Fostering Transformative Research in the Geographical Sciences

DETAILS

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Committee on Identifying Transformative Research in the Geographical Sciences; Geographical Sciences Committee; Board on Earth Sciences and Resources; Division on Earth and Life Sciences; National Academies of Sciences, Engineering, and Medicine

PREPUBLICATION

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Although the reviewers listed above provided many constructive comments and suggestions, they were not asked to endorse—nor did they see—the final draft of the report before its release. The review of this report was overseen by George M. Hornberger, Vanderbilt University and Robert B. McMaster, University of Minnesota, who were responsible for making certain that an independent examination of the report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the Academies.

PREPUBLICATION

CONTENTS

SUMMARY

The central purpose of all research is to create new knowledge. In the geographical sciences this is driven by a desire to create new knowledge about the relations between space, place, and the anthropogenic and non-anthropogenic features and processes of the Earth. But some research goes beyond these modest aims and creates new opportunities for further research, or affects the process of knowledge acquisition more broadly, or changes the way other researchers in a domain think about the world and go about their business. In such instances, research is capable of transforming an entire field of research. Because of its positive impacts, transformative research can be regarded as inherently having greater value than more conventional research, and funding agencies clearly regard transformative research as something to be encouraged and funded through special programs. Assessments of transformative research funding initiatives are few and provide a mixed picture of their effectiveness. The challenge, and central question posed to the committee that authored this report, is whether transformative research can be identified at the time it is proposed rather than after it has been conducted, communicated, and its influence on the discipline has become clear.

Complicating this question is the fact that no single definition of transformative or high-risk, high-reward research exists, or is likely to emerge in the near future. The definition most relevant to this study is perhaps the 2007 National Science Board definition: "Transformative research involves ideas, discoveries, or tools that radically change our understanding of an important existing scientific or engineering concept or educational practice or leads to the creation of a new paradigm or field of science, engineering, or education. Such research challenges current understanding or provides pathways to new frontiers." The language of the definition includes references to the results of transformative research (discoveries, creation of a new paradigm or field of science) but also to the inputs to such research, such as the tools used. What is transformed can include current understanding, an existing concept, or existing practice.

At the request of the National Science Foundation, the National Academy of Sciences established a committee to provide insight into how transformative research in the geographical sciences evolved in the past so that it can be encouraged in the future. The charge asks the committee to take an historic approach by reviewing how transformative research has emerged in the past, what its early markers were, and how it can be nurtured in the future (see Box 1.1 for the complete statement of task). To carry out its charge, the committee gathered information from a broad cross-section of the geographical sciences and affiliated disciplines as well as experts in assessing research outcomes via a workshop, an on-line questionnaire, and a review of relevant literature (see Appendix B for a complete list of contributors).

Because the primary instigator of the study was the National Science Foundation (NSF), and because NSF's primary program for funding research in the geographical sciences is the Geography and Spatial Sciences (GSS) program of the Social, Behavioral, and Economic Sciences directorate, the committee chose to express many of its conclusions in the form of recommendations to that program. Nevertheless many of the findings and recommendations will be relevant to the work of other agencies, institutions, and individuals.

The committee took a three-part approach in developing their recommendations for fostering transformative research. First, they explored the concept of transformative research in detail, examining some of the definitions that have been advanced, related terms, and funding programs that have been aimed at stimulating, encouraging, and fostering transformative research. Second, they examined the history of new research directions that have emerged in the geographical sciences over the late 20th and early 21st centuries as a result of a variety of transformative stimuli. These were considered in the context of a general model of innovation diffusion and the general lessons they might provide on research innovators and the successful diffusion of ideas. Lastly, to translate their findings from the historical review into a modern context, the committee examined the current climate for research within the U.S., especially with respect to funding, and compared it to the situation in the past and in other countries.

RECENT TRANSFORMATIVE INNOVATIONS IN THE GEOGRAPHICAL SCIENCES

To examine the development and diffusion of transformative ideas in the geographical sciences, the committee reviewed five areas of geographical research that can be argued to have been transformative over the past 50 years. These five examples are identified under the broad rubrics of Political Ecology, social theory that is directly informed by and related to geography and referred to here as Spatial Social Theory, Remote Sensing of the Environment, Geographic Information Sciences (GIS), and Global Climate Change. The inventors and early innovators, sources of ideas, and stimuli responsible for the development and widespread diffusion of these transformative research areas within and beyond the geographical sciences were considered in the context of the general innovation-diffusion concepts.

In all five case studies the subsequent diffusion and further development were parallel and complementary processes that were facilitated by a number of mechanisms. Face-to-face meetings and direct communication between developers and early adopters through a series of workshops, symposia, and formal research groups were a feature of the diffusion of spatial social theory and political ecology, while large official steering committees and semi-permanent governmental agencies were not. In remote sensing and GIS, early symposia were also important, and some of these have become institutionalized as regular formal events of considerable size. They also benefitted from the establishment of large national programs for research funding and knowledge diffusion. In the early 1990s, vigorous debate over whether GIS was merely a tool or technique versus a set of principles, knowledge, and theory helped raise the profile of GIS and establish it as a substantial intellectual domain. Global climate change has disseminated through a wide variety of disciplinary and multi-disciplinary journals and books as well as regular high-profile reports from the Intergovernmental Panel on Climate Change and the U.S. Global Change Research Program. Many of these publications garner large amounts of attention in the popular press that develop public awareness and draw in young people to the research community through this exposure. Like GIS, global climate change has also diffused broadly through high-profile debates. In addition to these observations, the committee developed several overarching findings during their case-study reviews:

Finding 1: Transformative innovations can arise from a wide variety of individuals and groups, from a wide variety of intellectual sources, including older and long-ignored ideas, and through revolutionary and evolutionary paths.

Finding 2: An open innovation system in academic science and research can encourage the exchange of information even between competing groups, and helps to achieve the desire of the nation, funding agency, or foundation for the most rapid, productive, and efficient academic research sector.

Finding 3: The promotion of rapid communication amongst innovators and adopters is critical for development as well as for the diffusion of transformative innovations.

Finding 4: There are no established indicators that would identify specific individuals or concepts as sources of transformative innovation prior to the conduct of research.

The committee also observed that research initiatives often did not originate in the geographical sciences, but the geographical sciences have come to play a key role and, in some cases, to assume a lead position in research. For example, the priming agents in the rise of political ecology spanned the realms of ideas, technology, and societal need, and were influential in many disciplines besides the geographical sciences.

THE CURRENT CONTEXT

The committee emphasized in the previous section that the source of innovation is often immaterial, but context is everything. If context is everything, then the potential for future transformations in the geographical sciences must be placed in a contemporary context and the committee sees the American research enterprise as currently facing four challenges, to which a new emphasis on transformative research may be a logical response:

1. Federal research and development (R&D) funding levels are likely to decline, or at least remain stable, in the near term. Competition for scarce resources will pit existing programs and institutions against revolutionary developments.
2. After three decades of growth, state-level funding for R&D has stabilized and is in decline in many states.
3. In the near term, demographics point to a proportionately smaller cohort of individuals available to pursue undergraduate education. Pursuit of graduate education is responsive to levels of undergraduate debt, stagnant wages, and uncertainty over future investments in further education.
4. Developing countries are building educational systems capable of supplying the skilled labor that is required to attract R&D investments. The U.S. system of R&D and higher education now has rivals.

FOSTERING TRANSFORMATIVE RESEARCH TODAY

The previous sections have used five examples to trace the history of transformation in the geographical sciences, and discussed the critical importance of transformative research to the U.S. economy at a time when research funding and higher education face severe and largely unprecedented challenges. In this final section the committee presents ideas and recommendations for fostering transformative research in the geographical sciences, with specific reference to the Geography and Spatial Sciences (GSS) program of NSF.

Initiatives in Education

Recommendation 1: *GSS should examine the degree to which its awards, especially those in support of geographic education, foster the potential for transformative research among the students who benefit from these awards, and encourage principal investigators (PIs) to give attention to such potential in their proposals.*

For funding agencies like the NSF, awards might be seen as opportunities for fostering transformative research by engendering the kinds of critical, creative, and independent thinking that such research requires. PIs might be encouraged to address and even emphasize relevant characteristics in their proposals such as:

- How and to what degree will students supported by the proposal be exposed to the concept of transformative research?
- How and to what degree will students supported by the proposed award be encouraged to think critically, creatively, and independently?
- How and to what degree will they be exposed to the nature and impacts of prior transformative research in the geographical sciences?
- How and to what degree will students with backgrounds in other disciplines be encouraged to learn and apply the perspectives of the geographical sciences to problems?
- How and to what degree will students with backgrounds in the geographical sciences be encouraged to learn and apply the perspectives of other disciplines to problems?
- How and to what degree will students with backgrounds in the geographical sciences be encouraged to apply the perspectives of geographical research to problems in other disciplines?

The Research Culture

Recommendation 2: *GSS should continue to emphasize NSF policies and programs that are designed to increase ethnic, age, and gender diversity among its awardees.*

The geographical sciences are already a multidisciplinary culture, in which collaboration across the boundaries of the traditional disciplines is not only common but encouraged, and in which transformative ideas have often stemmed from such collaboration. However, the community of researchers in the geographical sciences falls a long way short of "looking like

America," despite NSF's strenuous and longstanding efforts in this direction. The representation of women, GBLT, and ethnic minorities still varies across the various areas of the geographical sciences, despite the potential for such groups to bring new, transformative ideas to the research table. NSF might do more to encourage collaboration across differences of ethnicity and gender, and between experienced researchers and early-career faculty.

Recommendation 3: *In the interests of fostering transformative research, GSS should also recognize the importance of research collaboration between nations, between disciplines, and between academics, industry, government, and the military and intelligence communities.*

While collaboration between U.S. and European researchers is common and such efforts are also increasing with researchers in China, collaboration with many other parts of the world remains adversely impacted by differences in language and research culture, problems with travel and communication, personal security, and the lack of bilateral or multilateral funding programs. Yet, such collaborations could be enormously stimulating and bring a host of new ideas and perspectives. Much could also be done to foster increased interaction with industry, the military, and the intelligence community. Exchange and internship programs could provide increased opportunities for exposure to new and potentially transformative ideas. NSF could also encourage PIs to increase interaction internationally beyond Europe and China and to include representatives of these communities on advisory boards, at workshops, and in webinars.

Career Advancement

The traditional methods of evaluating candidates for academic appointments and for career advancement may not provide the best indicators of potential for transformative research. Recently the process of research evaluation has become even more quantitative, through the use of readily available bibliometrics, and less dependent on the independent, detailed, and qualitative evaluation by a candidate's immediate peers. Evaluation based on bibliometrics may encourage a candidate to partition contributions into least-publishable units, to increase the number of co-authors wherever possible, and to place emphasis on journals with high impact factors instead of those most likely to communicate results to the most interested colleagues. Referring to one of the most important advances in computer science in recent years, as measured by its impacts on society, the 2014 NRC report Furthering America's Research Enterprise noted: "Bibliometrics, for example, would not have flagged the supporting citations in the patent application for (Larry) Page's Google search algorithm as particularly high impact during the years surrounding the initial appearance of those publications."

Career advancement practices may also emphasize individual activity at the expense of collaboration. Junior faculty who build collaborations with other disciplines, and benefit from the stimulus and cross-fertilization that result, may in the end be penalized in a discipline-centric system. Organizing workshops, building networks of colleagues, and pursuing large awards of external funding are all significant contributions that can foster transformative research, but all are discouraged at early career stages when the potential for truly original ideas and discoveries can be highest.

As noted earlier as Finding 4, research to date has not been able to discover characteristics capable of predicting the likelihood that an individual will produce transformative research. At this time, therefore, the committee chooses not to make a recommendation on the individual characteristics GSS might look for should it wish to encourage transformative research. Nevertheless, and despite the current lack of solid supporting research, Chapter 2 ends with the committee's consensus view on the individual characteristics likely to be conducive to transformative research.

Funding Practices

Recommendation 4: *In the interests of being more supportive of transformative research, GSS should work with other groups within and beyond NSF to explore and evaluate the novel approaches to research funding and proposal review discussed in this section.*

The committee has suggested that some of the pressure for increased emphasis on transformative research stems from a belief that the processes of proposal review are essentially conservative, working against projects that might involve high risk, but might offer the potential for high return. Thus, one way to encourage transformative research might lie in a review and perhaps revision of the proposal process.

Conclusion

Although there is no single, succinct, and all-encompassing definition of transformative research, two distinct themes emerged from the committee's information gathering. First, transformative research has unusually high value or return that may be reflected in a variety of ways: the widespread redirection of research in an existing research community, the formation of a new research community or discipline, or the emergence of a new industry. The five case studies discussed in this report provide ample evidence of this high value or return in the case of the geographical sciences. Second, transformative research carries unusually high risk to a funding agency, because its groundbreaking nature is difficult for principal investigators to visualize and for reviewers to evaluate.

The rational response to this duality is to maximize the return while minimizing the risk. The committee used a model of innovation diffusion to frame a discussion of the factors that may be helpful in maximizing return: open sharing of ideas, rapid dissemination, and the breaking down of institutional barriers that include the disciplinary stovepipes of academia. The committee extended these ideas in the specific context of the geographical sciences and the National Science Foundation's Geography and Spatial Sciences program, and also made recommendations designed to minimize risk. These include finding better ways of preparing young geographical scientists for transformative research, identifying ways in which the research culture of the geographical sciences can be made more conducive to transformative research, addressing aspects of the process of career advancement that inhibit transformative research, and exploring novel approaches to the development and review of proposals for funding transformative research.

