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# Evaluating transformative research programmes: A case study of the NSF Small Grants for Exploratory Research programme

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This article describes an evaluation of the National Science Foundation's Small Grants for Exploratory Research (SGER) programme conducted for NSF by SRI International (a non-profit research company). SGER was a 16-year programme sponsored by NSF and operating across the agency from 1990 until 2006 to encourage programme directors to invest in high-risk, high-reward research that might not pass the traditional peer review process. This article provides a detailed background of SGER; a description of the outcomes of the programme; details about the methodology used to evaluate the SGER programme; and the findings of the evaluation. The analysis shows that SGER was highly successful in supporting research projects that produced transformative results as measured by citations and as reported through expert interviews and a survey. However, the NSF programme directors as a whole underutilized the tool for most of the years it was in operation spending far less than the allowable funds allocated to exploratory research; this suggests that internal actions to take risks may not have been rewarded. Moreover, the programme itself was successful beyond expectations. A high-risk programme would be expected to have transformative results in just a few cases. SGER had transformative research results tied to more than 10% of projects. This suggests that programme managers remained risk averse and continued to support projects that were likely to produce positive outcomes.

*Keywords: transformative research; high risk research; National Science Foundation; case study.*

## 1. Introduction

This article describes an evaluation of the National Science Foundation's Small Grants for Exploratory Research (SGER) programme. The article provides a detailed background of SGER; a description of the outcomes of the programme; the methodology used to evaluate the SGER programme; and the findings of the evaluation[1]. SGER was a 16-year programme sponsored by NSF and operating across the agency from 1990 until 2006. It was replaced with the EAGER[2] and RAPID[3] programmes in 2007. In requesting the evaluation, NSF specified two broad charges: 1) to evaluate the SGER's 16-year programme for its contribution to NSF's mission, and 2) to provide input to an internal planning committee considering changes to SGER's mission and operation. SGER was considered within the agency as having been successful in achieving its goal of providing flexible funding for programme managers seeking to fund risky or speculative projects that might not have succeeded in passing NSF's strict peer review process,[4] but there had been no evaluation of its overall impact.

SGER was part of a trend across the US Government to increase support for what is called 'transformative' scientific research. Sometimes called 'exploratory' or 'high-risk, high-reward' (HRHR) (the terms remain imprecise in part because different agencies had varying goals for the programmes); the idea is to push research to test limits and challenge existing paradigms. The trend was partly motivated by a concern that peer-reviewed research results were biased against high risk or exploratory research. This had not been proven to be the case, but this view became widespread and the quest for transformative outcomes was commonly cited as a goal of publicly-funded research in the USA.[5] For example, in the fiscal year (FY) 2011 guidelines to research agencies, the Office of Management and Budget recommended that 'Agencies should pursue transformational solutions to the Nation's practical challenges, and budget submissions should therefore explain how agencies will provide support for long-term, visionary thinkers proposing high-risk, high-payoff research' (OMB Circular 2010). Several federal agencies have experience in setting aside funds to encourage high risk, potentially transformative research projects, including the Department of Defense,[6] the Department of Energy,[7] the Department of Health and Human Services,[8] and the National Science Foundation.

The concept of creating an alternative programme for high-risk research emerged in part because of concern that peer reviewers might reject truly transformative research proposals (Roy 1985; Porter and Rossini 1985; Horrobin 1990). This assertion emerged from science studies and policy analysis—a discussion throughout the 1980s that culminated in a book, *Peerless Science,* by D. Chubin and E. Hackett (1990), critiquing the gaps in the peer review process. SGER programme designers were likely to have been influenced by this literature, which suggested that research traditions, personal commitments, and other interests affect peer review, resulting in *a conservative bias* on panels considering funding for grant proposals (Chubin and Hackett 1990). The science studies literature suggested that the conservative bias could render peer reviewers unable to select truly innovative approaches to science; and this was bolstered by survey results reported by Chubin and Hackett (1990) showing that scientists generally suspect that peer reviewers are reluctant to support unorthodox or high-risk research.[9]

Attempts to evaluate HRHR programmes are challenging along nearly every parameter typically used in programme evaluation. The extent of risk is unknown. Practices such as counting outputs, outcomes, and social impact are possible for individual projects, but these data cannot validly be aggregated for the programme as a whole. Moreover, defining for the purposes of evaluation the counterfactual or input–output factors are precluded by the number of variables that influence the outcome of any activity undertaken or funded through high-risk mechanisms. Neither a randomized control trial nor a quasi-experimental design method is valid due to lack of control groups. While it is reasonable to expect that HRHR programmes will produce at least some notable outcomes, the percentage share of positive versus negative outcomes is beyond quantification, and indeed might be an absurd measure—risk-taking is inherent to research, as is uncertainty of outcomes. Transformative outcomes in science are not subject to a law of averages or regularities that would populate a model or statistical expectation of outcomes. Most reasonable observers would react negatively to an HRHR programme that produced too many successes and consider it risk averse. (But would this be the fault of the programme, or a structural issue within the agency?) Conversely, too many failures would raise questions about the judgment of programme managers in making educated guesses. No group would wish to be seen as having such poor judgment that they altogether missed potentially transformative research. But how wide is the gap between the expectation of success or failure? And what does seeming success or failure look like?

