to provide more information for the program director. The COV is referring here to proposals placed in the "could fund" category by the advisory panel. Because program funds are tight, the LSS program director acts judiciously in allocating program funds to the most worthy research proposals recommended by the LSS advisory panel. Often that decision comes down to the expected contribution that the research will make and the cost. In a perfect world were budgets are not a constraint, more innovative research would surely be funded. But program base budgets have remained essentially flat during this COV period, making it impossible to fund many truly worthy proposals. The program director applauds the COV for raising this concern. But the COV was incorrect in stating that LSS funding rate is 23%. The actual figure provided the COV is 29%, which is above the 23% funding rate for the entire NSF. Reaching this 29% funding

rate for the program requires that funds be allocated very judiciously, while making sure that worthy projects are not under-funded.

Recommendation

 "On the whole, reviewers shared agreement on the extent to which the proposed research met NSF's review criteria. In the few cases in which there was some disagreement, the panel summary explicitly dealt with the lack of consensus. We applaud the reviewers' conscientious implementation of NSF's merit review criteria."

Agree

 Mail reviewers are a valued part of NSF review process. Program officers recognize that reviewers are busy people and do occasionally provide inadequate input into the reviewers they write for NSF. What LSS program directors have done and will continue to do in future is to request additional information from reviewers in those few instances where the review submitted is exceedingly brief or fails to address NSF review criteria. This allows both the advisory panel and the program directors sufficient useful information on which to base their funding decisions.

Recommendation

 "The COV does think the program officer should make every effort to secure more than the minimum of three reviews for regular research proposals (though not necessarily for dissertation proposals) to guard against the possibility that a single idiosyncratic reviewer could unduly affect a proposal's fate by giving it an extreme rating at variance with other ratings. We encourage the panel and the program officer to be diligent to guard against this possibility."

Agree

During this COV, there was only one instance in which a recommendation was made on the basis of two written reviews. It was a dissertation improvement grant proposal and the program director, following approval from the division director, requested that the entire panel discuss the proposal as a substitute for a third review. The Law and Social Science program directors make every effort to garner more than the minimum three reviews required for a recommendation, especially for regular competitive proposals where substantial funds are involved.

Recommendation

The reviewers used were appropriately, indeed well, qualified to assess the proposals they were assigned. The difficulty of finding appropriate reviewers is perhaps especially great

for a program officer in a truly interdisciplinary program like Law and Social Science, however. The COV and the program officer discussed constructive ways in which the program officer can improve his ability to select the best possible reviewers and entice them to submit reviews. For example, the program officer might take steps to further encourage P.I.s to suggest reviewers and to solicit suggestions from prior awardees of "up and coming" scholars who would make reviewers.

Agree

The LSS program officer is grateful for the COV's excellent suggestion that PIs be encouraged to suggest, and to solicit suggestions from prior award recipients of, promising new scholars as potential reviewers. The program officer has been updating the reviewer list with new potential reviewers but the suggestion of a more active search for new potential reviewers to increase the available pool is a good one and the program director will act on it.

Recommendation

"The overall quality of the projects supported by the program is excellent."

Agree

The LSS program director is delighted that the COV reached this conclusion following their review LSS program portfolio.

Recommendation

"We did not see in our sample of proposals any that were remarkably innovative or high risk. However, it is clear that the program has funded such proposals in the past, and that it is open to doing so in the future. The COV also noted that by committing a portion of its scarce funds to the support of dissertation projects, the program is taking risks on the ability of our most junior researchers to produce good and, possibly, even innovative research."

Partially Agree

The program disagrees with the COV's statement that in the portfolio of sample proposals, there were none that were remarkably innovative or high risk. There were indeed several highly innovative proposals including Devah Pager's CAREER award on measuring bias (co-funded with the sociology program) and Larry Heuer's proposal on procedural justice to name only two. Others remarkably innovative proposals funded are highlighted by the COV in their discussion of outcome goals for discovery, learning, and infrastructure (section B). Thus, it is unclear what the COV's definition of "remarkably innovative" entails if the innovative proposals discussed in that section do not qualify. The program director does agree with the COV that the program makes a good effort to

support a large number of dissertation projects, which (by their very nature) involve high risk and a high potential for creativity and innovation.

Recommendation

 "Available data show that the program received 48 proposals from minority P.I.s during 2004-2006, about 9 percent of the total of all proposals. While it is certainly desirable that this number increase, this submission rate is not a bad result, given the continued underrepresentation of minority researchers in the program's community. Only 15% of these proposals received funding, a rate that is lower than the overall funding rate for competitive LSS proposal."

Agree

The program takes the issue of under-representation of minority groups in LSS funding seriously. Only 48 proposals were submitted by minority PIs and only 15% of these were funded during this COV period. The numbers could be improved and the program has made some effort to increase these numbers. One approach that the program director has taken to address this concern is to include a visit to one or more minority institutions in his outreach trips, and to effectively discuss the process and encourage proposal submission from scholars in these institutions. The evidence tends to indicate that this strategy is having a positive effect on the submission and success rates. For example, while there were no proposals with minority involvement submitted to LSS in 2005, this changed remarkably in 2006 where the program had a minority submission rate of 25%. The program is also making efforts to diversify the advisory panel to include well-qualified minority scholars as a way of increasing the degree of alternative points of view presented during panel discussions. While the advisory panel is currently well-balanced in terms of gender representation, it is not so in terms of race. Currently the panel has no minority scholar out of 10 panelists. This status will change.