None of these recommendations can directly address the concerns raised in this report on the current context and the overall state of national science policy and performance. The geographical sciences are a very small part of the broader research enterprise, and though strides have been made in recent decades in achieving greater prominence for the discipline's central ideas, these ideas will almost certainly remain close to invisible in the national debates over the four challenges elaborated in this report. Nevertheless, within the context of the geographical sciences the four recommendations herein do specifically address the four national research challenges articulated in this report. The declining levels of national and state research funding (Challenges 1 and 2) can partially be offset through the development of more linkages and programs with the private sector and research partnerships with governmental agencies (Recommendation 3). This has been effective in the past growth of GIS and remote sensing as transformative sciences and will likely be just as effective and even more necessary in the future. The fostering of an open and collaborative system of innovation development and diffusion (Recommendation 3) will serve to help counteract the potentially stifling impact of competition for scarce research dollars (Challenges 1 and 2). Encouraging transformative research and targeted funding at the late inception/early diffusion stage can help to maximize governmental investment success and to offset overall reduction in funds. Targeting specifically the training of students in the nature and achievement of transformative research (Recommendations 1 and 2) will help to make sure that a larger proportion of highly educated individuals has the capacity to advance the geographical sciences and to offset both the potential declines in the absolute numbers of such highly trained individuals and the increasing competition by such students trained in other countries (Challenges 3 and 4). Increasing the diversity of the research community (Recommendation 2) serves not only to bring the wider range of perspectives that is important for the recognition of transformative research opportunities, but also serves to increase the pool of students for higher education (Challenge 3). In addition, greater diversity can promote increased engagement with the international research community and the exchange of ideas at the innovation and early diffusion stage. Finally, novel approaches to proposal review (Recommendation 4) have the potential to foster transformative research and thus ultimately to address Challenges 1, 2, and 4.

1

WHAT IS TRANSFORMATIVE RESEARCH?

The central purpose of all research, whether basic or applied, is to create new knowledge; research in the domain of the geographical sciences is driven by a desire to create new knowledge about that specific domain; that is, about the relations between space, place, and the anthropogenic and non-anthropogenic features and processes of the Earth. But some research goes beyond these modest aims, by having impacts that extend well beyond them. Some research creates new opportunities for further research, or affects the process of knowledge acquisition more broadly, or changes the way other researchers in a domain think about the world and go about their business. In such instances research is capable of transforming a field of research. Thus the very concept of transformative research is self-referential: by a process of positive feedback, a specific research effort can have impacts on an area of research that are much greater in magnitude than might normally be expected.

Transformation implies the existence of something to be transformed, and suggests Kuhn's concept of *normal science*: a steady state in which science proceeds by continuous, incremental accumulation of knowledge (Kuhn, 1962). Transformative research introduces a phase of what Kuhn termed *revolutionary* science, in which normal science is interrupted, disrupted, or transformed by new ideas, new technologies, or new questions. Many have argued that Kuhn's two-phase model is overly simplistic, implying for example that large areas, or even all of science, are interrupted by these transformative events; and of course like all models it does simplify. But there is no doubt that new research directions have emerged in the geographical sciences from time to time as a result of a variety of transformative stimuli. In Chapter 2 of this report the committee focuses on several of these examples and presents a series of committee findings regarding how transformative research has developed in the past. Chapter 3 examines the current climate for research within the U.S., especially with respect to funding, and compares it to the situation in the past and in other countries. It argues that a new emphasis on transformative research may be a logical response to shrinking funding and increasing international competition. In Chapter 4 the committee makes recommendations in several areas for fostering transformative research.

Because of its positive feedbacks and impacts, transformative research can be regarded as inherently more valuable than more conventional research, and in this chapter the committee cites examples of funding agencies that clearly regard transformative research as something to be encouraged and funded through special programs. This chapter also explores the concept of transformative research in detail, examining some of the definitions that have been advanced, related terms, and funding programs that have been aimed at stimulating, encouraging, and fostering transformative research. Thus the chapter provides the foundational context for the later sections of the report.

DEFINITIONS

In 2007 the National Science Board issued a report "Enhancing Support of Transformative Research at the National Science Foundation" (National Science Board, 2007). It adopted the following definition: "*Transformative research involves ideas, discoveries, or tools that radically change our understanding of an important existing scientific or engineering concept or educational practice or leads to the creation of a new paradigm or field of science, engineering, or education. Such research challenges current understanding or provides pathways to new frontiers." The language of the definition includes references to the results of transformative research (discoveries, creation of a new paradigm or field of science) but also to the inputs to such research, such as the tools used. What is transformed can include current understanding, an existing concept, or existing practice.* The definition is lengthy, and clearly attempts to be as inclusive as possible, using "or" no less than seven times.

In 2012 NSF sponsored a further workshop on "Transformative Research: Ethical and Societal Implications" (Frodeman and Holbrook, 2012). Chapter 3 of this report expands on this broader context for transformative research in the U.S. and its current importance, situating it within the major trends and developments that are impacting higher education, the research enterprise, American competitiveness, the level of funding for research, and the intensity of competition between researchers for that funding.

In the UK the Economic and Social Research Council (ESRC), roughly equivalent to NSF's Directorate for the Social, Behavioral, and Economic Sciences, settled on the following definition of transformative research: "research ideas at the frontiers of the social sciences, enabling research which challenges current thinking … We regard transformative research as that which involves, for example, pioneering theoretical and methodological innovation … novel developments of social science enquiry … an element of risk." While the definition may seem somewhat more succinct, especially when quoted selectively as here, one should note that it has been constructed specifically for the domain of the social sciences, and may need to be broadened if it is to be applied to all of the domain of NSF, or even that of the geographical sciences, which include physical geography.

RELATED DEFINITIONS

The theme of transformative research as inherently risky provides a link to several other closely related definitions. The National Institutes of Health recognizes "high-risk, high-reward" (HRHR) research. In the words of the Director, Dr. Francis Collins, "High-risk research isn't for the faint of heart. It's for fearless researchers who envision and develop innovative projects with unconventional approaches that, if successful, may yield great leaps in our understanding of health problems and/or biological mechanisms. It takes nerve and creativity to conceive such projects—and, often, special support to bring them to fruition. And, as the name implies, there is a significant chance of failure." (Collins, 2013). The European Research Council (ERC) prefers the term "frontier research", which "reflects a new understanding of basic research. On one hand it denotes that basic research in science and technology is of critical importance to economic and social welfare. And on the other that research at and beyond the frontiers of understanding is an intrinsically risky venture, progressing in new and the most exiting (*sic*) research areas and is characterised by the absence of disciplinary boundaries" (European Research Council, 2015).

In summary, no single definition of transformative research exists, or is likely to emerge in the near future. The NSB's definition of transformative research has clearly tried to accommodate all domains of science, technology, engineering, and mathematics (STEM), and to include all of the many ways in which research progresses. There is a temptation, therefore, to see the lack of a single, simple definition as a failure of language rather than of the concept the language is trying to capture, and to fall back on "I know it when I see it" (Lal and Wilson, 2013).

FUNDING INITIATIVES

The NSB report discussed earlier recommended a Transformative Research Initiative within NSF that was "distinguishable by its potential impact on prevailing paradigms and by the potential to create new fields of science, to develop new technologies, and to open new frontiers." Subsequently NSF introduced the cross-directorate CREATIV and INSPIRE funding programs specifically aimed at providing opportunities for transformative research, and several awards have been made under both programs.

NIH has four award programs in its HRHR portfolio, all drawing on the NIH Common Fund and thus above the programs of the individual institutes, and all closely associated with the Director: the Early Independence Award, the New Innovator Award, the Pioneer Award, and the Transformative Research Award. The last is described as being "created specifically to support exceptionally innovative and/or unconventional research projects that have the potential to create or overturn fundamental paradigms. These projects tend to be inherently risky and may not fare well in conventional NIH review. As compared to the other NIH Director's Awards … the primary emphasis of the Transformative Research Awards initiative is to support research on bold, paradigm-shifting, but untested ideas."

The reference to not faring well in conventional NIH review suggests a possible motivation for such funding programs: the belief that the conventional review process is inherently conservative, favoring normal science over revolutionary science. According to this view reviewers tend to assess proposals against conventional practices, and may be unable to appreciate the potentially transformative aspects of new ideas. They sometimes tend to react somewhat negatively to what are seen as "trust me" proposals that lack the kinds of methodological detail needed to make an independent assessment of viability. And perhaps reviewers sometimes overlook the fact that the results of research are inherently, and by definition, unknowable. Thus support for transformative research has sometimes been seen as requiring a novel approach to the review process. The CREATIV program at NSF, for example, allows a program manager to make decisions without external peer review on proposals valued at up to $1,000,000, a limit more than an order of magnitude higher than NSF's traditional practices for decisions based only on internal review, and subject to the requirement that at least two directorates be involved.

The ESRC's Transformative Research Call is at time of writing in its third annual cycle, having made two previous sets of awards. It also uses a somewhat unconventional mechanism for review: after an initial assessment by a panel of academic experts, short-listed PIs are invited to present their proposals at a "Pitch-to-Peers" workshop, with awards made shortly thereafter. Arguably peers who have submitted their own transformative ideas may be better able to assess the transformative ideas of others. Other novel approaches include the "sandbox" or "sandpit," in which the generation of novel ideas for research occurs in a workshop setting. Ideas are proposed, discussed among the group, and then voted on by the group, with a reasonable assurance that the sponsoring agency will indeed make an eventual award to the originator without further external review. Another alternative to the all-or-nothing nature of traditional funding practice could be what might be termed “progressive funding”, in which PIs with promising ideas would first be awarded small seed grants through a streamlined review process. If the results were promising, a subsequent proposal could be made for a second, larger phase of funding. Keeping the initial award small would reduce the risk to the agency.

On the other hand ERC’s support for its “frontier research” is obtained through its regular programs: “Support for investigator driven 'frontier' research can be obtained by individual researchers through the European Research Council (ERC)'s competitions for funding.” (European Research Council, 2015).

Any revision in the traditional review process must be considered carefully, lest it lead to a more elitist, top-down pattern of funding at the expense of the broader community. To date such revisions have involved only comparatively tiny fractions of the total funding stream, and many, such as “Pitch to Peers”, might be argued to be more community-based than traditional practices, by involving a larger panel. Nevertheless funding agencies would be wise to anticipate push-back from some sectors of their research communities if there is evidence that transformative research is being funded at the expense of normal science.

Many other agencies have developed various activities designed to encourage and support transformative and HRHR research in their respective domains. The details of these similar programs are not covered here; they include the Department of Defense’s Advanced Research Projects Agency, the Office of the Director of National Intelligence’s Intelligence Advanced Research Projects Activity, and the National Institute of Standards and Technology’s Technology Innovation Program. Other comparable programs exist in the national laboratories and in the private sector.

PROGRAMMATIC ASSESSMENT

With substantial efforts being made to promote and encourage transformative research, and substantial (though small in proportion to the total funding stream) sums being awarded to projects, there is clearly interest in assessing the efficacy of such programs. According to Johnston and Hauser (2008), “We have almost no information about what predicts transformation. Who are these people who go on to produce transformative studies and win prizes like the Nobel and the Lasker? Are they particularly ambitious, hard-working, smart, creative, or just lucky? Are they triple threats[1], or do they focus tightly on the mission at hand? Similarly, do we have any hope of identifying transformative projects in advance or do they really arise from good fortune, hard work, and resourcefulness? How important is environment? Do these discoveries come from working in isolation or from applying advances in other areas to a whole new problem? It seems particularly odd that the predictors of transformative research are completely unstudied.” These questions bear a remarkable similarity to the three questions of the committee’s charge (see Box 1.1 later in this chapter). The question of whether “the predictors…are completely unstudied” is addressed further in Chapter 4.

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[1] A term originating in sports writing denoting someone who is an expert in all of the skills (three in the case of football) required by a game or sport.

Lal and Wilson (2013) report on efforts to address some of these questions through systematic, quantitative research. In work partly supported by NIH they analyzed 35 of the awards given in the first three years of HRHR funding, using a variety of data sources including publications resulting from the work and biographical characteristics of the principal investigators (PIs). Examining potential predictive indicators of success in transformative research, they found that:

- "Researchers conducting transformative research tend to have a similar number of publications as compared with similarly excellent researchers;
- Researchers conducting transformative research tend to be no younger as compared with mainstream researchers;
- Transformative research is perceived to be risky by peers, but the degree of risk at time of award does not seem strongly associated with impact years after award; and
- Transformative research tends to be no more interdisciplinary or collaborative than similarly excellent research."

Note, however, that their null hypothesis was accepted for a small sample (35 in this case) and may have been rejected for a larger sample (in other words, these results, which are dominated by acceptance of the null hypothesis, may reflect Type II statistical errors). In the committee's view these results concerning pre-award indicators are as yet inconclusive because of the small sample size, the very broad basis of the questions being asked, and the limited domain of research that was sampled. The conclusions and recommendations presented in Chapter 4 balance this provisional evidence of the Lal and Wilson study with the views expressed at a workshop organized by this study's committee and by respondents to a questionnaire distributed on-line.