Adding to the challenge facing an evaluator is the variation among missions tied to funding sources, with some directorates within NSF or agencies across government having a basic research mission, and others having a development focus. Many government agencies, such as health, energy, and defence, have national missions that can be translated into expected outcomes, such as improved energy prices or reduced morbidity; in contrast, NSF funds basic research that generally results in new knowledge—an output that is difficult to tie directly to social or economic welfare. The evaluator must develop expectations as to the level of output and outcome that might be expected to emerge based on mission. Ironically, the lack of results could be a positive output measure because, as noted, a programme that produces too many successes would be viewed with suspicion. The question of thresholds of acceptable rates of success or failure poses special challenges for the evaluator since inputs and outputs will be weakly correlated.

## 2. Background on the NSF SGER programme

The Small Grants for Exploratory Research (hereafter, SGER, and known in the vernacular, somewhat fondly, as 'sugar') programme ran within the National Science Foundation from FY 1990 until the end of FY 2006. It was established with the goal of providing funding flexibility for NSF programme managers to identify and fund projects where the nature of the work made it worthy of a small amount of funding without subjecting it to the standard peer review process. The programme was designed to encourage and support several types of research efforts of short duration (up to 2 years) with small amounts of funding (up to \$200,000) in contrast to 'regular' fully reviewed proposals. SGER was a tool available to all of the NSF programme managers to use at their discretion—only the broadest guidelines were offered to guide its application.

SGER grew out of an earlier programme, Expedited Awards for Novel Research (EANR), initiated in 1987 by the Engineering Directorate. In January 1989, NSF Director Erich Bloch called for an evaluation of EANR, and an external advisory panel representing several disciplines was convened to examine the pilot. A brief assessment conducted in advance of the panel meeting concluded that the projects supported under EANR would not have received favourable treatment in peer review, often because the underlying concepts had not been sufficiently developed or tested. The panel recommended that NSF authorize Foundation-wide use of an EANR-like mechanism, and Mr. Bloch presented this recommendation to the National Science Board in May 1989 as part of the Director's annual briefing on proposal review. An internal task force then developed the specifications for SGER. In his memo authorizing SGER for use beginning in FY 1990, Mr. Bloch stated his expectation that '[w]idespread use of SGER grants has the potential to facilitate research advances, reduce applicant paperwork, accelerate decision making, and attract investigators who might not otherwise be inclined to propose high-risk work to NSF'.[10]

SGER operated for 16 years until 2007 when it was superseded by programmatic change at NSF. Over 16 years, NSF funded a total of 4,972 SGER awards, committing $284 million (in current funds) for an average of about $54,000 per grant. Most of the SGERs were for smaller amounts and shorter durations than allowed by the guidelines. SGER proposals are meant to result from discussions between a programme manager and a research practitioner; proposals were brief (several pages) and were acted upon by the programme officer without review by external peers. Grant funds were processed in much less time than was the case for peer-reviewed proposals. More than 3,700 researchers have been principal investigators on an SGER grant, meaning that some researchers received more than one SGER grant.

In 2001, NSF conducted a review of SGER. The evaluators found that the SGER funding mechanism was about 0.6% of the agency's operating budget, meaning that the programme was operating far below the 5% of funds that could to be committed to this activity. The majority of the funds had been committed to some form of exploratory or potentially transformative research, as opposed to 'urgent' activities—which was another possible use for SGERs—a finding we confirmed in our evaluation. In the review for this article, the term 'urgent' appeared in one out of 100 grant titles (between 1990 and 2002); the term 'Katrina' (Hurricane Katrina occurred in 2005) was found in the title of 220 grants; and the term '9/11' (the USA was attacked by terrorists in 2001) was found in the title of 35 grants in 2001–02.) The grants made for 'urgent' activities were less than 5% of all SGER grants in any one year.

Research practitioners could approach an NSF programme director with a request for a 'sugar' grant. Those who were contemplating an SGER proposal were 'strongly encouraged' by NSF to seek guidance from a programme officer as to whether the proposed work was suitable for SGER consideration. From interviews conducted for this evaluation, NSF staff said that most SGER awards were preceded by a conversation between the programme officer and the principal investigator. As a result of pre-submittal discussions,[11] the award rate for SGERs was considerably higher than for regular proposals. For example, in FY 2005 the NSF-wide SGER award rate was 76%—more than three times that of reviewed proposals.[12]

SGER was not based within any of the Directorates in NSF (names have changed over time, but generally the Directorates are aligned with scientific disciplines such as biology, chemistry, and mathematics). The programme operated as an agency-wide mechanism available to all NSF programme directors (PDs) so they could award small grants for specified types of research, based on their own judgment, and without external peer review. This evaluation showed that all divisions within NSF made use of the SGER tool, and that the most active users were the Civil, Mechanical, and Manufacturing Innovation Division (CMMI), Chemical, Bioengineering, Environmental, and Transport Systems (CBET)—both in the Engineering Directorate—and Information and Intelligent Systems (IIS) from the Computer and Information Science and Engineering Directorate (CISE).