Recommendation

 "These infrastructure projects are a unique construction by the Program and contribution to the knowledge and understanding of law and law-like institutions; no other nation has this wealth of systematically collected, valid, and reliable data available to citizens and scholars alike with which to explore the functioning and behavior of American and foreign courts. It is a unique achievement."

Agree

The program commends the COV for its recognition of LSS program's importance in building infrastructure for scientific research in law and the social sciences. One minor issue with these data infrastructure is the need to locate the data in one central depository.

In future, the program plans to encourage PIs on projects involving large data procurement to deposit their data at the Inter-University Consortium for Political and Social Political Research (ICPSR) at the University of Michigan. Every attempt to centralize the location of NSF-funded data will facilitate research and avoid duplicate funding of data projects.

Response to the 2007 Report by the Committee of Visitors

Methodology, Measurement, and Statistics Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The members of the 2007 Committee of Visitors (COV) for the Methodology, Measurement and Statistics (MMS) Program –Professors Charles F. Manski, Northwestern University, Trisha Van Zandt, Ohio State University and Dr. Nathaniel Schenker, National Center for Health Statistics - met at the National Science Foundation, March 8-10, 2007 to review the integrity and efficiency of the program's management practices and to assess and advise on all matters, including the program's portfolio of investments.

Recommendation:

The COV recommends that, although individual reviewers should be encouraged to consider both criteria in formulating their opinions, they should not be required to address the criteria in two separate fields. The two criteria are interrelated, and separating out the corresponding comments is difficult.

The COV recommends that the responsibility for addressing the two criteria separately during the review be given to the Program Director and the review panels.

Partially Agree: The Program agrees that the two criteria are interrelated and recognizes that separating out the corresponding comments can be difficult for reviewers. That said, as a matter of NSF policy program directors and review panels are required to address both criteria separately.

Recommendation:

The COV encourages the MMS program to continue to seek emerging opportunities. The following are three examples. (1) There is a pressing need for methodological research to enable more systematic interpretation of empirical findings emerging in "neuroeconomic" research on decision making and in neuroscience more generally. (2) It is highly important to develop better understanding of the problems and opportunities that arise in combining related data across data sets using different sampling and questionnaire designs. (3) There is a longstanding need to better integrate the "design-based" and "model-based" approaches to survey research.

More generally, the COV encourages MMS to continue seeking out research that promotes synergy between methodological research and substantive applications. On the one hand, application-driven research often encounters open methodological questions

that MMS-supported research may help to resolve. On the other hand, the usefulness and limitations of abstract methodological advances often become apparent only when serious applications are attempted.

Agree: The Program will continue to seek emerging opportunities and research that promote synergy between methodological research and substantive applications.

Recommendation:

As available data and information resources expand at an increasing rate, it is essential that NSF provide stronger financial support for the MMS program to ensure the development of critical quantitative and methodological building blocks for conducting scientific research.

Agree: The Program and SES division will continue to seek additional support for the development of analytical and statistical methods for the social, behavioral, and economic sciences.

Recommendation:

It is important that the research promoted by Mathematical Social and Behavioral Sciences (MSBS) continue to be supported, whether through the existing funding mechanism or through new funding provided to MMS.

Agree: The NSF units that participated in MSBS (SES, BCS, and DMS) recognize the importance of the MSBS activity and have discussed the potential for continued support. The Program will continue to pursue opportunities to continue this activity in some form.

Response to the 2007 Report by the Committee of Visitors

Political Science Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The members of the 2007 Committee of Visitors for the Political Science program –
Professors Kathleen Bawn, University of California, Los Angeles, John Freeman,
University of Minnesota, and Walter Mebane, Cornell University – met at the National
Science Foundation March 8-10, 2007 to review the integrity and efficiency of the
program's management and to assess and advise with regard to the program's portfolio of
investments. During their visit, the members of the committee produced a report of their
findings. Below are our reactions to the major parts of this report.

On Empirical Implications of Theoretical Models (EITM):

*It is curious that one group of reviews is convinced the impacts of the EITM project
already have been realized while another repeatedly calls for more rigorous assessment
of the curriculum and instruction. We side with the latter group; more could be done to
evaluate the institutes both in terms of student evaluations (which are quite unsystematic
now) and, more important, to demonstrate that the breakthroughs being achieved by
young scholars are the result of their having attended the institutes. In this regard, it is
important to show that the breakthroughs probably would not have been achieved if the
scholars had been unable to attend the EITM institutes.*

*Second, while efforts have been made to introduce some other kinds of theory and to
explore interdisciplinary enterprises of some kinds, the institutes have not so much
transformed as they have revitalized rationalize choice theory. In view of its importance
in political science, this is a very significant accomplishment.*

Response: PARTIALLY AGREE.   EITM (Empirical Implications of Theoretical
Models) started as a special funding opportunity in the Political Science Program that
focused on combining theoretical work with empirical tests. This competition resulted in
the funding of two summer institutes, several research projects and workshops, and a
small set of graduate training fellowships. The institutes mentioned in this section were
originally funded through this competition.