With respect to post-award indicators, their research found that:

- "Transformative research does tend to follow more innovative research approaches as compared with similarly excellent researchers;
- Transformative research tends to have greater impact as compared with similarly excellent researchers; and
- Transformative research garners less disagreement among peers, and does not take longer to be accepted by the community as compared with similarly excellent researchers."

In the case of post-award indicators, then, Lal and Wilson (2013) were able to reject the Null Hypotheses, lending greater weight to their conclusions than in the case of pre-award indicators.

Thus the results are mixed: it appears that proposals that are selected by programs designed to foster transformative research have greater impact, and are more innovative on reflection, than the work of similarly excellent researchers that is funded by more conventional programs. Moreover there is ample anecdotal evidence of the highly controversial nature and long-delayed acceptance of many transformative ideas such as anthropogenic climate change (discussed in Chapter 2), evolution, or continental drift. Further systematic research is clearly needed, using larger samples and addressing problematic cases: where ideas were judged potentially transformative at proposal stage but turned out not to be, and where transformative research was not identified as such at proposal stage. As the committee discusses elsewhere in this report, such evidence would be very useful in addressing the three questions of the study charge.

COMMITTEE APPROACH

The committee was asked to provide insight into how transformative research in the geographical sciences evolved in the past so that it can be encouraged in the future. The charge asks that the committee to take an historic approach by reviewing how transformative research has emerged in the past, what its early markers were, and how it can be nurtured in the future (see Box 1.1). The charge refers repeatedly to the "geographical sciences", so a brief note of clarification might be useful at this point. The committee recognizes the term as overlapping substantially with the discipline of geography, but differing in two respects: first, that not all researchers who identify with geography would be happy thinking of what they do as science, and second that the methods and principles of the geographical sciences are used and advanced in many other disciplines, from engineering to the humanities. The committee uses the term "geographical scientist", meaning someone engaged with the geographical sciences, when it seems appropriate. Finally, the committee also uses the term "geographer", while recognizing that the term is distinctly ambiguous: to be a geographer must one have ones final degree in geography, or work in a department of geography, or is it simply a matter of self-identification? There are no simple answers to these questions, and it is therefore left to the reader to attach whatever meaning the context suggests.

The statement of task instructs the committee to hold a workshop as its principal information-gathering activity. A workshop was held on August 5-6, 2014 in Irvine, CA with approximately 30 invited participants from a broad cross-section of the geographical sciences and affiliated disciplines as well as experts in assessing research outcomes (see Appendix B and C for the contributors and agenda). Two keynote lectures started off the workshop, followed by a series of moderated panels on (1) society, polity, and economy; (2) methods, models, and geographic information systems; (3) environmental sciences; and (4) being transformative. Each panelist provided a short white paper that was distributed to participants prior to the workshop. In developing these papers, the panelists were asked to describe one or two transformations that

BOX 1.1
Committee Statement of Task

Transformative science drives significant advances by providing new theoretical or technical frameworks that re-orient existing fields or even create new fields. Yet, the history of science shows many transformative concepts were very difficult to identify when initially introduced. An ad hoc committee will organize a public workshop as a primary source of information to examine transformative research as it has influenced the evolution of the geographical sciences to provide insight into how transformative research evolved in the past so that it can be encouraged in the future. In particular, the committee will seek insights into three questions noted below, drawing principally on the workshop presentations and discussions:

1. How has transformative research emerged in the past and how did it become transformative?
2. What might be the early markers of transformative research and how does it become possible to identify their transformative character?
3. What has helped nurture and bring transformative research to fruition and how can it be fostered in the geographical sciences?

The committee will not evaluate existing funding and operational programs or make budgetary recommendations.

have occurred in their field of interest and to consider the following questions to whatever extent possible:

1. How did the transformative research emerge and how did it become transformative?
2. What were the early markers of the transformative research and how did it become possible to identify its transformative character?
3. What has helped nurture and bring transformative research to fruition in your field and how can it be fostered?
4. Is there past research that should have been transformative (in your estimation), but in hindsight was not?

The first three questions reflect the committee's statement of task, while the fourth question was intended to encourage panelists to explore the equally important question of why some promising research does *not* become transformative. These white papers provided a jumping off point for discussions as well as a rich source of information for the committee to draw upon as they wrote this report. The first three questions produced useful input to the report. The fourth question proved to be more problematic; while many participants could cite examples from personal experience, nothing emerged in the way of general and useful principles, though there is clearly room for a more extensive investigation of what is surely an important and interesting question. The committee also chose to collect information via a questionnaire that was distributed on-line (see Appendix D) to include the ideas of those who were unable to attend the workshop and to bolster the committee's other information-gathering activities. The questionnaire was not designed for nor subjected to statistical analysis, but the responses were reviewed and considered by the committee in the writing of this report.

2

RECENT TRANSFORMATIVE INNOVATIONS IN THE GEOGRAPHICAL SCIENCES

In this chapter the committee examines selected case studies of major transformative research events in the geographical sciences that occurred over the late 20th and early 21st Centuries. These are considered in the context of general models of the diffusion of innovation and the potential general lessons they might provide on research innovators and successful diffusion.

TRANSFORMATIVE RESEARCH AND GENERAL MODELS OF THE DIFFUSION OF INNOVATION

Many geographical scientists are aware of the treatises by philosophers such as Popper, Kuhn, Lakatos, and Feyerabend on the underlying nature of science and scientific revolutions. There is a long history of examining the geographical sciences in relationship to such philosophical perspectives and debates (e.g., Bird, 1977; Wheeler, 1982; Mair, 1986; Johnston, 1997; Bassett, 1999). Most recently Inkpen and Wilson (2013) provide an examination of physical geography in this context. The philosophical views range from the progression of science conceptualized as a logical and formal process of the construction, testing, and refutation of hypotheses, as espoused by Popper, to the much messier, and perhaps more realistic view of Kuhn and Lakatos that progress occurs by the overthrow of entrenched paradigms and programs and the triumph of competing paradigms. Finally there is the rejection of any real rationality in scientific creativity and progress as argued by Feyerabend.

An alternative way to consider the patterns and processes by which transformations in scientific fields occur is through the lens of Diffusion of Innovation theory. Rogers (1962) conducted seminal work in this field and it has been further developed over the past 50 years (e.g. Rosenberg, 1972; Eveland, 1986; Wejnert, 2002; Rogers, 2003; Hall, 2004; Godin, 2006; Peres, 2010; Wisdom et al., 2014). Research on diffusion of innovation theory has largely been conducted in the context of product development and marketing. Models of innovation diffusion are widely applied to economic sectors such as technology and biomedicine, but these models are also applied in realms such as public policy and education. Economists and the business sector are particularly interested in how innovative new products are optimally developed, refined, and then diffused to acquire maximum market share. The initial creation, further development, diffusion, and adoption of transformative research may be examined in the same manner.

Rogers graphically summarized the innovation diffusion process as the logistic growth in market share as a product is developed and then diffused. Underlying this is a normal distribution of innovators, early and late adopters, and then the final adopters (so called 'laggards') (Rogers, 2003). In the context of transformative research this can be seen as the process by which an idea is created by one person or a small number of individuals, a small number of early adopters then actively refine and develop it, and then it diffuses to other researchers. The idea then gains credence and is applied by the majority of researchers in the field. The remaining laggards come to adopt the transformative research paradigm late in the game, for want of information, lack of capacity to implement it, or due to initial intellectual resistance. In the arena of academic research an important part of this process would be the communication of both the existence of a new research paradigm and knowledge about how to successfully apply it oneself. No matter how appealing an idea or technology might be, if it cannot actually be applied by potential adopters due to complexity or capital costs it is unlikely to be transformative. In the case of pure research the equivalent of market share for a transformative idea might be the numbers of peer-reviewed publications, citations, doctoral students, etc.

Is there any empirical evidence that the adoption of transformative research follows a pattern similar to that proposed by Rogers? Data for a number of recent transformative research areas in the geographical sciences suggest some truth in the general model. Examples drawn from an *n*-gram analysis of published books in the Google database (Lin et al., 2012) all show a logistic pattern of growth in the use of terms related to transformative research paradigms in the geographical sciences (Figure 2.2). Fields such as remote sensing matured and reached a steady state early while areas such as climate change are still increasing (Figure 2.1). Indeed, Rogers' model of innovation diffusion is identical in form to the Isserman Curve of productive scientific inquiry which NSF's Geography and Spatial Sciences program has used in panel assessments (NSF, 2011). In that formulation the curve represents the accumulated knowledge generated by the incipient creation of research ideas through to the end phase of gap filling when further major innovation and generation of significant new knowledge is unlikely (Baerwald, 2013).

What then, in general terms, drives the initial development, refinement, and diffusion of an innovation? In the context of marketing the impetus for innovation and diffusion has been characterized in terms of Technology Push, where inventors and innovators recognize that a newly created and developing technology or idea has the potential for widespread adoption (market share) in some sector; or Market Pull, where the eventual adopters of a technology or other product provide explicit demand and support for the development of new innovations to meet pressing needs (Dowling, 2004). For example, in environmental technology development the market pull created by newly implemented governmental regulations may provide the most important impetus to develop new products (Horbach, Rammer, and Rennings, 2012).

Experience shows that a transformative innovation may be a singular event attributable to one individual or single networked group, or it may arise independently amongst distinct individuals or groups. What lies at the heart of an original and revolutionary innovation and how to predict and then promote such breakthroughs has long been a focus of considerable thought and debate. No general model is widely accepted (Simonton, 2009; Sulloway, 2009). Transformative innovations can range from the relatively sudden appearance of revolutionary new ideas, such as the initial publications of Darwin's theory of natural selection or Agassiz's glacial theories, to long-term incremental advances in tools, such as the progression from balloon photography to satellite remote sensing. Transformative ideas may also be built upon earlier concepts that are evolutionarily refined and grow in application, are reframed in new, and sometimes controversial, ways, or are based upon retrogressive approaches that resurrect previously discarded ideas (Sulloway, 2009). As a negative example of the latter, obsolete Lemarkian evolutionary theory was forced upon Soviet science by Lyschenko in the 1930s, while as a positive example one might cite the resurrection and validation of Wegener's general ideas on continental drift following the Second World War.

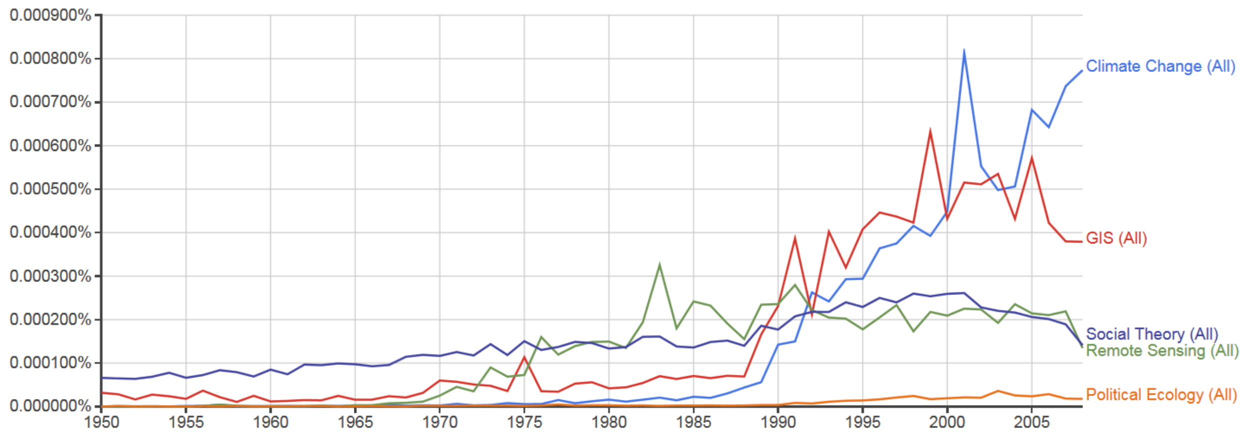

Figure 2.1 The growth of the terms Political Ecology, Social Theory, Remote Sensing, GIS, and Climate Change in published books held in the Google database, normalized by the total output of books. Data from Google Books *n*-gram Viewer (Lin et al., 2012; http://books.google.com/ngrams). The terms relate to broad research areas that while transformative within the geographical sciences, extend into many other fields and include research beyond the geographical sciences.

Finding 1: Transformative innovations can arise from a wide variety of individuals and groups, from a wide variety of intellectual sources, including older and long-ignored ideas, and through revolutionary or evolutionary paths.

A question that confronts firms in the development of innovative new products is whether to embrace open or closed models of innovation (Chesbrough, 2003). Is the faith in one's individual capacity to secretly develop a product and accrue the potential competitive advantages of that product outweighed by the synergies and infusion of new ideas and technologies into product development, which would accrue in an open setting with many groups and individuals within and outside the firm freely exchanging information? It has been argued that in the technology sector an open innovation approach is the most successful, despite the potential loss of product exclusivity (Chesbrough, 2003). Two recent examples of commercial firms embracing this approach are the release of patented battery technology by Tesla and the release of patented fuel-cell technology by Toyota.

Finding 2: An open innovation system in academic science and research can encourage the exchange of information even between competing groups, and helps to achieve the desire of the nation, funding agency, or foundation for the most rapid, productive, and efficient academic research sector.