Discussions held with NSF staff about the formation of the SGER programme revealed divergent opinions about whether NSF peer-reviewed research was indeed risk-averse (many felt that NSF makes appropriate decisions that include risk or exploration); but nearly all of those interviewed agreed that the SGER mechanism allowed PDs the ability to offer funding to investigators with unique approaches or exploratory ideas that might have failed to obtain funding through traditional peer review. This was accomplished through two avenues: 1) single proposals that matched one or more of the five guidelines suggested for SGER or 2) those that were part of a multi-proposal response to a specific NSF 'call' for proposals. NSF PDs handled the single proposals. If NSF requested proposals for SGER proposals, NSF staff managed the review internally by convening a panel of several staff members. (This process did not constitute a traditional peer-review, however.) Not all proposals of the first type were generated solely through the applicant's initiative. NSF staff was authorized to suggest the use of the SGER mechanism to an investigator who appeared to have an interesting idea. In addition, at their discretion, NSF staff could convert a regular proposal to an SGER grant if that proposal had merit but the government official felt it would not pass peer review. Based on our interviews, this approach appeared to be rarely used. More often, the NSF staff person would suggest that a scientist develop an SGER proposal around a promising component of a regular proposal that had been, or which they believed would be, declined in a traditional peer review panel.

There were only a few small changes to the SGER programme over the 16 years of operation; the agency increased the award ceiling (from $200,000 to $400,000) and the duration of the life of the grants, but the formulation of SGER up until FY 2007 was conceptually and operationally similar to its original purposes and practices.

## 3. Methodology for evaluating SGER

In developing a retrospective programme evaluation, the evaluators considered a number of qualitative and quantitative evaluation methods. (Certain methods, such as a benefit–cost analysis, counterfactual analysis, or a stakeholder survey, were not chosen since these methods would not shed light on the programme's goal.) The evaluators conducted an initial informal interview process of programme participants to obtain input into the evaluation design. The evaluation had the goal of assessing the functioning of the programme, the impact of the programme on the careers of those who received the grants, and the impact on the scientific community of investments made in exploratory HRHR research. Evaluators chose client-centred logic modelling, literature review, interviews, surveys, and bibliometric and patentometric analysis as tools for analysis.

The evaluation began by identifying the implied goals of the programme based on historical documents and interviews. The SGER funding guidelines presented the first set of inputs to help structure the evaluation. The SGER mechanism, as envisioned from the start, enabled programme managers to fund projects with a level of 'acceptable risk' without further definition. The limits placed on the grant defined the boundaries of acceptable risk as (a) the funding amount that could be committed was relatively low ($200,000) compared to 'traditional' grant awards, and (b) the programme manager could make a funding decision without justifying the decision to a peer review panel, thus reducing the burden on the investigator to show probable success. The SGER mechanism involved reduced organizational risk, since the potential 'waste' of resources was limited, and the reduced personnel time saved money. In addition, the mechanism aided the programme manager, since there was little pressure to ensure that the award decision conformed to the assessment of other scholars in the field.

In developing tools to assess the extent to which the programme met its goals, we reviewed the goals established by NSF for SGER. The programme grant proposal guide specified five characteristics of research that would benefit from SGER:

(1) preliminary work on untested and innovative ideas;
(2) ventures into emerging and potentially transformative research areas;
(3) application of new expertise or approaches to 'established' topics;
(4) efforts likely to catalyze rapid and innovative advances;
(5) work having a severe urgency with regard to availability of data, equipment or facilities, including quick-response research on disasters and similar unanticipated events.[13]

Categories 1–4 are broad, not mutually exclusive, and limited to qualitative assessment. According to the NSF-determined guidelines, transformative research by its nature would go beyond existing accepted theory, models, and methods, and thus could involve 'untested and innovative ideas' (item 1). The ability of transformative research to reframe and redefine an 'existing' concept or practice could be considered as similar to the 'application of new expertise or approaches to "established" topics' (item 3). If transformative research can work on 'emerging research areas' (item 2) or enable the creation of a new paradigm, then it is very likely to produce new insights which will 'catalyze rapid and innovative advances' (item 4) in one or more scientific fields. Thus, the evaluators considered that the guidelines presented overlapping and interconnected goals, and while they could provide pointers to transformative research, the guidelines did not by themselves define transformation or risk.

As drawn from literature, the various methods of supporting transformative or high-risk research include: 1) sponsoring what some call 'radical innovation,' 2) using untested and exploratory tools and concepts, 3) challenging existing theories, and 4) testing a new perspective. Each of these concepts, when applied to scientific research, could be associated with risk. High risk, in the view of the evaluators, connoted a high probability of failure, or at least of producing no reportable (publishable) results. This definition guided the process of developing interview questions, although no consensus exists as to how best to evaluate programmes with these characteristics.

The evaluators further explored the literature for ideas regarding how best to evaluate a programme dedicated to funding *potentially* transformative research. Part of this effort included studying other evaluations that addressed similar types of programmes, although the literature is sparse. Grant and Allen (1999) reported on an evaluation using an expert panel where members were asked to assess the extent to which five grant programmes funded research that was 'risky', 'novel', 'speculative', 'adventurous', and 'innovative'. The evaluation was conducted by applying epidemiological methods in the form of a masked randomized trial, which eliminated as much systematic error as possible. With the SGER grants, it was not possible to survey the reviewers, since the grants do not use peer reviewers, but it was possible to survey programme managers and successful grantees, and this tool was added to the evaluation protocol.