 We agree that the EITM institutes need to do a better job in terms of student evaluations
and evaluating the impact of the programs. Both institutes were pushed in these
directions through the use of the addendum mechanism. In addition, one institute is
being funded through a continuing grant increment in order to increase our level of
oversight.

We agree that these institutes have been important to the discipline of Political Science. However, we disagree with the COV's assessment of this impact. The institutes and EITM, in general, has transformed the manner in which a large number of political scientists undertake their work. The emphasis on melding theoretical and empirical work has changed the research of political scientists in ways other than the revitalization of rational choice theory. While some political scientists still do empirical work that is void of theory, rarely do we see theoretical work without some reference to empirical testing.

On the Institute of Qualitative Research Methodology:

The COV agrees that the development of scientifically rigorous qualitative methods is an important part of the program's mission. Political scientists definitely need a better understanding of research design, interview technique, etc. This said, we are not convinced that the Qualitative Methods Institute at ASU is providing this kind of training. As the original reviews of the proposal noted, there seems to be no coherent curriculum that is scientific in nature. The topics covered by this group cover everything from the history of the discipline to "interpretivist" thought. Certainly, these topics are of interest to scholars. But how exactly they contribute to scientific progress is not, as described in the jackets, clear. We believe these funds would be better spent on 5-6 high quality rigorous dissertation fellowships.

Response: PARTIALLY AGREE. Some reviews of the renewal proposal note the lack of specificity about the training and question the products of the institute's first sessions, and these matters need to be examined. The institute has been running for a long enough time that we need to evaluate the benefits to the discipline before considering additional funding. That said, we believe that NSF funding for qualitative methods training can be more valuable than the funding for 5-6 dissertation grants. In fact, involvement will insure that rigorous training in these methods will occur. We agree with COV members that NSF should oversee such an institute more actively, perhaps through a competition where investigators provide a more detailed curriculum.

On SGERs:

The one SGER proposal that raised some concern for us was a proposal (by Carol Swain) to conduct focus groups in Nashville, TN, bearing on immigration. The research set out by the proposal was to occur during a period when interest and mobilization about immigration was high, and the PI proposed to draw in a diverse range of participants (blacks, whites and Hispanics). But we were concerned that an empirical basis of focus groups all conducted in one city is worryingly thin. This small concern notwithstanding, we find the political science program's use of SGER awards to be highly effective.

Response: PARTLY AGREE. We certainly agree with the COV members that the program has funded high quality SGERs over this last period.

With regards to the SGER where Carol Swain was the principal investigator, we respectfully disagree with their concerns. We understand their concerns about this proposal, but believe it is based on an incomplete understanding of our funding decision. The Swain award complements a supplement Political Science provided to a national survey of Hispanics, which produced better understanding of the opinions of Hispanics about the ongoing immigration debate and resulting protests. The Swain SGER is a qualitative complement to a portfolio that already contained a large quantitative award on this subject.

On quality of the regular reviews:

We are favorably impressed by the review process, and the quality of reviews. In our sample of proposals, we did find cases in which individual reviews were too superficial to be helpful to the principal investigator, but such cases were not typical. The typical review shows a high level of engagement with the proposal. In addition to explaining the recommendation, a large number of reviews offer constructive criticism that would help the PI's research agenda. This latter point holds true for proposals not funded as well those that are.

Response: AGREE. We think that most reviewers provide a useful evaluation of proposals sent to them. Clearly, it is not possible to expect that all reviewers will provide a thorough evaluation of every proposal sent to them, even assuming the best intentions to do so. We think that it is significant, however, that we receive enough thorough reviews to give less weight to the few superficial reviews and make a well-considered decision about each proposal.

On quality of the dissertation reviews:

We note that reviews for doctoral dissertation fellowship [sic. Doctoral Dissertation Improvement Grant] proposals are often shorter and less detailed than for regular research proposals. Perhaps this is to be expected, given the preliminary nature of dissertation projects. But the graduate students who write these proposals would benefit from more detailed feedback.

Response: PARTIALLY AGREE. The dissertation reviews tend to be shorter because doctoral dissertation improvement grant proposals are shorter, simpler, and reviewed only by panelists. With panelists reviewing 20 or more proposals, we also cannot expect very detailed reviews of every proposal. We greatly appreciate the time that panelists already provide to us. While we could solicit ad hoc reviews for these proposals that would likely have the quality of the reviews of regular proposals, we believe that such an approach is inadvisable because of the strain it would put on our reviewer pool. We also think that one should keep in mind that the primary function of the review process is to provide program officers with the information to decide whether or not a proposal merits funding. In cases where the written reviews may indicate that proposals are worthy of consideration for funding, program officers generally do receive more detailed information with which to make decisions from the panelists' discussion of the proposal.