No matter how brilliant a new research concept may be, it will not be transformative unless it is widely adopted. This entails refinement and diffusion. In its simplest formulation the inception and progress of a scientific transformation might be seen as the classical three-phase process of Invention > Innovation > Diffusion (Bush, 1945; Godin, 2006). However, it is now widely recognized that this linear model is an oversimplification of the dynamics that occur in the development and adoption of innovation. Rather, successful development and diffusion of innovation typically happens through a process of backward and forward communication and resulting feedbacks between the inventors, innovators, and adopters (von Hippel, 1986; Tuomi, 2002; Godin, 2006; Bogers, Afuah, and Bastian, 2010).

Finding 3: The promotion of rapid communication amongst innovators and adopters is critical for development as well as for the diffusion of transformative innovations.

Where might the biggest pay-off and least risk be when attempting to identify and promote the innovation and diffusion of a promising transformative research idea? It is typically difficult to identify a successful innovation in its earliest stages of development. Consider this in light of the success rate for the commercialization of U.S. patents and of venture-capital investments. Of the roughly 1.5 million current patents issued only about 3000 (0.2%) are eventually commercially viable (Klein, 2005). Venture-capital investments typically have high failure rates (>50%) even after careful analysis of start-ups by investment firms (Sahlman, 2010). Furthermore, only about 1% of venture capital is actually invested in embryonic seed-stage companies, while 45% and 50% is invested in relatively mature early-stage companies and developed later-stage companies which offer greatly reduced risk as compensation for reduced rates of investment return (Korteweg and Sorensen, 2010).

Finding 4: There are no established indicators that would identify specific individuals or concepts as sources of transformative innovation prior to the conduct of research.

Using innovation diffusion theory and commercial experiences as admittedly imperfect general guides, one might modify Rogers' heuristic model to incorporate some indication of risk and of potential pay-off to the institution seeking to develop and diffuse innovative and transformative scientific research technologies and paradigms. In this conceptualization some resources specifically targeted to promote innovation are spread amongst inventors and early innovators to promote the development of risky, but potentially innovative new ideas. Similarly, some of these innovation-targeted resources are also provided to allow for full diffusion and widespread application of mature innovations to later-stage majority users. The bulk of the innovation resources, however, are focused on the period of late innovation and early adoption when proof of concept is clear and applicability is demonstrated. This is the time when resources for final optimal development and early diffusion of the innovation can provide maximum leverage through mutually beneficial feedbacks between early adopters and innovators, producing rapid refinement of the idea. Aside from resources for direct technical development or application, communication between and amongst innovators and adopters is also an important area for support. Again, at this stage support can have high impact because such communication can promote refinement of the idea through feedback between innovators and adopters, and accelerate dissemination. It should be kept in mind that this model only refers to the proportion of total resources specifically targeted to promote innovation, and this investment occurs in the context of sufficient other resources being allocated to developed and ongoing areas of research.

CASE STUDIES: INVENTION AND EARLY INNOVATION

In this section the committee examines five areas of research that have been transformative in the geographic sciences within the past 65 years. These are discussed under the broad rubrics of Political Ecology, social theory that is directly informed by and related to geography and referred to here as Spatial Social Theory, Remote Sensing of the Environment, Geographic Information Sciences, and Global Climate Change. This list of research areas is not comprehensive, omitting some very significant areas of the geographical sciences where transformations have undoubtedly occurred. Nevertheless it represents a manageable sample of significant transformations that provides useful input to the committee's further deliberations and recommendations. Each of the research areas is now maturely developed, represented in many geographical science departments and beyond, and widely published. One would not have encountered any of these rubrics within the geographical sciences prior to the mid-20th Century and they thus clearly represent significant transformations.

During this period there have been contemporary transformative events within long-recognized fields of the geographical sciences, such as the theory of island biogeography within biogeography (MacArthur and Wilson, 1967) and non-equilibrium theories in geomorphology (Phillips, 1992). Similarly, with the broader field of geography there have been transformative developments such as the Quantitative Revolution (Burton, 1963) or the critical social theory of space and place (Harvey, 1973; Massey, 1973). Newer concepts such as sustainability science (Kates et al., 2001) or integrated approaches to the Anthropocene (Crutzen, 2006) are currently diffusing rapidly within the geographical sciences and will likely have similarly significant impacts. These and other past and present transformative events are all worthy of examination, but lie beyond the scope of this report. The five case studies presented here identify many of the inventors and early innovators, sources of ideas, and stimuli responsible for the development and widespread diffusion of these transformative research areas within and beyond the geographical sciences. These histories will then be considered in the context of the general innovation-diffusion concepts discussed above. The accounts, which by the nature of this report must be the briefest of outlines, are drawn in part from literature, but also rely on the expert views and opinions expressed by the workshop participants.

The individuals or groups who are responsible for the invention and early-stage innovation of the five transformative research areas discussed here come from both within and outside the academic discipline of the geographical sciences, and indeed in some cases from outside of the traditional academic-research sphere altogether. It is recognized and argued here that attribution of many important and transformative research ideas to any individual or one seminal piece of work is often impossible; and it is equally impossible to identify all of the individuals responsible; the intent here is to illustrate by example, rather than to be exhaustive.

Political Ecology

In political ecology, the geographical sciences were transformed by the shifting in focus away from classical agricultural/environmental and cultural views of rural development to the examination of larger economic forces, largely from a perspective of political economy theory. Political ecology is a melding of agricultural and soil sciences with development studies and political economics. The early transformative ideas are associated with both anthropology and geography (Bryant, 1998; Escobar, 1999; Blaikie, 2008; Robbins, 2012). Many consider Blaikie's and Brookfield's widely cited 1987 book *Land Degradation and Society* pivotal in defining the area of research within the geographical sciences. However the term political ecology has also been used in other and earlier contexts. For example, in 1935 a note by botanist/journalist Frank Thone compared Japanese displacement of indigenous pastoralists in Manchuria to the 19th Century displacement of Native Americans by U.S. expansion (Thone, 1935). Eric Wolf used the term in connection with a symposium published in *Anthropological Quarterly* which according to Wolf demonstrated "how sophisticated anthropologists have become in following through the connective linkages in local ecosystems and in specifying the parameters of economic change" (Wolf, 1972, p. 201).

In the geographical sciences the antecedents can be found in part in traditional rural regional development, hazards, and cultural ecology studies and in part in radical geography informed by Marxism and political-economy perspectives (Bryant, 1998; Robbins, 2012). The most direct lineage is to work on the political economy of the environment and hazards (e.g., by Watts, 1983; Hewitt, 1983; O'Keefe, Westgate, and Wisner, 1976). Political ecology has retained the attention of anthropologists, but even they acknowledged that by the 1990s "geographers and ecological economists have taken the lead in this endeavor" (Escobar, 1999, p2). Political ecology became dominated by geographers studying development in the 1980s and 1990s with students, especially from Clark University and the University of California at Berkeley, adopting it as their research approach and later becoming leaders in the field both within geography and more broadly (e.g., Peet and Watts, 1996). All of this activity helped to further develop political ecology and led to the growth of this research field within the geographical sciences; and led in turn to the ascendency of the discipline within this field. Political ecology provided particularly powerful analyses of the impacts of neoliberalism and the commodification of nature on livelihoods and landscapes in regions such as the Andes, Amazon, Mexico, and Sub-Saharan Africa, focusing on environmental resources that included water, forests, biodiversity, wetlands, carbon, and fisheries (Hecht and Cockburn, 1989; Liverman, 1990a; Zimmerer, 1991; Carney, 1993). In contemporary geography political ecology is a common approach to human–environment relations and includes many studies in the developed as well as developing worlds (Robbins, 2012; Peet, Robbins, and Watts, 2011). The work of political ecologists now informs both environmental policy and environmental activism (Perrault, Bridge, and McCarthy, 2015).

If one were to look for seminal figures in the development of political ecology in the geographical sciences one might cite Blaikie and Watts. Both came out of a doctoral background concerned with agricultural development, in India and Africa respectively. There is a clear trajectory between this early career work and their development of political ecology. To Blaikie and co-author Brookfield might go the credit for defining political ecology as it is considered in the geographical sciences: "The phrase *political ecology* combines the concerns of ecology and a broadly defined political economy. Together this encompasses the constantly shifting dialectic between society and land-based resources, and also within classes and groups within society itself " (Blaikie and Brookfield, 1987, p17).

Spatial Social Theory

Spatial social theory is another transformative research area for which disciplines such as sociology acknowledge a strong debt to the geographical sciences. It has a similar genesis to political ecology in that some of the earliest work in its conceptualization can be traced to sources outside the geographical sciences. In this case the German sociologist/philosopher Georg Simmel's work *Die Grosstädte und das Geistesleben* (*The Metropolis and Mental Life*; Simmel, 1903) might be considered an early fountainhead. Subsequent development of critical social theory within the context of the geographical sciences was highly influenced by the work of the French philosophers Henri Lefebvre, most notably perhaps *La production de l'espace* (1974), and Michel Foucault (e.g., Foucault, 1976). At approximately the same time within the geographical sciences David Harvey in his book *Social Justice in the City* (Harvey, 1973) was laying the groundwork for critical spatial social theory that would be an essential component in its early-stage innovation. Other geographical scientists such as Doreen Massey (1973, 1994) followed similar lines of analysis with vigor and insight. Similar critical reframings of urban social theory were being pursued well beyond the geographical sciences at this time (e.g., Gordon, 1978), helping to ferment a rich intellectual medium. As is the case with political ecology, the list of scholars whose work formed the initial impetus included both geographical scientists and representatives of other fields, though all were Marxist in their analytic perspectives and linked theories of political economy with the development and delineation of urban space.

Interestingly Harvey's doctoral work was in historical geography and this was followed by his influential 1969 book *Explanation in Geography* (Harvey, 1969) which propounded a largely positivist and quantitative perspective for the geographical sciences. Although historical analysis remains important in the work of Harvey, his trajectory as an innovator of the more radicalized and activist perspectives he developed was not clearly presaged in his earliest work. At the same time the sociologist Harvey Molotch's 1976 paper "The City as a Growth Machine" (Molotch, 1976) further drove urban spatial theorists to move beyond former views of the role of geography in the city and consider social context of land parcels and the opportunistic and contextual social factors of the different communities involved that help determine urban form. Free access to U.S. Bureau of the Census data on the geographic distribution of variables such as income and race provided critical empirical data to formulate and test hypotheses. Such data, for example, play an important role in Molotch's paper. As the innovation of spatial social theory progressed, geographical scientists such as Kay Anderson, Peter Jackson, and Linda McDowell and others expanded beyond the political economy context of class to include issues of spatial structure in the context of social constructions of race (Anderson, 1987; Jackson, 1987) and gender (McDowell, 1993; Massey, 1994).

Remote Sensing of the Environment

Remote sensing of the environment transformed the geographical sciences through the capacity to examine earth-surface features and processes from air- and space-borne platforms. Although often coarse in spatial resolution, this allowed integrated analysis over huge geographic areas and remote locations quickly and with increasingly sophisticated measurements of surface properties. Over the past 50 years satellite remote sensing of the environment has come to form a major component of research in the geographical sciences. Modern remote sensing of the environment has its antecedents in the 1840s to 1860s, when Francois Arago, the Director of the Paris Observatory, proposed the concept of using photographs taken from balloons for the purpose of topographic surveys (Aronoff, 2005). Airborne photography for both civilian and military use further expanded with the invention of the airplane. Both the technology of image acquisition and the methods of photogrammetry and interpretation were accelerated by military applications during the Second World War. Immediately after the close of the war captured German V-2 rockets were used by the now antagonistic American and Soviet military to take photographs of the Earth from the thermosphere and the science of space-borne remote sensing was born. However, as the military took the lead in technology development and deployment, access to the latest developments was generally kept from civilian users (Aronoff, 2005). The declassification of remote-sensing technologies and images during the late 1950s through the 1970s allowed for the flowering of space-borne remote sensing of the environment as an academic research area (Aronoff, 2005). Indeed, the term *remote sensing of the environment* first appears during the period of the early 1960s, perhaps most notably with the convening of the first International Symposium on Remote Sensing of Environment in 1962. This conference was held at the Willow Run Laboratories at the University of Michigan, which were largely an engineering and defense research facility. Although some geographical scientists were undoubtedly involved in the earlier development of space-borne remote sensing techniques, this line of research, its potential applicability, and the term 'remote sensing of the environment' explicitly entered the geographical sciences literature in the early 1960s (Bailey, 1963; Bird and Morrison, 1964).

It is the launch of ERTS-1/Landsat-1 in 1972 that serves as a point in time when the modern and transformative science of remote sensing of the environment can be said to have commenced. Originally called the Earth Resources Technology Satellite (ERTS-1) and later renamed Landsat-1, this was the first platform designed specifically for the collection of data on Earth-surface conditions and natural resources. The geologist William T. Pecora, Director of the USGS from 1965 to 1971, is considered largely responsible for the higher-level vision and political effort that led to the creation of the ERTS/Landsat series of Earth observing satellites (USGS, 2012). The satellite was built by General Electric, designed and launched by NASA in partnership with the Department of the Interior, and funded by the U.S. Government (Williams, 1976; USGS, 2012). This event was at the vanguard of the explosive growth in environmental remote sensing through the 1970s and 80s (Figure 2.1). Over the past 40 years it has been joined by a host of environmental remote-sensing satellites. These have been both governmental and private in ownership and from a number of different countries. An early publication regarding ERTS-1, its potential, and some of its earliest results was issued by the USGS in 1976 (Williams, 1976). Although dominated by federal-government scientists, it is notable that academic geographical scientists such as Neal G. Lineback from the University of Alabama, Richard Ellefsen from California State University, San Jose, and John B. Rehder of the University of Tennessee were amongst the small group of non-governmental scientists working on the earliest development of ERTS-1 applications and products (Williams, 1976). By the 1970s environmental remote sensing was transforming research and educational curricula in many geographical sciences departments (Estes and Thaman, 1974).