Generally, research evaluation protocols suggest using a risk profile that estimates the probability of failure under a given set of conditions. Creating such an estimate in this case proved especially challenging since the actual conditions that would support or inhibit success were themselves unknown. In fact, the value of knowledge production may be unknown for many years. (Consider the example of 'sleeping beauties' in science (Van Raan 2004)—defined as results that may appear unremarkable at first, only to be resurrected years later and applied to an emerging subject.) This issue of uncertainty across time is qualitatively different from the issue of 'risk', because it means that the programme officer (and therefore the evaluator) may not be able to reliably determine a risk profile for a proposed project. This reality also added to the evaluation challenge and turned our focus to using survey and interview tools in an effort to elicit from programme users how they viewed risk.

We determined that most of the SGER grants would not result in measureable outputs or outcomes but would have results in a broader context. To aid the definition of these results, the team developed a logic model, with the help of NSF staff, to identify specific goals against which the team could apply measures to identify any actual outputs, outcomes, or results. The evaluation tools were drawn from standard evaluation methods, but the context of the final analysis was framed with expectations developed from the literature review and initial discussions with NSF staff. Outputs and associated measures are listed below. In addition, the team expected to find several spectacular and transformative developments in a small number of cases from the SGER-funded activities, a result we set at about 10% of projects with significant outcomes and 1% of projects with spectacular outcomes based upon Kuhnian theory of normal and transformative research.

The evaluation team narrowed the focus to answering two questions, with a set of six expected outputs.

(1) Were the projects funded with SGER awards qualitatively different from projects funded through a standard grant process?
(2) Did a certain portion of SGER-funded projects produce 'transformative' results in a way that was different from what one might expect from a standard NSF-funded project?

| *Output* | *Measure* |
|---|---|
| New knowledge created | Bibliometrics review of publications, patents, and citations |
| Students trained | Survey responses to questions |
| New research tools created | Interviews and survey responses |
| New methodologies created | Survey responses to questions |
| Unique resources accessed | Survey and bibliometrics review |
| Databases created | Survey responses |

*Outcomes* expected included: 1) the filing of full research proposals with NSF or other funding agency; 2) new fields of science established and laboratories built; 3) knowledge disseminated through references; and 4) citations to SGER-funded work.

The evaluation process began with 20 interviews with NSF programme managers. This was followed by the task of compiling all of the NSF SGER grant information sorted into a data set, along with contact information for all the SGER grantees. A survey tool was developed and approved by the responsible US governmental agencies, and the tool was field tested with 10 awardees. An internet-based survey was sent to more than 4,000 grantees in spring 2008 (the survey had a response rate of 80%). A bibliometric and patentometric analysis was conducted on the grantees and set aside to check against the survey. An interim and final analysis resulted in several briefings as changes to the original SGER concept were considered; and a final written report was submitted to NSF.

## 4. Findings

SGER was considered by various user groups to have been successful in funding potentially transformative HRHR science when the decisions were made in tandem between the principle investigator (PI) and the NSF programme manager. The findings uphold the view expressed within foundational documents that government officials cannot rely on the statements of PIs themselves regarding whether or not their research proposals are 'potentially transformative', but that the programme managers must be able to exercise judgment based on their own expertise. One particular problem in making an advance determination of the risk or promise of research is that PIs themselves may not recognize that their research is 'potentially transformative' during the proposal stage. Based upon interview results and survey responses, a dialog between NSF officers and practitioners proved to be the most effective way to formulate many of the successful SGER projects that were funded.

Access to unique resources (enabled by the grant) is listed as an anticipated output in both the logic maps, and by 15% of survey respondents. Survey respondents reported that their projects involved access to specific databases, perishable data, archival collections and other materials; special equipment, tools, facilities, and cruise vessels; special collection sites; and particular individuals. Access to unique resources was more likely to be an outcome of the urgent and timely research projects than of the exploratory research projects.

The creation of new research tools and equipment were anticipated outputs of the SGER activities listed in the logic maps; and NSF staff members often pointed to the development of new tools, techniques, and equipment as one of the most notable outcomes of the SGER grants. In addition to reporting within NSF, 53% of PIs reported having developed new techniques, research tools, and/or instruments as part of their grant-based research, and 25% reported modifying a research tool or instrument.

In their comments, respondents identified additional outputs, including development of breeding stock, industrially useful design software, new methods for photogrammetric analysis of time lapse sequences, new methods for cross-disciplinary change, and a large-scale experiment for a research infrastructure 'that will persist for decades and will yield long-term benefits'. Other awardees discovered new archaeological sites, showed for the first time that DNA could be used to produce semiconductor structures, collected new data that was evidence of sudden and significant political change, demonstrated the potential for drought prediction, settled a 100+ year debate in the scientific literature, produced information that has been incorporated into curricula, and paved the way for creation of an alternative technology that may supplant the existing approaches in climate change research. Another contribution to knowledge, mentioned by several respondents, was proving the infeasibility of a research idea; these so-called 'failures' provide essential information by demonstrating what does not work.

In response to another question, more than a third of respondents (35%) said their SGER project contributed to knowledge about a topic of public interest. The most common of these topics related to: 1) climate change and the environment (air pollution, biodiversity, public health, conservation): 2) disasters (both man-made and natural); and 3) national security. Numerous respondents said their SGER work contributed to knowledge of disaster effects or disaster management (prevention, preparedness, response, recovery). Other reported topics of public interest included voting, medical devices and rehabilitation, teacher knowledge, and student achievement gaps.