Finally, while it may provide a nice service to provide graduate students with greater feedback on their proposals, we accept that some tradeoffs have to occur in order for us to receive the highest quality advice in making our funding decisions.

On the reviewers' interpretation of broader impacts:

"Intellectual merit" is addressed in virtually all reviews, and in most cases, addressed with notable care and thoroughness. "Broader impact" is more problematic and more complicated. It is interpreted in a variety of ways, not all of which seem consistent with what we perceive to be the intention of this criterion.

Response: AGREE. The analysis and advice offered by the COV is very helpful to the program and, perhaps, to the entire Foundation. That said, we cannot alter NSF review procedures but can modify our instructions to reviewers. We believe that the community has a limited understanding of what NSF means by the broader impacts of a study, and the COV members make an excellent suggestion of one measure we can take to improve our community's understanding of this criterion.

On COIs:

NSF COI rules are notoriously strict. While we recognize the value of erring on the side of caution in this matter, we note that grants for infrastructure projects that involve large numbers of scholars (many in minor roles) made be disadvantaged by having a large number of qualified reviewers lost because of COI rules.

Response: PARTIALLY AGREE. NSF COI rules are established and implemented for the Foundation as a whole and cannot be altered by a program, so we will reply in general terms to the substance of the COV's observation. COIs can certainly be a problem when trying to find the best reviewers for some large-scale proposals, but we do think these cases are more the exception than the rule. Generally speaking, conflict of interest problems do not put program directors in the position of being unable to seek advice from well-qualified reviewers. Given the public nature of the NSF's funding, we believe that it is important that our review process remains above reproach. In such instances where many qualified people are unable to review proposals because of conflicts of interest, we believe that NSF's policy of giving program directors flexibility to make recommendations that may go against the received reviews provides an important, though perhaps not perfect, counterweight to these problems. Once again, this is an area where we believe that the system allows program directors to use good judgment in the face of inevitable tradeoffs.

On CAREER awards:

Related to the topic of new investigators is the low frequency of CAREER awards – only one during our review period – and the sentiment expressed in the "Program Information" section that Political Science should try to award more of them. Our reading of the CAREER proposals in our sample confirms the Program Directors'

judgment that the proposals received in this category are not of particularly high quality – not at the level of quality that would merit funding for a normal research proposal. The Program Directors conjecture that the low success rate in Political Science has become self-perpetuating (low success rate leads to low salience, low expectations, and lack of models for success) and this may indeed be the case.

Response: PARTIALLY AGREE. The CAREER program has recently undergone a COV of its own, which may result in changes of policy or practice. We agree with the COV members that the CAREER grants pose some fundamental difficulties to junior political scientists. It may be difficult, and possibly not in many junior scholars' interest, to prepare CAREER grants, when the chances of getting a standard grant are low and those of a CAREER grant lower still. For many scholars, the added value of the CAREER grant may well be offset by the lower probability of success. Moreover, we think the COV members raise an interesting point that the CAREER grant may be better suited for junior scholars in the other sciences. We also agree that it would be unwise to award CAREER grants on the basis of a young scholar's promise rather than the specific content of the proposal. The COV members are correct that funding a subpart proposal could actually hurt such a scholar's progress by "saddling" that scholar with a problematic research project. Finally, we somewhat agree that funding even one per year may prevent us from funding more promising regular proposals given our current budget constraints. That said, CAREER is an NSF-wide program and all fields of science supported by the Foundation are expected to participate.

On broadening participation:

NSF's most effective and appropriate strategy for promoting participation of underrepresented groups is by funding research proposals.   Of course, we would like to see the fraction of proposals involving women and minorities increase, and we believe that some NSF activity is this area has paid off. Training programs like the Bunche Institute (which promotes recruitment of minorities into Ph.D. programs) and EITM (which deepens the quality of graduate training) are reasonable long-term strategies for increasing diversity.   We were less persuaded that the Workshop on Women's Advancement in Political Science produced much of value, though we note that it was not a costly undertaking.

Response: PARTIALLY AGREE. We are gratified that the COV members praise several steps the program has taken to broaden participation, and we will continue to look for further opportunities to broaden participation. To be fair to the goals of the workshop, it did lay the foundation for an NSF ADVANCE proposal. Unfortunately, while this proposal was reviewed favorably, there were not funds available for its funding. Moreover, we believe that while the Workshop on Women's Advancement may not yet have produced direct, tangible benefits, it has been useful for directing attention to areas, such as mentoring, where the program can look for opportunities to broaden participation effectively. That is, the workshop has proven useful to program directors in directing our attention in proposals to the types of activities that are likely to promote broader

participation and away from those likely to have little effect. In this sense, the small cost of the workshop to which the COV members point, may have significant benefits in directing our investment toward the most productive activities for broadening participation. We do agree, however, that this is an area where we have to be proactive in evaluating the effects of these investments.

On Future Directions:

Among the important new opportunities for research in political science are the following.

1. Global climate change and government capacity. Currently, little political science research directly addresses the increasingly important challenges posed by disasters and changes in the environment. Yet the general question of how to design effective government institutions is subject of much ongoing high quality research. We would recommend efforts to connect basic research agendas on in the area of institutional design to the specific problems posed by hurricanes, tsunamis, and similar events. Equally important, our knowledge of whether and when governments can solve the collective action problems (both within and among countries) can be effectively applied to problems associated with global warming.