Important early roles in development of environmental remote sensing were played by geographers David Simonett and Jack Estes. Simonett was a pioneer in radar remote sensing. He graduated with a doctorate in geography, and working with colleagues from Engineering and Geology co-established a radar based remote sensing unit at the University of Kansas in 1966. He later worked for a time in the commercial sphere at Earth Satellite Corporation and eventually joined Estes on the faculty of Geography at the University of California, Santa Barbara (Morain, 2006). After obtaining a Geography degree Estes worked in remote sensing intelligence with the Central Intelligence Agency and then with Texas Instruments before returning to university for a geography doctorate in cartography. He was a pioneer in developing marine remote sensing of phenomena such as oil spils and with Simonett established a remote sensing unit in the Department of Geography in 1972. He wrote a widely used textbook and later undertook extended assignments in federal agencies such as the USGS and NASA to help formulate national remote-sensing strategies (Dozier and Asrar, 2001; Jensen, 2008).

Unlike political ecology, the inception of environmental remote sensing cannot be said to have been instigated by a small group of seminal figures, but rather has been 'big science' and 'big technology' from the start, with a host of innovators working to create the broad spectrum of innovations that collectively gave rise to this research area.

Geographic Information Systems and Science

Although cartography and the analysis of maps has always been at the heart of the geographical sciences, the development and deployment of computerized geographic information systems (GIS) has not only revolutionized the collation, mapping, and visual presentation of geospatial data in a rapid and efficient manner, but allowed for the detailed quantitative spatial analysis of individual variables and the synthetic and integrative analysis of many variables simultaneously. In the past two decades research and teaching in GIS has become a fixture of any geographical sciences program of note. In addition to having considerable synergy with remote sensing, its inception may in some ways be seen as a hybrid of how political economy was conceived and how environmental remote sensing developed. The history of GIS in the mid to late 20th Century has many elements (Coppock and Rhind, 1991; Tomlinson and Toomey, 1999; Chrisman, 2006; Esri, 2012), but if one figure were to be cited as seminal in the inception of GIS it would be Roger Tomlinson of Canada. He held undergraduate and graduate degrees in geography from both the UK and Canada. The focus of his Masters work at McGill University was on glacial geomorphology (Tomlinson, 1963). Tomlinson worked in the private sector on aerial photography (Tomlinson and Brown, 1962) and then as a consultant to, and subsequently employee of the Government of Canada, where he conceived the first computerized GIS, and developed it in collaboration with IBM. The introduction of the actual term "geographic information system" can be found in his 1968 publication "*A geographic information system for regional planning*" and its origins in his work can be traced to earlier reports and publications (Tomlinson, 1962; Department of Forestry and Rural Development, 1967).

Tomlinson's vision, which became the Canada Geographic Information System (CGIS), was initially devised to solve a technical problem in the Government of Canada, that of accurately estimating areas of land from the maps being drawn for the Canada Land Inventory. Known techniques were labor-intensive and inaccurate; Tomlinson speculated that if the problems of digitizing maps and storing their contents in computers could be solved, then computing and reporting areas would be both fast and accurate. Computers had been developed as calculating machines, and the idea of using them to manipulate the contents of maps was outlandish; the technology to scan maps had to be invented from scratch; but perhaps the most innovative aspect of CGIS was the notion of using computers to analyze the contents of maps. It was this last idea that led to the initial adoption of GIS as a useful and eventually transformative tool for the geographical sciences.

The conception of GIS owes much to Tomlinson, but at least three other sources can be recognized. One is Howard Fisher, an architect, who founded the Laboratory for Computer Graphics at Harvard in the 1960s to develop computer software for mapping. A second is Jack Dangermond, a landscape architect, who in 1969 founded the commercial company Environmental Systems Research Institute (Esri) following his studies and work in the Graduate School of Design and the Laboratory for Computer Graphics at Harvard. Esri now dominates the market for commercial, governmental, and academic GIS with its ArcGIS line of software products. A third is the group at the Bureau of the Census who developed geographic databases such as GBF/DIME in preparation for the 1970 census. These databases were made publicly accessible, and gave enormous stimulus to geographical scientists, who could now readily use mapping software and census data to create new knowledge about human geography. TIGER, the outgrowth of GBF/DIME for the 1980 census, was a major factor in stimulating the growth of wayfinding services, and ultimately of the online mapping services that the research community and the general public use today.

Innovation in GIS continues today at an increasingly rapid pace. In part the pace is set by technical innovations, such as the Global Positioning System (GPS) and the advent of Web-based mapping and wayfinding services. Some of the innovation is societal in origin, as for example in the growing engagement of the general public in both the consumption and the production of geographic information (Sui et al., 2012), and in its growing concern over privacy. High-performance computing is another technical innovation, from outside the geographical sciences, that is having a profound impact on the field, in the form of CyberGIS (Wang, 2010). Finally, while early GIS had been largely about representing the contents of paper maps, in other words representations of a flattened and therefore distorted Earth, the notion of GIS as a container of globes has been gaining strength, stimulated in part by a speech of Vice President Gore in 1998 and by the release of Google Earth, the first publicly available virtual globe, in 2005. These virtual globes are manifestations of a vision of Digital Earth, a term first coined by Gore (1992) and denoting a comprehensive digital representation of all that is known about the planet, in effect a mirror world.

Global Climate Change

Global climate change due to human activity that increases greenhouse gas concentrations is arguably one of the most transformative research topics affecting the geographical sciences and many other disciplines today. The analysis of how the Earth’s climate is changing due to anthropogenic increases in greenhouse gasses, the reaction of biophysical and human systems to such changes, and the coupled biophysical-socioeconomic analysis of mitigation of, and adaptation to global climate change has generated research and educational efforts from every sector of the geographical sciences. Research on anthropogenic climate change has a long history (see Jones and Henderson-Sellers, 1990; Rodhe et al., 1997; Fleming, 1998; MacDonald, 2011).

Scientific speculation on the potential for anthropogenic greenhouse gasses to significantly warm the planet can be traced back to the 19th Century. Swedish physicist Svante Arrhenius and geologist Arvid Högbom calculated that doubling atmospheric CO_2 concentrations would cause a 4–5^{o}C change in global temperature and that the burning of fossil fuels could add such significant amounts of CO_2 to the atmosphere. These ideas on future global warming were largely dismissed by the mid-20th Century for a number of reasons based upon then-current empirical observations and climatological and oceanographic theory. However, in the 1930s the English engineer and amateur meteorologist Guy Callendar collected available measurements of atmospheric CO_2 and surface temperature and took the countervailing stance that there had been an increase in both since the 19th Century, and that greenhouse warming might already be underway. By the late 1950s and 1960s the work of scientists such as physicist Gilbert Plass at Johns Hopkins University, oceanographers such as Roger Revelle at the Scripps Institution of Oceanography, UC San Diego, and atmospheric chemists, most notably Charles Keeling, also at Scripps, began to demonstrate that increasing levels of greenhouse gasses could indeed be sufficient to increase global temperature.

The geographical sciences had an early role in the formulation of climate–human relationships and responses to natural hazards that laid a foundation for integrated climate–society analysis of global climate change. Natural-hazards researchers led by Gilbert White were providing important insights into the human response to climate variability (White, 1945). Geographical scientists were also leaders in developing the multi-faceted synthesis and analysis of how human activity across a broad range of physical and biological systems, including climate, had changed and were changing the face of the Earth (Thomas et al., 1956; Turner et al., 1990). They were also leaders in improving our understanding of the impacts of past climate change, providing a groundwork for similar multi-faceted analysis of climate-change impacts.

The development of high-powered computing allowed scientists to project the impacts of increasing carbon dioxide on the climate. Several geographical scientists made important contributions to the development of such climate models, including Roger Barry, Ann Henderson Sellers, and Jill Williams (Jaeger) (Williams et al., 1974; Henderson-Sellers, 1978; Williams, 1978).

In the 1970s the scope of climate change research and public interest expanded markedly, in part fueled by the 1970 workshop "Study of Critical Environmental Problems" at the Sloan School of Management. A second conference held quickly thereafter in Stockholm brought this view forcefully to the attention of the public at large with the publication of the edited volume *Inadvertent Climate Modification. Report of Conference, Study of Man's Impact on Climate* (Wilson and Matthews, 1971). It can be argued that powerful articles in the mid to late 1970s, such as "Climatic change: are we on the brink of a pronounced global warming?" by Columbia University earth scientist Wally Broecker (Broecker, 1975) and the popular book *The Genesis Strategy: Climate and Global Survival* (Schneider and Mesirow, 1976) were the clarion call and set the course for the explosion of global climate change research from the 1980s to the present (Figure 2.1). The World Climate Conference in 1979 (chaired by geographical scientist Ken Hare) included an important statement by geographical scientist Bob Kates (Kates, 1979) on climate and society that set an agenda for what became known as human dimensions research. As a result Kates could be considered the pivotal figure for geographical scientists in research on the impacts of climate change.

Meanwhile geographical scientists were taking the results of climate projections and connecting them to potential impacts on society, including agriculture, water, ecosystems, and urban areas, with a key volume edited by Bob Kates on Climate and Society (Warrick and Riebsame, 1981; Kates et al., 1985) and with a new journal *Climatic Change*, (Warrick and Riebsame, 1981; Terjung et al., 1984). The work of geographical scientists also featured prominently in the two-volume study by Martin Parry and colleagues on regional impacts of climate change (Parry et al., 1988).

The Intergovernmental Panel on Climate Change (IPCC) was established in 1988 by the

World Meteorological Organization (WMO) and the United Nations Environment Program (UNEP) to regularly gather together and assess all aspects of climate change and its impacts and assist in formulating realistic response strategies. It was this development more than any other that led to the merging of intense scientific, public, and policy interest in climate change, producing an explosive growth in innovation of climate change research in the geographical sciences and far beyond (Figure 2.1). A clear reason for this is that the regularly published IPCC reports (Houghton et al., 1990; IPCC, 1995, 2001, 2007, 2013) consider the span of the physical and life sciences and then extend to agriculture, conservation, development studies, economics, energy studies, and public policy, and explicitly point out uncertainties that require research. The topic draws widely across many research spheres.

A broad range of the geographical sciences spanning from climatology to biogeography, geomorphology, hydrology, pedology, and paleoclimatology have all contributed to research on climate change. Important aspects of this contribution have been the compilation and temporal-spatial analysis of large environmental and paleoenvironmental data sets (Legates and Willmott, 1990; Bartlein et al,, 1998; DeFries et al., 1998; MacDonald et al., 2006), integrating the impacts of land-use and land-cover change with climate-change drivers and feedbacks at fine spatial scales (DeFries et al., 1999; Feddema et al., 2005), and developing the concept of climate *vulnerability*, where the impacts of climate change depend as much on the conditions of society as they do on the climate change itself. Geographic innovators in the analysis of vulnerability include Michael Watts, Tom Downing, Karen O’Brien, Robin Leichenko, Susan Cutter, Diana Liverman, and Hallie Eakin (Eakin and Luers, 2006; Leichenko and O'Brien, 2008; Watts and Bohle, 1993; Cutter, 2003; Downing et al., 1993; Liverman, 1990b). The study of environmental vulnerability and risk assessment as pioneered by geographical scientists in the 1980s and 1990s, by researchers such as Diana Liverman (1990b), Susan Cutter (1996), and others, has become a critical component. Geographical-science tools such as the hazards-of-place models of vulnerability (Cutter et al., 2000) have obvious widespread applicability.

The social sciences are now a core area of global climate change research (ISSC and UNESCO, 2013) and the geographical social sciences are important contributors on mitigation and climate governance (Stripple and Bulkeley, 2013; Bulkeley and Newell, 2010; Betsill and Bulkeley, 2006). The importance of engagement with the humanities on issues of climate change is increasingly recognized (Hulme, 2011).

Finally, climate change research has been an important impetus for the development of geographically based Earth systems models that integrate a variety of physical, biological, and increasingly societal features into integrated quantitative simulations, and draw upon remote sensing and GIS in their formulation and testing. This area itself is becoming a transformative wave in the geographical sciences. These factors listed above have today created the huge, integrative, and rapidly innovating science of climate change research. It should also be noted that publicly available meteorological data from the weather stations of numerous countries and the extensive, public, climate data and climate-model data available from the IPCC and national bodies, most notably the U.S. National Oceanic and Atmospheric Administration, have been important fuel for the growth of climate change analysis and modeling. These factors listed above have today created the huge, integrative, and rapidly innovating science of climate change research.

CASE STUDIES: DIFFUSION AND DEVELOPMENT

In all of the cases discussed above the subsequent diffusion and further development were parallel and complementary processes that were facilitated by a number of mechanisms. Some of these mechanisms apply to all the areas of transformative research outlined in the previous section and others were more restricted in their applicability. Important mechanisms of diffusion and development of the transformative research areas outlined above are provided below.