Twelve respondents reported project outcomes that included creating a widely-used database. Examples of these databases included:

- yeast genes involved in lipid droplet assembly, with use in research on obesity, diabetes, and lipodystrophy;
- a multidimensional visualization tool that promoted multidimensional analysis of complex structured data, such as multidimensional text databases and information networks which led to a NASA-funded Event Cube project;
- a foundational database that led to transformative opportunities in relation to alternative energy potential for landfills;
- a database that is being used around the world for public knowledge: science, math, and geography teaching using visual environmental data.

'Conferences and workshops' was the final category of output expected from the SGER projects. Very few SGER grants actually sponsored workshops and none sponsored a full conference. In many cases, however, PIs reported having used grant funds to present their research at conferences and/or workshops or otherwise participate in or organize conferences with subsequent grant money.

We expected to find that SGER projects allowed students to be trained. This did not prove to be a significant benefit of the programme. The feedback from NSF staff and PIs was that the duration of SGER awards was too short to allow for student training. There was too little time allowed under the SGER mechanism to complete the proposed work properly and at the same time recruit students and postdocs for the project. There were examples cited of students and postdocs being supported as part of an SGER, but it was not a major part of the programme's benefits.

Among the outputs expected to emerge from SGER grants, the most commonly found were proposals for follow-on research. Two-thirds of PIs responding to an awardee survey reported using, or planning to use, the results from their first SGER project as a basis for a regular proposal. About 17% of these respondents had not submitted a regular proposal based on their SGER results by the time of the survey. Of those known to have submitted a proposal, 73% received an award, 18% were declined, and 9% had proposals still in review at the time of the survey.

### 4.1 Differentiating features of SGER-awarded projects

The SGER mechanism was intended to fund work on ideas which are intellectually valuable, but which might have been rejected by a peer review panel. Analyses of the merit review system have pointed out that 'traditional' grant review processes may suppress 'potentially transformative research', although the evaluation did not ask this question. What it did ask is whether SGER allowed NSF programme directors to provide support for proposed projects which were (a) likely to be rejected by a peer review panel due to the nature of the proposed research, and (b) worthy of support due to the potential outcomes if the project were successful.

There is no reliable method for predicting if a peer review panel would indeed reject a proposal, but we asked in the survey if a version of the SGER project had been rejected prior to the SGER grant. It is useful to note that over 1,000, or 25%, of the SGER awardees surveyed reported that at least one of the following reasons was an 'extremely important' factor in their decision to pursue SGER funding: 1) The proposed research idea would have been considered too 'high risk' by a review panel; 2) The proposed research idea countered a conventional paradigm which might prejudice a review panel; 3) The proposed research idea was too new to be understood by a review panel; 4) The proposed research idea was too controversial to be considered fairly by a review panel.

While these responses are subject to the usual caveat tied to self-reporting error, the data indicate that some PIs used the SGER mechanism to pursue funding in situations where they felt deterred from pursuing a standard NSF grant due to the nature of their proposed research. In this sense, it seems reasonable to conclude that the SGER mechanism encouraged PIs to propose research ideas that were 'riskier' than they might propose in a standard grant process. The survey data indicate that a significant number of SGER awardees felt that their proposals were riskier than a peer review panel might accept. In particular, those respondents who felt that their proposed research constituted a 'venture into emerging or potentially transformative research ideas' believed that their proposals were 'too high risk' to be submitted for a standard grant.

### 4.2 Assessing the transformative outcomes of SGER awards

A finding that exceeded our expectations is the number of projects that indeed resulted in truly transformative scientific advances. The extent of the success offers some support to the original idea that spurred SGER programme formation—that the NSF peer review process was too conservative when choosing grants to fund. If the successes from within SGER would not have passed peer review, one can ask whether some part of the communication process was not operating as well as it could. The findings below highlight several of the most spectacular results where highly transformative research was funded (at least initially) by an SGER grant.

### 4.3 Cases of SGER awards with high-transformative research

The bibliometrics review showed that advances in nanoscale science and technology, biomolecular genetics, parallel computing, genetic sequencing, high-powered lasers, and advances in ecology, psychology, and philosophy can trace their origins to a high-risk grant given under this programme. In addition, the survey provided a list of candidate projects for additional review. The following cases summarize a sample of SGER awards where the PI and the Programme Director indicated that the project was either potentially transformative by design, or produced transformative results, or both. Analysts at SRI International identified these examples from a larger set of projects where the PI's survey responses, or the Programme Director's comments, indicated that the project was likely to have a transformative impact. Given the fact that most transformative research is not recognized as transformative except in retrospect, preference was given to projects where the SGER funds were awarded prior to 2002. SRI then conducted supplemental research on those projects, using bibliometrics measures and/or documentary materials from the web and elsewhere, to verify the significance of the results.