2.Health crises. The same can be said about research on infectious disease. Infectious disease is a burgeoning area of research in the life sciences, and we are beginning to see high quality projects that address the questions of government capacity to manage disease and (or) to participate with other governments to this end. Again, this is an area in which general models of institutional design can be fruitfully applied to problems of significant social magnitude, in a way that both sheds light on the particular problem and advances basic theoretical knowledge.

3. Human Biology and Politics. Our understanding of how brain structure affects the human behavior and of the genetic components of individual behavior, choices and preferences has grown enormously in recent years. Advances in neuroscience and genetics offer alternatives to the standard rational choice model of behavior, as well as suggesting empirical regularities that could be fruitfully incorporated into standard theories.

4. Institutions. Still more research on electoral systems—including voting technologies—should be supported. The two SGER grants on Making The Vote Count and on the quality of the voting experience (Magleby) are indicative of the kind of projects that ought to be encouraged.

5. Methodological challenges. We must find ways to study whole political systems; reductionist approaches can answer narrow questions. But topics like those mentioned above require models of large scale that allow for nonlinearities and

*dynamics. Research should be encouraged in areas that link social network
theory, complex (agent-based) systems, and evolutionary game theory to data, in
particular in ways that promote falsification. This is the kind of research that
some of us think is most like to be genuinely transformative.*

Response: PARTIALLY AGREE. We agree that the areas of research highlighted
by the COV report are important ones that have the potential of transforming the
discipline. We are beginning to see proposals in some of these areas, such as Human
Biology and Politics and Environmental Governance – in fact, we funded a proposal
in Fall 2006 (SES-0648447) that looks at institutions governing forest use in Latin
America. We will also continue to explore opportunities to fund projects in these
other areas exclusively through our own program, through broader co-funding with
other programs, and through Foundation-wide initiatives. Finally, these areas may
also be ones where we consider greater use of SGERs or workshops to encourage
research.

However, we believe that there are two areas of current research that could continue
to have a major impact on research in Political Science and warrant special mention.
These are EITM and Qualitative Research Methods. Both of these areas have been
addressed earlier in these responses. EITM has had a transformative effect on the
field of Political Science and continued funding will confer additional benefits and
provide a model for other disciplines.

Continued funding of Qualitative Research Methods could also have a major impact
on the discipline. While many political scientists use qualitative methods, most do so
in an ad hoc and non-rigorous manner. Unlike quantitative methods, little systematic
training is available in this area. As such, funding quality proposals in this area as
well as continued support for training institutes could help transform many areas of
inquiry, including ones that focus upon whole systems, where quantitative methods
have some methodological limitations.

Response to the 2007 Report by the Committee of Visitors

Science and Society Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The Committee of Visitors (COV) – Drs. Helen Longino (Stanford), Sharon Kingsland (Johns Hopkins), and Stephen Hilgartner (Cornell) – convened on March 8-10, 2007 to review the Science and Society (S&S) Program in the Division of Social and Economic Sciences (SES). Dr. Shari Diamond represented the SBE Advisory Panel for the cluster of which Science and Society (S&S) is a part. The S&S Program Directors –Fred Kronz and Priscilla Regan – joined the COV as requested for portions of the meeting. The COV addressed two sets of topics: (1) oversight concerning the management of the Program and (2) evaluation of the outputs and outcomes of NSF investments in the Science and Society Program.

Summary Observations: In general, the COV concluded that it "was very impressed with the efficiency of the program," that the management of the program was excellent but the two PO's were clearly overworked." The COV also concluded that the "projects funded in this program are conceptually diverse and highly innovative." The COV did raise some concerns and make some recommendations, each of which is discussed below.

Specific Comments and Recommendations with Program Responses:

Recommendation

Both of the merit criteria, intellectual merit and broader impact, were carefully considered throughout the process. Reviewers were often thoughtful not just about how neighboring disciplines would benefit from a project, but were also attentive to broader social and educational impacts, in all their multiple dimensions ranging from the academic community to the larger society. *Occasionally reviewers did not seem to know what "broader impact" referred to, or how to address that criterion, and would benefit from some instruction on what is intended here.*

Partially Agree: This appears to be a more systemic problem than one that is unique to the Science and Society Program. The Program has a link in its letter to reviewers with the information on what is meant by both of NSF's review criteria. We will add a fuller explanation of the "broader impacts" criterion to the text of the review request letter in order to help clarify its meaning and importance.

The Ethics and Value Studies (EVS) component of the Science and Society Program recently funded a workshop on the broader impact criterion. It may be beneficial to have

a number of SES program officers attend that workshop and make its report generally available.

Recommendation

The proposals supported have received excellent ratings from the reviewers. *The dissertation improvement program is excellent and we strongly support its continuation and even expansion.* From 2004 through 2006 the program received 1016 proposals, including 806 regular proposals and 210 dissertation proposals. The regular proposals included 34 nanotechnology proposals and 85 Ethics Education in Science and Engineering (EESE) proposals that were not reviewed by this COV. The overall funding rate is 29 percent, suggesting that the process is very competitive and that only high quality proposals get funded. If dissertations are not included, the funding rate is 27.9 percent.