Political Ecology and Spatial Social Theory

Face-to-face meetings and direct communication between developers and early adopters were critical in the cases of political ecology and spatial social theory. Later, the Political Ecology Research Group was formed in England in 1976 to foster interactions between adherents of the new research paradigm on an informal basis. A regular political ecology workshop was established at Berkeley. The Cultural Ecology Specialty Group (the name was expanded to 2000 to Cultural and Political Ecology Specialty Group) was formed within the Association of American Geographers in 1980. These workshops, symposia, conferences, and informal interactions were typically facilitated in ad hoc ways, through foundation support or as part of larger disciplinary meetings. Universities such as Berkeley and Clark were important in these efforts. Large official steering committees and semi-permanent governmental agencies were not a feature. Blaikie (2008) has termed the process eclectic but also inclusive. Publications in the form of books, including edited volumes, and in the form of journal articles were critical in the diffusion of ideas in both political ecology and spatial social theory. Although the *Journal of Political Ecology* was founded 1994 as an open-access forum for the discipline and many political ecology papers appear in the disciplinary *Annals of the Association of American Geographers* and *Geoforum*, it is reasonable to say that, for both political ecology and spatial social theory, publication in a wide variety of journals, including those favoring radical and critical perspectives such as *Antipode*, was equally important for diffusion in the geographical sciences.

Environmental Remote Sensing and GIS

The diffusion and development stage of these two somewhat technology-driven transformations has some similarities, but also striking differences from political ecology and spatial social theory. Face-to-face meetings of key individuals in research agencies or university departments were important for early diffusion, particularly when they drew together multidisciplinary groups such as the radar remote sensing faculty at Kansas. Symposia were also important, and some of these have become institutionalized as regular formal events of considerable size. Since the inaugural International Symposium on Remote Sensing of Environment was launched in 1962 this now-biennial event has been held 35 times at venues throughout the world. These events have also become relatively large with some 700 abstracts submitted for the 2015 International Symposium on Remote Sensing of Environment. Similarly, the recent Geoscience and Remote Sensing Society meeting drew 1800 participants. These meetings receive support from both governmental agencies and industry. The American Society for Photogrammetry and Remote Sensing also holds a large annual conference. In the 1970s the USGS and NASA directly established and supported the Pecora Symposia to promote the exchange of scientific information from remotely sensed data for a broad range of uses and to provide a forum for discussing ideas, policies, and strategies for remote sensing of the Earth.

Symposia have also been important for the diffusion and development of GIS. In 1963 Edward Horwood, a planner at the University of Washington, organized the first annual Conference on Urban Planning Information Systems and Programs which eventually became the GIS-Pro conference under the auspices of the Urban and Regional Information Systems Association and has run now for some 50 years. Many other conference series have been instrumental in promoting the diffusion and development of GIS, including the biennial International Symposia on Spatial Data Handling, the biennial International Symposia on Geographic Information Science, and a host of training workshops. The commercial firm Esri has hosted an annual GIS User Conference since 1981. It is some measure of the success of GIS as a transformative research area that the 1963 Conference on Urban Planning Information Systems and Programs had 48 attendees and recent Esri User Conferences have attracted some 14,000.

Both environmental remote sensing and GIS have also benefited from the establishment of national programs for research funding and the diffusion of knowledge. In the case of the Landsat program and other U.S.-based remote sensing, the U.S. government through NASA has long supported fundamental and applied research internally and through grants programs. The Mission to Planet Earth program launched in 1991 and the current activities of NASA's Earth Systems Science initiative reflect this. The current annual budget for NASA Earth sciences initiatives is on the order of $1.7 billion.

A significant impetus to the diffusion of GIS began in 1988 with the funding by NSF of the National Center for Geographic Information and Analysis (NCGIA). Located at three sites (University of California Santa Barbara, the University at Buffalo, and the University of Maine), NCGIA was charged with advancing GIS technology and practice through fundamental research; promoting the use of GIS throughout the sciences; and developing materials for GIS education. Its Core Curriculum had a large and lasting impact by identifying and elaborating the key elements of a university course in GIS.

Many academic geographers were early-stage adopters of GIS, recognizing its potential as a tool for the geographical sciences. Other disciplines quickly followed: archaeology and forestry were notable early adopters, followed by ecology, criminology, public health, and many other fields. Today adoption has reached a late stage as Figure 2.1 suggests, with GIS being acknowledged as an essential tool for scholarship across the social and environmental sciences, and increasingly in the humanities.

One significant change occurred in 1992, at a time when a growing chorus of scholars was beginning to question the intellectual significance of GIS. To many it appeared as a tool or technique, much like remote sensing, cartography, or even word processing, to be addressed through service courses and supported by academic staff, but hardly a profound innovation in the discipline. In the words of one President of the Association of American Geographers, GIS was "non-intellectual expertise." The opposing arguments were already evident in the discussions leading up to the funding of NCGIA, and culminated in a 1992 paper in which the author defined what he termed "geographic information science" (Goodchild, 1992), a set of principles and knowledge that could be acquired through either empirical research or the development of theory, and that formed the foundation on which geographic information systems were constructed. The “tool vs science” debate (Wright et al., 1997) still surfaces from time to time, but geographic information science is now recognized as a substantial intellectual domain, with its own journals, programs, and conferences.

There are many specialized journals and book series that serve as outlets for research and reviews of research in environmental remote sensing and GIS (where S denotes variously science and system). As is the case of the various regular conferences, these journals have grown to have widespread readership and impressive citation statistics. Examples include *Remote Sensing of Environment*, *International Journal of Remote Sensing*, *Photogrammetric Engineering and Remote Sensing*, *International Journal of Geographical Information Science*, and *International Journal of Digital Earth*. In addition, diffusion and further development was aided by special book series, including *Remote Sensing and Digital Image Processing*, *Remote Sensing Applications*, and Esri Press publications. However, as with political ecology and spatial social theory, publications regarding GIS or using GIS transcend these venues.

Global Climate Change

The diffusion and development stage in global climate change has functioned through a wide variety of means. Institutional face-to-face interactions between individual scientists at places like Scripps were important, as were the early symposia at the Sloan School of Management, in Stockholm, and at the World Climate Conference. Focused journals such as *Climatic Change, Global Environmental Change,* and *Global Change Biology* also played a critical role from the late 1970s onward. There are also dedicated book series such as *Critical Climate Change* and *Princeton Primers in Climate*. However, the vast majority of publications occur through a wide variety of disciplinary and multi-disciplinary journals and books. The IPCC brings hundreds of researchers together in many meetings and through frequent communication to develop each of its reports, leading not only to the regular diffusion of an immense amount of information on global climate change science, but also to the creation of community networks and the stimulation of further research. Within the U.S. the reports of the Global Change Research Program provide important syntheses, especially the National Climate Assessments. Furthermore these reports garner large amounts of attention in the popular press and result in communication of knowledge to the public. This helps to develop public awareness and draw in young people to the research community through this exposure. The political debates about global climate change in the U.S. and elsewhere further diffuse information about this area of research.

The Importance of Individuals[1]

The role of dedicated and charismatic individuals in driving the diffusion and development stage of transformative research cannot be ignored. Individual thought leaders, either early innovators such as Piers Blaikie, Michael Watts, or Susanna Hecht in political ecology, David Harvey and Doreen Massey in spatial social theory, and Wally Broecker, Steve Schneider, and Martin Parry in global climate change, or early adopters and developers such as

[1] In citing specific names in this section and elsewhere in this chapter the committee acknowledges the impossibility of recognizing every contribution, and accepts full responsibility for any unintentional omissions.

Michael Goodchild and Jack Dangermond in GIS, are extremely important in early diffusion and the spurring of further development through their publications and presentations. In some cases their efforts in developing formal and informal networks and infrastructure and leading national and international committees are also highly important drivers of diffusion. Finally, the transmission of transformative research ideas in undergraduate and graduate education is of great importance. Here gifted educators often play a role. In all of the research areas discussed here one can easily trace the diffusion of knowledge from academic advisors who were innovators or early adopters to their students who then not only adopted the research paradigms, but also contributed to their further development. Coppock and Rhind (1991), in tracing the interplay between individuals, government, and commercial sectors in the rise of GIS, point out that the lack of a teaching program at the Harvard Laboratory meant that it directly added few new professionals to the field and perhaps even shortened its own life by its lack of a teaching component.

The Importance of Support Structures

Development and early application of transformative research is of course dependent upon resources to support such work. Political ecology and spatial social theory have relied upon traditional academic research granting programs and foundations as well as international development funds. Institutional structures have played a role in the other three case-study areas. Of particular importance for remote sensing, various NASA programs specifically focused on funding research on remote sensing of the environment by outside scientists. To support research in or using GIS, academics have tapped funding from many parts of NSF, especially the Geography and Spatial Sciences program, the Education and Human Resources directorate, and programs within the Computer and Information Sciences and Engineering directorate. Significant awards for GIS research have also come from the National Institutes of Health and the National Geospatial-Intelligence Agency, among many other sources. Global climate change research has benefited from many targeted grants programs of governments and foundations. The IPCC, while not embarking on primary research, has provided an ongoing venue for extensive networking, synthesis of research results, and dissemination. Critical governmental support for transformative research has also come in forms other than direct research grants. Governmental agencies employ many scientists and engineers working on issues of remote sensing, GIS, and global climate change. This enhances the diversity of the community working on these issues, and provides a continuation of the science and opportunities for employment by geographical sciences graduates. The ready availability of U.S. Bureau of the Census data for spatial social theory and GIS research, and the similar availability of Landsat data for environmental remote sensing and the NOAA weather and climate data for global climate change research, have been invaluable for the development and diffusion of these transformative research areas. Yet although these three latter areas have benefited from coordinated and long-term ‘big science’ support, political ecology has also shown that not all transformative research in the geographical sciences must involve such levels.

SYNTHESIS

Technology Push or Market Pull?

What has ultimately driven the emergence and success of the transformative research areas discussed above? As outlined earlier, the impetus for innovation and diffusion has often been characterized in terms of technology push, where inventors recognize that a newly created and developing technology or idea has unrecognized, widespread need and the potential for widespread adoption; or market pull, where an innovation is created specifically to meet a demand. In all of the cases discussed here both of these factors have played a role in varying degrees. It would seem at first thought that remote sensing and GIS are models of technology push. However, in actuality each of these areas developed to meet needs dictated at the first instance by governmental agencies. Much of the early post-war remote sensing development was done in the context of defense, while the embryo of GIS lay in Tomlinson's response to Canadian governmental resource and planning requirements. Similarly, political ecology and spatial social theory were developed in response to perceived weaknesses in rural development models, and social and urban theories based on traditional economics' neo-classical paradigm and positivist social-science paradigms. It could also be argued that these two areas developed more viscerally in response to observed disparity in social and economic status between the developing and developed worlds. More often than not in the geographical sciences transformative research is likely to develop in response to widespread need that transcends the sub-fields, individual academic disciplines, and academia in general and extends to having wider (global) societal impacts.

If there is a strong case for a transformative research initiative initially generated by technology push it is global climate change. Here scientists predicted the potential for climatic change, based on many observations and the availability of high-powered computing for models, long before there was any empirical evidence suggesting such changes were occurring and long before there was any significant governmental or public interest in the potential impacts of anthropogenic climate change. Indeed, the current concerns of governments, other agencies, and private citizens regarding climate change are more in response to scientifically projected conditions in the latter half of the 21st Century than they are to present conditions. In this case as above, however, the applications of the research extend across myriad disciplines and directly address societal concerns at a global level.

Evolution or Revolution?

The case studies outlined here would suggest that transformative research does not arise and come to dominate the geographical sciences in a rapid revolutionary manner. Rather there is a relatively long process of decades at least between innovation and full diffusion. This has important consequences as innovators, early adopters, and early supporters may not see full ascendancy of a research paradigm for some time and are therefore taking a risk even with a successful innovation. Political ecology in its current form and GIS can arguably be said to have the most revolutionary origins, having been conceived in the 1960s and 1970s, while environmental remote sensing and the study of global climate change have scientific histories extending back beyond a century. Yet even in these cases the histories are not clear cut. The ecological and political concepts underlying political ecology can be traced to the 19th Century, and GIS has deepest roots in cartography, an ancient discipline, and the Quantitative Revolution in the geographical sciences of the 1950s and 1960s. Global climate change research in its current broad multidimensional scope can be seen to have arisen in the 1970s, although scientific speculation on greenhouse gas effects long predates that. What can be said is that in all cases these transformative research areas built upon preceding research and technologies, from which they then embarked in transformative trajectories. Engagement in and support of established research areas in the geographical sciences are important in and of themselves to answer pertinent scientific and societal questions, but they also provide the foundations from which transformative research will arise. At the same time as engaging in and supporting established research there must be some engagement and support for exploring new concepts and approaches even though their final success as a transformative research paradigm may not be clear for years. In a sense there is a double risk here. Novel approaches and technologies may not always work when put to the test, or may take longer to fully develop than anticipated. It may also take many years to see the full diffusion and adoption of those that succeed. However, not to embark on innovative, if unproven, research and be willing to stick with it through development and diffusion invites stasis.

What Sorts of Research are Likely to be Transformative?