***4.3.1 DNA-based computing.*** In 1996 and 1997, an NSF programme director approved a series of SGER awards, some sponsored in conjunction with the DARPA Information Technology Office, on the topic of DNA computing. The interdisciplinary nature of the research, and the speculative nature of the proposed solutions, made these projects high risk, according to the programme officer. While these projects were not the first to explore the topic of biomolecular-based computing—the field had first gained attention from a 1994 article in *Science* on using DNA to solve the Hamiltonian Path problem—they were taking unconventional approaches to solutions. The SGER awards focused on some of the issues identified in earlier literature as barriers to the development of DNA-based computers. Peer-reviewed articles resulting from SGER-funded work examined alternative approaches to manipulating DNA molecules for computational purposes and applied their techniques to solving NP problems. Two associated SGER grants produced additional research that has become highly cited, including in mainstream articles within a pathway of progress towards DNA computing. These focused on the promise of DNA computing in one area, cryptology, and specifically on using DNA computing to develop a brute-force solution to crack the US government Data Encryption Standard. A second grant that resulted in highly cited research looked at some of the design parameters for building a biomolecular computing system.

***4.3.2 Nanopores for DNA sequencing.*** In the early 1990s, two researchers began exploring the idea of using a nanopore to detect the presence of genetic material.[14] The lead researcher applied for an SGER award to support some of his contributions to joint work on this topic with a post-doctoral researcher. They began working on the development of testing equipment in collaboration with researchers from the US government's National Institute of Standards and Technology. The three researchers demonstrated that they could construct a membrane with nanopores, which could not only detect the presence of RNA and DNA, but also measure polynucleotide length. This provided the foundation for developing nanopores as a new approach to rapid DNA sequencing. At the time of the SGER proposal, the use of nanopores for DNA sequencing had not been suggested in refereed literature. The SGER-proposed idea was borrowed from work on using nanopores for different applications than the ones proposed. As of 1994, the research idea for this project was just emerging, and had not been tried; moreover, the technique envisioned by the lead researchers required knowledge of chemistry and physics, meaning that the proposal relied heavily on interdisciplinary knowledge. The NSF programme manager recognized that the benefits of a successful project could be substantial, and indeed could reduce the time and cost associated with DNA sequencing by 'several orders of magnitude'. Such a technology would revolutionize biotechnology research and supplant the long-standing 'Sanger' method of DNA sequencing.

The SGER-based project was successful. The resulting paper is highly cited, and the citing works span a number of disciplines, beyond physics and biochemistry, to include materials science, engineering, mathematics, and other fields. The work led to an active research community focused on nanopores for biosensing and to further grants made through more traditional peer-reviewed processes. At the time of the writing of the evaluative report, the process was believed to enable researchers to achieve the goal, set by the National Institutes of Health, of sequencing an entire mammalian gene for less than $1,000, as opposed to the current costs of $100,000 or more.[15]

***4.3.3 Influence of gene expression on honeybee behaviour.*** At the time of writing an SGER proposal, the lead researcher in this project had hypothesized that gene expression in the brain of bees might be connected to brain structure, and therefore to behavioural changes in the bee colony. Dr. Robinson used the candidate gene approach to identify the period gene as one that might be involved in this process. As there was no existing experimental basis for this supposition, and since the researchers did not have access to genomics technology, he realized that the supposition was tenuous. The SGER grant contributed to the initial work of establishing a correlation between circadian rhythms in bee behaviour and changes in the period gene.[16]

At the onset of the SGER process, the researcher was unsure about whether the hypothesis was valid. One question that came to mind was why no one had ever tried this particular experiment. The researcher was deterred from making a full proposal by the fact that the honeybee was not viewed as a viable organism for genomic analysis, which meant that it could not be used in any genome-specific funding sources at NIH—or so it was thought at the time. Moreover, the proposal was not based on existing theory, making it more likely to be rejected under traditional peer review.

With the SGER award, the researchers spent time learning about microarrays. The researcher consulted with other genomic experts and created a new research approach based on creating an Expressed Sequence Tag database from bee brains. This research required the integration of research methods and theories from both genomics and behavioural studies. The work initiated with the SGER grant eventually resulted in additional funds in this area and the results were published as a cover story in *Genome Research* and later in *Science* magazine. The work has formed one of the foundations of the field of sociogenomics.

***4.3.4 Social research in virtual environments.*** Two young researchers who studied behavioural and psychological responses to threatening and challenging situations began exploring the use of experiments with virtual reality. They saw an opportunity to use virtual environments as a way to immerse test subjects in different types of challenging situations and gauge their responses. SGER was one of the first grants given to these researchers to begin this work on the use of virtual environments as a tool for social research. The initial SGER proposal fit the guidelines in several ways. The primary feature was the application of a technology developed outside of the researcher's discipline (virtual reality) as a research tool in his own field of research (challenge and threat). If successful, the efforts in the empirical assessment of the value of immersive virtual environments as a methodological tool in social psychology offered a foundation for new forms of experimentation in psychology.

The articles resulting from this initial work on the topic of virtual environments as a research tool in psychology have been cited numerous times by subsequent authors. In particular, the publication of the articles has influenced research in fields outside of psychology. Moreover, the researchers received at least three subsequent NSF grants to extend their research on virtual environments, and graduate students and post-doctoral researchers who worked on the SGER project are active contributors to the field.