Partially Agree: The Program agrees that the dissertation improvement grants are an important and integral funding mechanism. The Program definitely supports its continuation and would support its expansion in the areas of philosophy of science, ethics and values studies, and policy studies. The number of dissertation proposals in history of science and social studies of science is appropriate at this time and does not need to be expanded. In the Program's outreach activities, dissertations in the other areas will be strongly encouraged.

Recommendation

The project budgets are appropriate and amounts awarded are appropriate. Projects that require more funding or more time probably are not being proposed because PI's are aware of budgetary constraints. As the program develops *we believe it will be important to continue to fund individual scholars and maintain the dissertation awards, but it will also be important to be able to fund more ambitious projects, such as research involving multiple sites or more collaborators, that might contribute to theoretical development.*

Partially Agree: If there were to be additional funding, the Program would support more ambitious projects as suggested. However, in the absence of additional funding, the Program believes that funding more awards at lower funding levels is preferred over funding a small number of large awards. This strategy seems appropriate given the nature of most research activities conducted in STS.

Recommendation

The program is the locus for a form of self-reflection on the part of the entire research community. Scholars in the S & S areas offer resources for examining the effects of

different forms of organization of research, of the effects of methodological preferences and choices, of the interaction between social values and priorities and research agendas and research practices, for studying (and evaluating) the character of hypothesis and model assessment and the structure of knowledge in the various fields of science, for studying the conceptual foundations of fields like physics, biology, and chemistry, for studying the forms of scientific change, both incremental and revolutionary/transformative, for studying modes of applying scientific research to technical and practical problems, to understanding and making recommendations for the improvement of public understanding of science. *The program has been flexible enough to respond to top-down initiatives like the Nanoscience initiative while continuing to support ongoing research deemed important by the S & S in a bottom up fashion. Continued support of the field in this way is necessary if we are to respond to future developments in sciences and technology.*

Agree: The Program agrees that expansion of the base support for core programs, such as Science and Society, is critical to the NSF and well positions SES both to respond to new initiatives and to advance new ideas. The example of S&S's being able to take a leadership role for the societal and ethical implications of nanotechnology is indeed a good example of the former. The Biology and Society "Dear Colleague Letter" provides an example of the latter.

Recommendation

The COV was very impressed with the efficiency of the program. We believe that the program requires three full-time program officers; presently the program is being run by two rotators who are doing an excellent job, but are clearly overworked. Two positions are currently being advertised, one to replace a program officer who left in January, and the other to replace a rotating officer who will leave in June 2007. *In order to maintain institutional memory, which is crucial to running an efficient operation, we recommend that of the three program officers, one be a permanent position.*

Agree: The Program agrees that having three program officers, one of whom is permanent, is absolutely essential.

Recommendation

The POs are currently considering whether one panel could be convened to consider all proposals in the 4 supported areas instead of 2 as is currently done. This might better reflect the unification of the ethics and policy research with the HPS and SSS research under one umbrella and might broaden the comparison classes for rating of proposals. Such a change should take into account possible impacts on individual panel members' workloads. Consideration should also be given to the question of how this might affect the distribution of funds to the two groups (normative and theoretical/empirical).

 Agree: In discussions about restructuring the management of the Program and having one panel for faculty proposals and one for dissertation proposals, the Program has been

very conscious of the considerations about panel members' workloads and distribution of funds. The Program believes that these changes can be made without burdening panel members by increasing the size of the panel. Questions about distribution of funds will be addressed by the program officers as they create a balanced portfolio of awards; the Program believes the present balance is basically appropriate and would expect that to be maintained.

Recommendation

The task of addressing S&S issues, like other areas of NSF activity, involves both capacity building and taking advantage of important opportunities for advancing research. The S&S programs have had a significant impact in both respects. The existence of these programs in NSF has produced a wide body of research results and it has also contributed to the growth of a vibrant research community. *As the program evolves, the COV believes that it will need to find ways to expand the number of scholars working in this domain.*

Agree: At this point the primary way that the Program expands the number of scholars is through the dissertation grants. Given the interest that exists in the broader academic community, it is likely that increased funding for SGERs and IGERTs would also help expand the number of scholars in this area. Pursuing such opportunities is a task for the research community.

Recommendation

The role of the S&S program in helping develop an NSF effort to address the social and ethical issues surrounding nanotechnology nicely illustrates the importance of having institutionalized capacity in place. The existence of NSF's successful S&S programs, and the diverse academic workforce with relevant expertise that these programs have cultivated, enabled the Foundation to respond quickly and appropriately. As with other basic research, investigations in the history, sociology, philosophy, anthropology of science, and science & technology studies, can become relevant to development in completely new areas.

Agree: The Program agrees that having a strong program well positions NSF to respond to new initiatives that are relevant to the Program. Given the role of science and technology in fostering social and economic changes, it is likely that there will be an increasing number of instances where NSF is tasked with generating research on the social and ethical issues surrounding new scientific or technological advances.