It is also important to note that the characteristics of the transformative innovations themselves vary. Remote sensing and GIS were highly dependent upon the development of new technologies such as satellites, map scanners, sophisticated sensors, and powerful but readily available computing, visualization, and data-storage technology. Political ecology was driven by critical ideas rather than dependent on technology. In addition, the fundamentals of Marxist political economy that were at the roots of political ecology's early conceptions were already a century old when this transformative research area was being formulated. Political ecology draws upon scientific theories related to ecology and scientific methods related to agriculture, soil sciences, ecology, epidemiology etc. in the analysis of rural development. Climate change research, initially identified by interdisciplinary earth scientists and advanced through computer modeling, was transformed and linked to policy as it engaged with social scientists who explored the human causes and consequences of climate change and the potential responses to it, while at the same time accumulating observational data on gas concentrations and temperature increases provided empirical support for the theory. The examples above suggest that transformative research in the geographical sciences often is that which transcends a sole focus on either technology, natural science, social science, or policy in terms of methods and contributions.

Early Identification of Transformative Innovators

As is consistent with the experience in the business and technology sectors, identifying the specific characteristics of individuals who might create a transformative research idea is fraught with uncertainty. The examples cited here include individual savants, small groups independently working at the same time on similar issues, and large networked group efforts. Innovators came to the geographic sciences from varied origins and routes. In their early graduate careers David Harvey and Roger Tomlinson pursued entirely different interests from the transformative research areas that would eventually become linked to them. Blaikie and Watts were academic geographers with doctorates in that discipline. However, Henri Lefebvre and Michel Foucault were philosophers. Although Roger Tomlinson was an academically trained geographer he was situated in the private sector and government when doing his seminal work. Howard Fisher was an architect, and Jack Dangermond a landscape architect whose work was influenced by graduate studies at Harvard, but Dangermond's important contributions in the initial development of GIS stem from his private firm Esri. Goodchild's training progressed from physics to geography.

One common element in the five cases is that the innovators worked broadly beyond disciplinary or academic constraints. There was much cross-fertilization through exposure to different disciplines or work within government agencies and the private sector. Having access to a diversity of perspectives, ideas, and research needs is critical and can be actively promoted. Identifying which specific individual will have the vision and see the opportunity to meld it into a transformative research innovation is more difficult. In addition the innovators and early developers typically had a deep grounding and facility in established research disciplines from which to draw. Finally, they were not only effective communicators in publication and other forms and persuasive in securing support, but they had persistence in advancing the transformative research agenda.

Effective Modes of Diffusion and Development

All of the transformative research areas considered here benefited from early face-to-face meetings and workshops, and the establishment of formal and informal networks. In the case of remote sensing and building the Landsat program these interactions largely took place via government agencies, whilst for political ecology and spatial social theory these occurred largely in traditional academic venues such as conferences, symposia, and faculty-student interactions. In some cases specialized journals have been helpful, particularly for remote sensing and climate change, but it is arguable if this is as critical for political ecology or spatial social theory, although for these geographically oriented journals and journals based in political economy have been important. In all cases energetic and persuasive innovators and early adopters were important in marshalling these mechanisms.

Mechanisms for funding of research and diffusing results have been critical for all areas. The impacts of the large budgetary support for work in remote sensing and global climate change are manifest in the rapid growth of these areas and in the size of the research community organized around them. GIS has had a hybrid model where funding directly for GIS research by agencies such as the NSF has been modest compared to support for remote sensing or global climate change, but the large applied component of investment in the public and private sectors has helped to drive the adoption of the technology and to spread the results of GIS research. Although the levels have been more modest, governmental and foundation support for work in political ecology and spatial social theory has also been vitally important for their growth and application.

One factor that is critical in the histories of all of these transformative research areas is the power of the open model of innovation in generating positive feedback and synergy between diffusion and further development. The often-informal networks of scholars working on political ecology and spatial social theory certainly helped the early development of these field. The U.S. Census data, declassification of military remote sensing technologies, the open-skies policies for global remote sensing and free provision of Landsat and MODIS data, the NOAA open databases on climate, the expansive and open nature of the IPCC efforts, and the online dissemination of these reports and data have all worked to drive transformative research forward in an accelerated fashion. Research support provided by the NSF, NASA, NOAA, and other U.S. government agencies generally requires a timely and open distribution of data upon completion of studies. This has been an important driver of the diffusion and further development of transformative research in the geographical sciences.

Putting it Together –the Recipe for Transformative Research

Taken together the five case studies suggest that the innovation of past transformative research in the geographical sciences has occurred when talented individuals have had exposure to diverse research perspectives and approaches and have tackled questions of multi-disciplinary and societal importance. The diversity of experience that engendered this included work with other disciplines and often outside of academia. These individuals were able to envision the development of new research tools and approaches or the novel application of existing tools and approaches to meet those demands. They were knowledgeable and drew liberally from established research inside and outside of the geographical sciences. They had support for the initial research, and growing, networked groups for refinement and dissemination. Those individuals and the networks they developed included scientists who had educational backgrounds solely within the geographical sciences, and mixtures of geographical scientists and others from different disciplinary backgrounds. Energetic and charismatic innovators and early adopters played a crucial role in refinement and diffusion of research innovations. Although it is difficult to generalize on the traits that would identify individuals as likely innovators and key early adopters or diffusers, aside from intellect and imagination four features seem apparent:

1. Deep knowledge of the established research on which they are building;
2. Boldness in the face of the risks of departing from established paradigms;
3. Ability to write and otherwise communicate persuasively; and
4. Persistence.

3

THE CURRENT CONTEXT

Transformative research in the geographical sciences came of age in an era of strong federal and state investment in big and small science and the parallel development of America's system of higher education. Indeed, as the previous chapter conclusively demonstrates in the case of the geographical sciences, since the Second World War, government involvement as a funder of and a market for research findings was instrumental in the development of many kinds of new scientific and engineering knowledge. Interwoven among major economic downturns and international political conflicts, more than 60 years of funding for research represents the matrix into which the seeds of new ideas took root. While a counterfactual analysis designed to isolate the effects of six decades of support is impossible to conduct, the previous chapter traces the impact of five scientific endeavors undergirding the world as we know it today, enriched by transformative scientific discoveries in the geographical sciences.

As documented in Chapter 1, today's call for increasing support for transformative research is being made not only because of the practice it represents (how science is done), but also and importantly because today's most challenging problems require approaches that are of a scale and scope reminiscent of the early post-war decades. In an era of scarce resources and shifting priorities, funders are being asked to choose among competing visions of the future. This includes everything from protecting existing portfolios of projects and programs to having the license to redirect resources toward revolutionary solutions to problem solving (PCAST, 2012).

This chapter discusses the broader context of the study, and emphasizes how that context is today very different from the context in which many of the transformative ideas of Chapter 2 took root. Changing context provides an important ingredient to Chapter 4, where the committee considers possible recommendations. Thus this chapter focuses in part on the importance of transformative research as a propellant of the national economy at this time in U.S. history. Tracing the intellectual development of the field of geographical sciences highlights examples of transformative knowledge that not only fostered innovations in the geographical sciences, but informed the broader scientific community through conceptual, methodological, and technology spillovers. Interdisciplinary by nature, the field of the geographical sciences embraces the physical and social sciences, and to a lesser extent the humanities, while utilizing methodological approaches that encompass the spectrum of practices from bench science to surveys, to textual analysis utilizing computer algorithms that are aimed at describing the content, structure, and meaning of the written word.

This chapter is divided into four sections. The first section examines the forces of change that are reshaping the current environment for research, including changes in funding for basic research and higher education. The second section examines changes in demographics that are altering the demand for higher education at both the undergraduate and graduate levels. In the third section, developments in the international economy highlight the challenges facing America's former unrivaled position in research and development (R&D). As the fourth section demonstrates, developing countries of the Global South are vigorously pursuing the technology frontier utilizing capabilities fueled and funded by decades of strong national economic growth, and following an explicit strategy of catch-up. The U.S. system of higher education no longer stands alone, if it ever did. China, India, Korea, and Singapore (to name a few of the less-developed countries that were among the first out of the starting gate) are building world-class educational systems with annual output rates in the tens of thousands of college graduates, trained on the latest technologies. Utilizing policies that favor import substitution, industrial targeting, and strategic infrastructure investments, ambitious countries are offering viable alternatives to the historic geographic concentration of global research and development functions.

In summary, the committee sees the American research enterprise as currently facing four challenges:

1. Federal R&D funding levels are likely to decline, or at least remain stable, in the near term. Competition for scarce resources will pit existing programs and institutions against revolutionary developments.
2. After three decades of growth, state-level funding for R&D has stabilized and is in decline in many states.
3. In the near term, demographics point to a proportionately smaller cohort of individuals available to pursue undergraduate education. Pursuit of graduate education is responsive to levels of undergraduate debt, stagnant wages, and uncertainty over future investments in further education.
4. Developing countries are building educational systems capable of supplying the skilled labor that is required to attract R&D investments. The U.S. system of R&D and higher education now has rivals.

The four sections that follow discuss these challenges and provide the background for the fifth and final section. In that section the committee explores the potential role of transformative research in responding to these challenges, and possibly helping to mitigate them. Thus the committee sees transformative research, both broadly and in the geographical sciences in particular, as an evolutionary response to current developments.

It is also possible to take the view that transformation has always been characteristic of the U.S. research enterprise, and in Chapter 2 the committee documented the history of transformation in five areas of the geographical sciences. From this perspective, the committee sees a new call for transformative research as potentially countering the threat that these four challenges pose, through a systematic effort to rejuvenate a historic legacy.

THE AMERICAN SYSTEM OF KNOWLEDGE PRODUCTION: 50 YEARS IN THE MAKING

Transformative research is the hallmark of America's interlinked R&D and higher-education systems (Council of Graduate Schools and Educational Testing Service, 2010). Set on a course initiated more than fifty years ago, the modern U.S. research enterprise arose from federal and private-sector investments in science and technology that were aimed at winning the Second World War (National Science Foundation**,** 2002; Adams, 2009). These efforts would stimulate far-reaching experiments and unleash new trajectories of technology development (Dosi, 1982) that resulted in military and commercial applications that have borne abundant fruit over the past six decades.

During and after the war much of this research effort migrated toward universities (NRC, 2013) and government-affiliated research centers and labs. Mounting massive research projects required a scientifically and technically trained workforce that had never before been necessary. During (and immediately after) World War II, research laboratories were set up in key locations around the country (NRC, 2013). To serve the needs of the nation and the world, the military invested heavily in universities and research labs. Shortly after the war ended many of these labs were recombined and often dispersed to new locations. This type of diffusion supported the development of strategic and innovative research capabilities across the U.S. research university enterprise (NRC, 2013) and led to the formation of industrial clusters and complexes that spawned autonomous innovations that extended well beyond the original stimulus.

With over 100 Research I universities and another 99 colleges and technical schools designated as of high research intensity, 41 federally funded research and development centers, and 15 defense-sponsored university-affiliated research centers (Carnegie Foundation, 1973), the nation's innovative base fed off the human capital and ingenuity nurtured in these settings to produce new ideas and capabilities.

R&D FUNDING LEVELS IN DECLINE

The Federal Perspective

Calls for increasing the nation's emphasis on transformative research activities, such as that from the National Science Board (2007), are occurring simultaneously with an evident recent decline in federal funding for research and development, both in dollar terms and in percent of GDP (Figure 3.1). Setting aside the counter-cyclical funding awarded under the American Recovery and Reinvestment Act of 2009, which blunted the worst effects of the recent recession, the Congressional Budget Office estimates indicate that "since 2009, real federal R&D spending has declined by about 10%" (CBO, 2014, p10).

In recent decades the federal role in funding R&D activity has departed sharply from other countries around the world. As measured by R&D as a share of GDP, the U.S. is in a period of retrenchment compared with earlier decades (Figure 3.2). This shift has been accompanied by the rise of new competitors around the world that are devoting larger shares of national GDP to R&D activities. Starting at the end of the 1990s, the U.S. level of R&D intensity has been outpaced by many national economic growth trends, most notably in China, South Korea, and Taiwan (Figure 3.2).

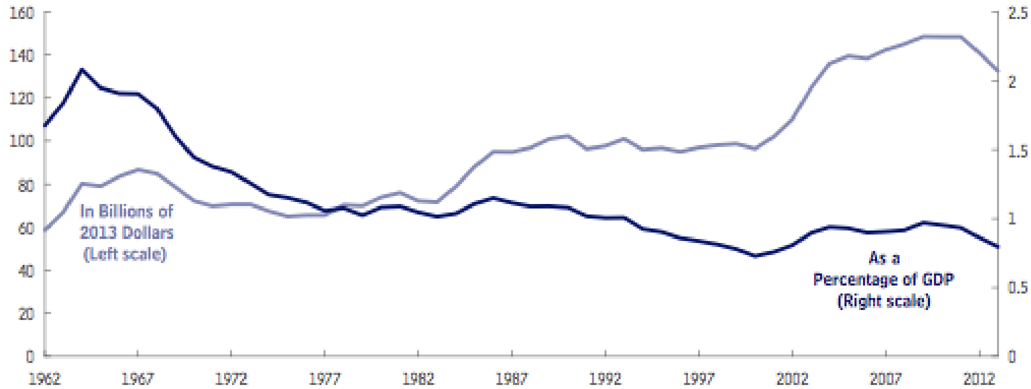

Figure 3.1 Federal outlays for the conduct of research and development, 1962-2012. The dark blue line shows declining outlays as a percent of GDP (right-hand *y* axis) while the light blue line shows total outlays in 2013 dollars (left-hand *y* axis). Source: Congressional Budget Office, 2014.