***4.3.5 Total internal reflection fluorescence microscopy.*** Beginning in the 1970s, a researcher who would be supported by an SGER grant had studied the process by which cells move molecules across their membranes. The researcher was limited in the ability to expand the research because the existing imaging technologies could not provide enough resolution; thus the research was shelved for a time. In the mid-1990s, the invention of 'total internal reflection fluorescence microscopy' (TIRFM) offered a new approach to study this phenomenon. (TIRFM was a technology originally developed in the field of physics and first applied to biological research in the early 1990s.) The researcher applied for a SGER grant to study new applications of TIRFM in exploring cell signalling and protein–protein interactions. The proposal was focused on taking research tools that were well understood and had been developed in one discipline and applying them to another. The researcher and the programme manager believed that the technique offered some promising advantages over more conventional technologies when studying the movement of molecules across membranes. The SGER-funded research and follow-on work produced multiple articles on processes using TIRFM to observe subcellular processes that were not observable using conventional fluorescence.[17] The researcher was awarded a follow-on NSF and NIH peer-reviewed grants.

### 4.4 Second-generation citations to key SGER-funded research

A number of researchers responding to the PI survey reported that the SGER-funded work had been published in refereed journals and was subsequently highly cited. A review of a random sample of these projects showed that indeed the scientific contributions measured in citation impacts of the SGER-supported projects were considerable. The list in the Table 1 shows the impacts of specific papers resulting from SGER-funded research that were then cited in other work, demonstrating the high-leveraging effect of this research.

### 4.5 Patentable products

A number of SGER projects have produced significant outcomes with benefits beyond transformational scientific discoveries to include patentable products. In particular, a number of projects led the PIs to develop technologies with commercial and social value, including: a technology for adaptive optics that was subsequently used in telescopy, retinal imaging, and secure communications; 'microneedles' for transdermal drug delivery; and 'focal plane array' infrared spectrometers for improving the detection of chemical and biological agents.

***4.5.1 Unexpected benefits.*** SGERs could be given to researchers who belonged to under-represented groups, and it appears that in the case of women this has had a significant pay-off. Among the highest performing scientists emerging from the programme's rosters are several women who now rank among the most highly cited scientists in their fields. Advances in nanoscale science and technology, biomolecular genetics, parallel computing, genetic sequencing, and high-powered lasers, as well as advances in ecology, psychology, and philosophy, can trace their origins to a high-risk grant given under this programme. (We were not able to test for assistance to racial minorities.)

A number of cases of transformative work funded by the SGER mechanism involved PIs who were in the middle of their careers and who were in the process of changing their research focus or exploring new approaches to existing research interests. In other cases, SGER funding supported PIs and projects which did not lead directly to transformative results, but which opened up an area of research that became highly productive.

SGER has also been used in a few cases to enable research grants to be given as a rapid response to an event or opportunity. These special solicitations were made because the time involved in writing, peer reviewing, and issuing a grant through NSF channels is too time-consuming for rapid response. When an event occurred where data collection opportunities would be lost if action was not taken quickly, the SGER mechanism was used to allow for immediate action. This occurred in the case of Hurricane Katrina, the terrorist attacks of 11 September 2001, and several natural disasters. The grants made as part of a solicitation were a small percentage of all grants given under SGER.

The outputs and outcomes of the SGER projects are extremely varied, even more than anticipated in the list of outputs and outcomes shown in the logic maps. The survey asked PIs to provide information about outputs and outcomes, with questions that elicited information about expected and unexpected findings and outcomes related to:

- The research plan: faulty reasoning or other problems with the plan, refinement of research questions.
- Contributions to knowledge: preliminary findings about novel/untested ideas, new avenues of research/new hypotheses; sufficient data collected for use in a follow-on proposal; development of new techniques/tools/instruments or modification of existing ones; rapid and innovative research advances; potentially transformative findings; data collected at a disaster or other situation where a quick response was essential.
- Dissemination of findings: new database available to other researchers; supplement to/enhancement of existing database; published books/articles; patent applications; dissemination to the public or to professional communities at meetings, conferences and workshops.

**Table 1.** Examples of second-generation citations to key SGER-funded research findings

| Direct Citations to the SGER-funded Research as Published (from date of publications up until 2009) | Citations to the Articles Citing the Original Article (second-generation citations) |
|---|---|
| 294 | 14,063 |
| 38 | 934 |
| 50 | 374 |
| 172 | 2,650 |
| 342 | 12,655 |
| 75 | 1,516 |
| 61 | 1,014 |

### 4.6 Policy implications of findings on SGER-funded research

As noted, SGER grants resulted in some spectacularly successful, highly transformative research results. The rate of spectacular success—creating whole new fields of scientific inquiry—was the result of more than 10% of the SGER awards. In fact, the percentage of successes is higher than one might wish from a risk-oriented programme. To set a baseline, consider the recommendation made by the National Academies in their 2007 report on high-risk research: that about three percent of research projects would be expected to have transformative results. Thus it can be argued that SGER was underutilized, and that NSF programme managers did not make broad enough use of the SGER option for funding more trials that did not result in transformative research. The same observation can be made about the overall usage of the programme as an alternative outlet. NSF's own study showed the SGER was being underutilized in percentage terms of funding opportunities, where much more money could have been spent on exploratory grants.

A review of the case studies above and other projects with a significant indication of 'transformative' characteristics yields several observations about the process for identifying 'potentially transformative research' in the context of the SGER funding mechanism.