Recommendation

Maintaining the core mission while expanding into new areas. We believe that all current forms of funding are important and should be continued. It is clear that this Program should participate in broader NSF initiatives such as the Nanostructures and Nanosystems

initiative, and program directors have correctly identified significant ways in which this Program contributes to these broader goals at NSF. Looking to the future, it is likely that such emerging initiatives as Environment and Society, or Science of Science Innovation and Policy, will both generate keen interest in the scholarly community encompassed by S and S. These are worthwhile efforts; the "Dear Colleague" letter on biology and society represents an appropriate model for soliciting relevant research proposals. Similar efforts are worthwhile in such areas as computer science and environmental sciences. However, while we are enthusiastic about such expansion, it is also important that participation in these special initiatives not erode the core work of the Program, which supports the development of the various disciplines and sub-disciplines in history, philosophy, and social studies of science, engineering and technology. Research and educational programs in these fields depend crucially on the support of NSF and the breadth of coverage that the Program now has should be preserved. As new initiatives are explored, there is also a need to expand the base or core activities that enable the scholarly community to thrive.

Agree: The Program agrees that special initiatives should not erode the core work of the Program. Special initiatives can only effectively be built on the bases of strong disciplinary and sub-disciplinary work. If special initiatives were to replace core programs, the result would not only be the erosion of the core programs but also the capacity to respond to new initiatives. In much the same way that strong disciplines are crucial for robust inter- and trans-disciplinary work, strong core programs are crucial for special initiatives.

Recommendation

Since the last big round of Integrative Graduate Education Research Traineeship (IGERT) [that is, programmatic support for graduate education] funding in S&S (in the early to mid 1990s) and partly as a consequence of that, the field has developed substantially. The IGERT funding helped develop a cadre of scholars through graduate student and postdoctoral support, fostered the development of cross- and interdisciplinary partnerships within institutions, and made S&S scholarly activity more visible nationally. The COV believes that some new initiatives similar in scale to the IGERT and dedicated to consolidating the advances made in the past 15 years could have similar effect and we urge the Foundation to consider another round of such funding.

Partially Agree: It is difficult to target funding to a particular field within SES. The Program believes that in its outreach to the STS community, more emphasis could be given to existing IGERT, HSD, and ADVANCE programs as well as to new special initiatives such as the Science of Science and Innovation Policy. The IGERT program would welcome proposals from the STS community, and it is up to that community to write proposals to compete for those funds.

Recommendation

In addition, we agree with POs that developments in philosophy of science and social studies of science in the last five years provide openings for greater interaction and potential collaboration among researchers from the two areas. An exploratory workshop or mini- along the lines of the Research Policy as an Agent of Change (RPAC) workshop could be very helpful in catalyzing such collaborations.

Agree: The Program agrees and would very much like to see more collaborations between scholars in the philosophy of science and the other three components of the S&S program (social studies of science, policy studies of S&T, and ethics and values studies). A workshop would provide an excellent opportunity to identify and catalyze such interaction and collaboration.

Recommendation

We notice that a few projects in the last 10 years have addressed science and technology issues in developing societies. Many projects we could envision, especially related to the environment initiative, would be focused on science and technology in developing societies. Here again an exploratory workshop could be of use in helping frame issues and identify potential areas of concern. Such research is in any case likely to be more costly and cumbersome to administer and we urge the program to consider how to respond to what is likely to be an emerging area of scholarly interest.

Partially Agree: The Program has already seen an increase in the number of proposals addressing S&T issues in developing societies and agrees that the environmental initiative is likely to produce more. The Program is not sure that a workshop is necessary to frame the issues as it seems they have already been well framed in the scholarly literature. The Program does agree that research projects in developing societies are more expensive. In the past the Program has had somewhat mixed success in securing Office of International Science and Engineering funding.

Recommendation

The COV was impressed with the excellent articulation of a broad research agenda in the February 2007 report of the NSF Workshop on Social Organization of Science and Science Policy. The science of science and innovation policy will require conducting a wide range of studies on topics including innovation and well-being; social environments for innovation and creativity; political economic of science, technology, and innovation policy; evidence and expertise in science-intensive decisions making, and science, technology, innovation, and global change. The COV believes that the S&S programs have much to contribute to this domain.

Agree: The S&S Program expects to continue to play an active role in the Science of Science Policy initiative and agrees that the Program has much to contribute.

Response to the 2007 Report by the Committee of Visitors

Sociology Program
Division of Social and Economic Sciences
Social, Behavioral, and Economic Sciences Directorate
April 2007

The Committee of Visitors (COV), Drs. John Kennedy (Indiana University), Vilna Bashi (Rutgers University), and John McCarthy (Pennsylvania State University), convened on March 8-10, 2007 to review the Sociology Program. Dr. Guillermina Jasso (New York University) represented the SBE Advisory Panel for the Social and Political Sciences COV cluster. The Sociology Program Directors, Patricia White, Paul Ciccantell, and Kevin Gotham, joined the COV as requested for portions of the meeting.

1. Recommendation: Program Staff prepare a detailed template for ad hoc reviewers.

PARTIALLY AGREE: We appreciate the COV's desire to see all of NSF's review criteria addressed in each review. The Sociology program officers are limited in their ability to address this recommendation because the National Science Board establishes the review criteria for research projects. Fastlane provides information that reviewers can access to assist them in preparing reviews. The program directors, however, seek consistency and quality in panelist reviews by incorporating more explicit instructions in panel participation instructions about what is expected in reviews and notifying panel members when reviews omit key components. Program directors are less able to influence the quality of ad hoc reviews, but will consider whether redrafting the reviewer letter to include additional instructions not provided in Fastlane would potentially encourage more thorough comments.

2. Recommendation: Program Staff encourage ad hoc reviewers to fully explain the broader impacts of the research.

AGREE: The COV members pointed out that the vast majority of Sociology Program reviews addressed the Broader Impacts criteria in their reviews, but reviews focused on the scientific impacts of the research and less frequently on the impacts of broadening participation, teaching, training, learning, and research infrastructure. The COV would like reviewers to address all dimensions of NSF's Broader Impacts criteria in their reviews. The Sociology program directors will revise the review request letter sent to ad hoc reviewers to encourage a more thorough assessment of the broader impacts of research projects. Yet we should also note that it is difficult for the reviewers to assess the broader impacts of a proposal if the intellectual merits are not clear.

3. Recommendation: The Program Staff attempt to increase the proportion of senior scientists as ad hoc reviewers while paying careful attention to the overall diversity of the reviewers.

AGREE: The Sociology Program routinely requests reviews from both senior and junior scholars. As the COV report notes, it is more difficult to get senior scientists to complete reviews. The program directors will continue to request ad hoc reviews from both senior and junior scholars and further enhance the Program's already diverse body of reviewers.

4. **Recommendation: The Sociology Program continue to balance its portfolio by providing awards to projects that will advance the science of sociology and address national priorities.**

AGREE: The Sociology program directors will continue to participate in cross-disciplinary and cross-directorate programs and encourage sociologists to submit proposals to these programs. The program directors will continue to identify and support cutting-edge sociological research that advances the field and contributes to national priorities such as enhancing the diversity of the STEM workforce, increasing U.S. competitiveness in the global economy, understanding the impacts of immigration, and addressing social inequalities.

5. **Recommendation: The Sociology Program should organize a small conference of distinguished researchers known for their creativity and forward thinking to discuss and make recommendations for funding for awards that may lead to transformative research.**

PARTIALLY AGREE: NSF is very interested in promoting transformative research, and the Sociology Program should contribute to this effort. The Sociology Program will explore the conference mechanism, as well as other potential avenues, for identifying and implementing ideas that will encourage the submission and funding of potentially transformative research.

6. **Recommendation: The Sociology Program should attempt to balance the awards to quantitative and qualitative dissertation research proposals.**

AGREE: In examining the portfolio of dissertation proposals submitted and funded over this COV period, the COV members felt that there was an under-representation of proposals using quantitative methods. This appears to be due to the funding policies for these grants that are a better fit with qualitative research, including funding travel for research away from the home institution, transcription services, survey costs, and laptop computers for fieldwork. In contrast, dissertation research using quantitative methods tends to use datasets and computer facilities that are readily available on the students' home campuses, with the result that there are fewer tasks appropriate for funding by the Sociology Program. The Sociology program directors and members of the Dissertation Advisory Panels in recent years have discussed this issue extensively. The Program will continue to solicit input on this issue, seek mechanisms, and create guidelines that will

encourage the submission and funding of high quality quantitative as well as qualitative dissertation proposals.

7. Recommendation: The Sociology Program recruit two rotators or one rotator beyond the one or two years that has become the norm duration of incumbency.

PARTIALLY AGREE: The Sociology Program proposal load has grown significantly in recent years, particularly when program participation in inter-disciplinary initiatives is included, so having one permanent and two rotating program directors would strengthen the program's managerial capabilities and the ability of the program directors to participate in directorate-wide and cross-directorate activities. Increasing the length of rotators' terms beyond the current norm is constrained by NSF-wide policies for rotators. Also, the number of staff allotted to a particular program is based on management need assessments for staff placement.

8. Recommendation: The Sociology Program continue its guidelines for funding of awards.

AGREE: The 2000 COV provided guidelines for award budget categories, e.g. no more than $15,000 per year for summer salary, no support for the purchase of computers unless a compelling rationale is provided, and the expectation that the university will provide office supplies and administrative support from indirect costs. These guidelines were again reviewed by the 2004 COV and they recommended that they remain in place in order to make the best use of limited program funds. The Sociology Program agrees that the funding guidelines currently in place, given little or no projected increases in the base program budget, allow it to maintain a funding rate of approximately 25 percent.

9. Recommendation: The Sociology Program support a graduate student who would conduct research on dissertation awardees to determine their early career patterns.

AGREE: The Sociology program directors have begun working with the Director of Research at the American Sociological Association to design and conduct a study of the career impacts of receiving an NSF dissertation improvement grant. This study will be carried out during FY 2007, with a report anticipated by May 2008.