***4.6.1 Difficulty in predicting the transformative potential of proposed research.*** This study indicates that NSF programme officers cannot rely on the statements of PIs themselves regarding whether or not their research proposals are 'potentially transformative'. One particular problem is that PIs may not recognize that their research is potentially transformative during the proposal stage. Transformation may be the result of serendipity, in that the researcher stumbles upon a discovery by chance; or it could be due to the fact that the researcher may not comprehend fully the implications of the research proposed. This would be true in particular for research in emerging fields where the researcher lacks sufficient understanding of the research to extrapolate its possible consequences, and where the field itself is evolving during and after the project. Thus, judgment on the part of the programme manager is highly influential in making full use of this kind of programme.

***4.6.2 Trans-disciplinary versus interdisciplinary research.*** A number of the transformative research projects seem to be trans-disciplinary rather than interdisciplinary in nature. The term 'trans-disciplinary' in this context refers to cases where the researcher proposes to borrow concepts or approaches from one discipline and apply it to his or her own research topic. In contrast, 'interdisciplinary' typically denotes areas of research where two or more separate disciplines converge. Trans-disciplinary research approaches may produce significant results because they bring new perspectives and new tools to the study of problems in an existing discipline. The resulting capacity to 'see differently', compared to traditional approaches, is associated with increased creativity and innovation in research (c.f. Seely-Brown 1997).[18] More of the SGER successes were transdisciplinary, suggesting that programme managers could make a point of looking for these types of research opportunities as having potential to be transformative.

***4.6.3 Transformative research and career status of the PI.*** A number of cases of transformative work funded by the SGER mechanism involved PIs who were mid-career and were in the process of changing their research focus or exploring new approaches to existing research interests. For example, one researcher used SGER funding to apply virtual reality technology to an area where he had an established track record of research—the psychological response to challenging or threatening situations. This exploration was highly fruitful for this researcher. Similarly, another researcher had already conducted preliminary work on the relationship between neurochemistry and bee behaviour, and the SGER funds provided resources for him to validate his belief that gene expression had an effect on social behaviours. Both researchers had received NSF funding for their prior work, but the SGER allowed them to take that research in very new directions. This finding suggests that programme managers might seek opportunities to help mid-career researchers to branch into an exploratory area of research.

***4.6.4 SGER funding as an enabler of downstream transformative research.*** In some cases, SGER funding supported PIs and projects which did not lead directly to transformative results, but which opened up an area of research that later became transformative. For example, one researcher did not actively pursue his initial findings on DNA computing, but his work inspired others in the field to create a new research community around the topic. This later work was highly successful. Another researcher developed work from within the physics community, and the results led to the creation of a tool that contributed to a fundamental breakthrough in AIDS research. In these cases, SGER funds served as an enabler of first-step exploratory and provocative work, contributing significantly to research that became transformative. This suggests that programme managers may wish to seed more tool-based exploration and diffusion of knowledge.

Small grants for exploratory research as a programme was underutilized by NSF programme managers, and even then, created significant and transformative results. Findings show that ideas, which resulted from a series of discussions between an investigator and a knowledgeable programme director, were the most fruitful among the grants given. Within this set of activities, those that were most likely to be transformative were given to mid-career men who wanted to explore a new sub-field of research, or to early career women with highly promising ideas. The model of providing small grants to exploratory research proves to be a useful model to create opportunities for transformative research when used by research agencies seeking to fund important developments in science.

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## Notes

1. The original evaluation was conducted by SRI International's Center on Science Technology, and Economic Development under contract to the National Science Foundation. The original study was conducted between 2007 and 2009.
2. Early-concept Grants for Exploratory Research (EAGER).
3. Grants for Rapid Response Research (RAPID).
4. A description of NSF's peer review process can be found at http://www.nsf.gov/bfa/dias/policy/meritreview/.
5. In the M-09-27 4 August 2009 Office of Management and Budget Memorandum to research agencies the following guidelines were offered: 'In their budget submissions, agencies should describe the expected outcomes from their research in relation to these four practical challenges and cross-cutting areas, providing quantitative metrics where possible, and describe how they plan to evaluate the success of various techniques to increase support for high-risk research.'
6. The DOD has sponsored high-risk research through the Defense Advanced Research Projects Agency (DARPA) for more than two decades.
7. The DOE initiated the Advanced Research Projects Agency-Energy (ARPA-E) in fiscal year 2009.
8. The National Institutes of Health, an agency of DHHS, instituted the Transformative R01 Program in fiscal year 2009.
9. The literature on peer review and risk in science was recently reviewed in Luukkon 2012.
10. NSF Staff Memorandum, *Small Grants for Exploratory Research*. 14 August 1989. O/D 89-11.
11. The Small Grants for Exploratory Research issued some solicitations during the period of its operation. These solicitations were designed to encourage the research community to respond to urgent or timely events as the data collection needed to take place quickly.
12. *Report to the National Science Board on the NSF's Merit Review Process Fiscal Year 2005. March 2006*. Arlington VA: National Science Board (NSB-06-21).
13. For the purposes of this study, we focused on cases where the SGER award fit into one or more of the first four categories—not those addressing the need for urgent funding. While an 'urgent' award might also be potentially transformative or exploratory, that is not the goal of these funds, so we judged that those awards are best viewed through a different evaluation process.
14. Harvard's Daniel Branton Discusses Building a Nanotube Detector for Nanopore Sequencing. *In Sequence*, published at http://www.genomeweb.com, 22 April 2008.
15. Branton et al. (2008).
16. Toma et al. (2000).
17. See 'Protein highway', 25 April 2003, p. 1–2.
18. John Seely-Brown (1997).

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