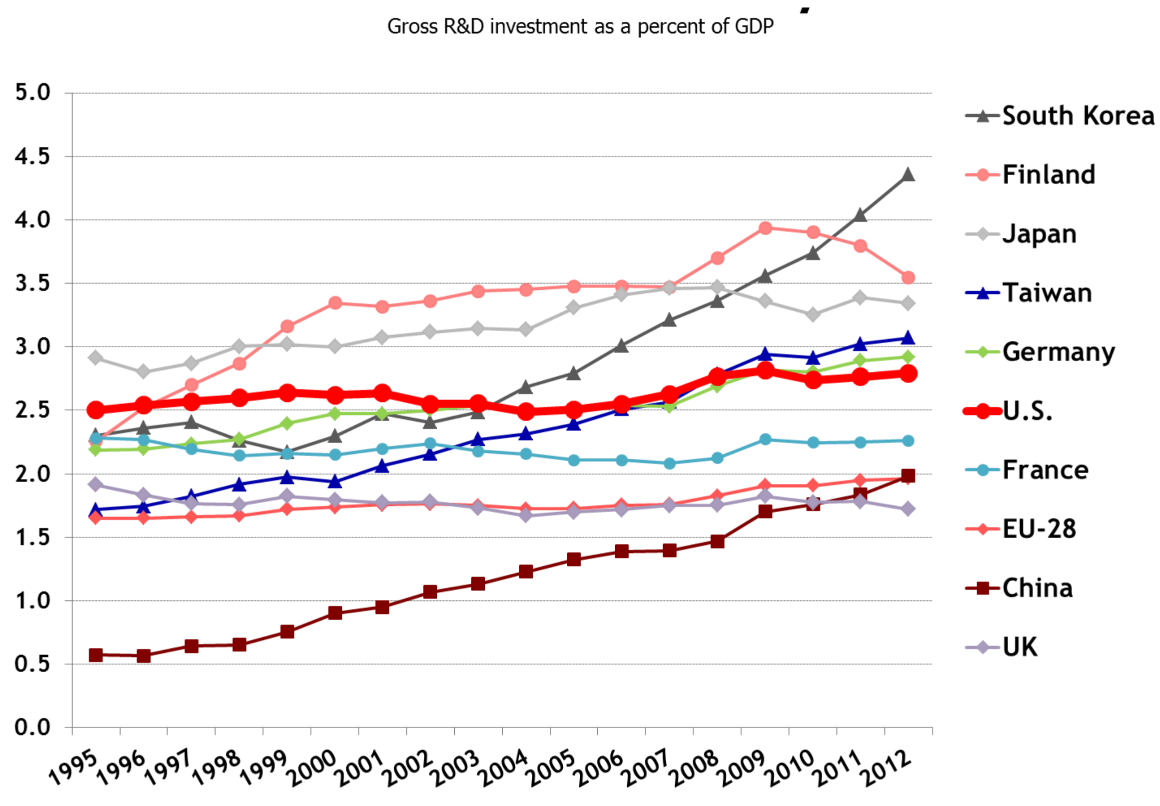

Figure 3.2 National R&D intensity in selected countries as a percentage of GDP. Source: (AAAS, 2014).

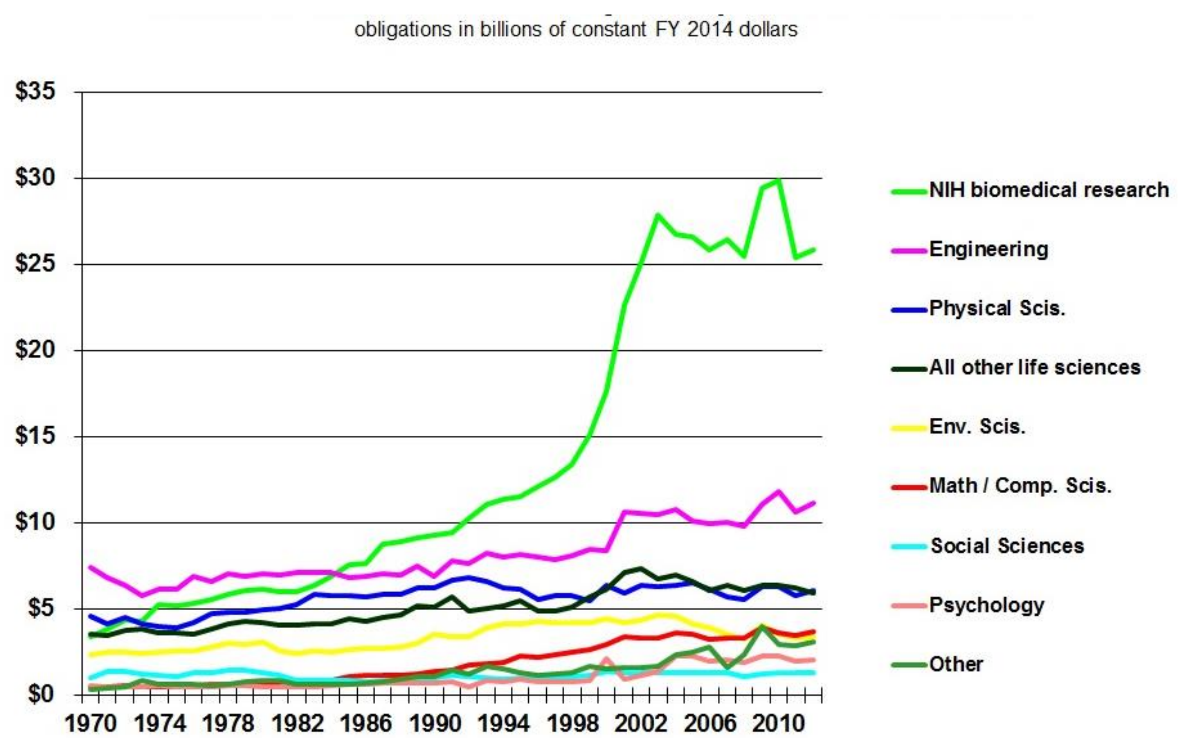

Figure 3.3 Trends in federal research by discipline, FY 1970–2012 in billions of constant FY2014 dollars. Source: AAAS; http://www.aaas.org/sites/default/files/Disc_0.jpg

Since the 1980s, fluctuations in federal R&D levels of funding for academic disciplines reveal an increasing concentration of funding in medical science (Figure 3.3). By contrast, the share of funding for the social sciences stayed tiny and essentially flat. Although the geographical sciences have been effective at garnering support through collaboration with other disciplines, the fact remains that at NSF their "home" program lies within the Social, Behavioral, and Economic Sciences directorate.

In past eras, federal-level retrenchment typically initiated reactions by universities, government research centers, and non-profit research enterprises to shore up funding sources and reprogram activities to fill in gaps (Douglass, 2010). But the R&D shortfall of the past decade cannot be remediated by incremental adjustments to current operating budgets. A major factor in the growth of university spending for research and development is the cost of operations, which universities have increasingly had to cover from their own resources. The university's share of R&D expenditures has grown to around 20% of the total R&D budget, up from 12% in the early 1970s (Howard and Laird, 2014; Figure 3.4). Faculty start-up packages and laboratory infrastructure are typically not covered by sponsored research funds and hence covering such gaps draws on university endowment and other discretionary sources of funds (Dorsey et al., 2009). This model is being called into question as universities and colleges face constraints on the level of support they can expect to receive from other sources including state government.

Fiscal Year	All R&D Expend.	Federal Govt.	State and Local Govt	Private Industry	Institutional Funds	All Other Sources
1956	100%	57.3%	14.2%	7.8%	11.6%	9.1%
1966	100%	73.5%	9.1%	2.4%	8.6%	6.3%
1976	***100%***	***67.4%***	9.8%	3.3%	***12.0%***	***7.6%***
1986	100%	61.4%	8.4%	6.4%	17.1%	6.7%
1996	100%	60.1%	7.9%	7.0%	18.1%	7.0%
2006	100%	62.9%	6.3%	5.1%	19.0%	6.7%
2012*	100%	61.0%*	5.6%*	5.0%*	20.8%*	7.6%*
2012†	***100%***	***59.5%†***	5.8%†	5.2%†	***21.6%†***	7.9%†

* Includes $2.4 billion in one-time supplemental funding appropriated under the American Recovery and Reinvestment Act of 2009 (ARRA).

† Excludes ARRA funding.

Figure 3.4 R&D expenditures by funding source as a proportion of all R&D expenditures by institutions. Source: Council on Government Relations, 2014. *Finances of Research Universities*. Washington, DC.

The State Perspective

Strengthening the nation's capabilities to perform transformative research requires cooperation among three jurisdictions of government: local, state, and federal. While the federal government has traditionally provided funds to cover the cost of research and development, states have covered much of the cost of human capital formation by creating and maintaining a national system of higher education. Despite recent decreases in state funding, state-supported institutions still educate three-quarters of all students enrolled in higher education (Oliff et al., 2013).

The impact of state budget constraints on university operations is far reaching. State support for higher education is at a 30-year low. From a high in 1975 of 60% among states for higher-education-related expenditures, by 2010 those contributions were less than 40%. Student spending levels are down and tuition has risen sharply in public institutions (Figures 3.5 and 3.6)

With the economy recovering slowly, some states continue to cut, while other states are beginning to replenish funding for higher education, but few expect a return to levels seen in the early 2000s (Oliff et al., 2013). A now familiar concern is the decreasing affordability of higher education. The lack of transparency evident in the budgets of higher education institutions is fueling public skepticism about the ability of university leaders to adequately address how rising costs are impacting families and students (Brand, 2014).

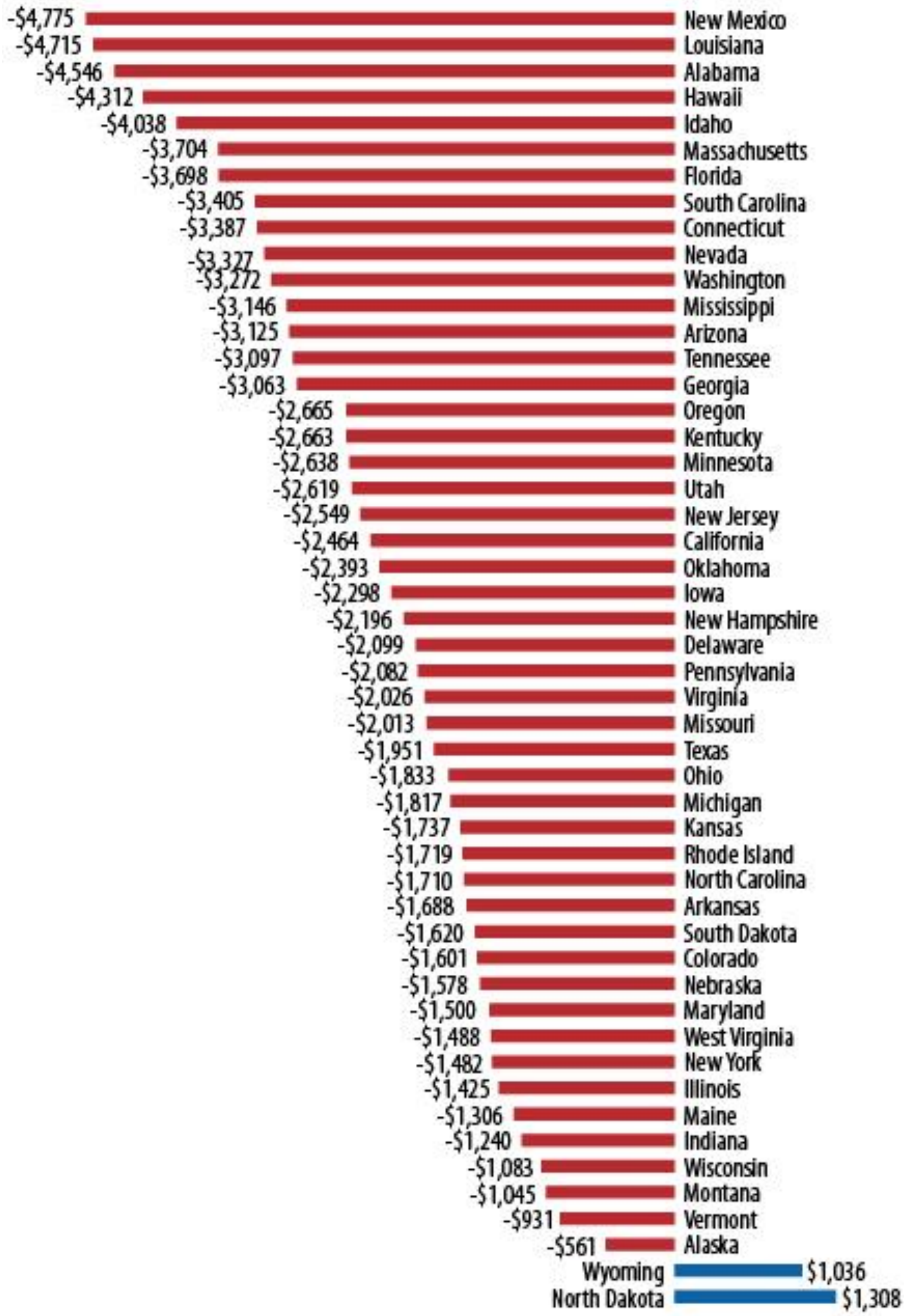

Figure 3.5 Change in state spending per student, inflation adjusted, FY08 to FY13. Source: http://www.cbpp.org/cms/?fa=view&id=3927.

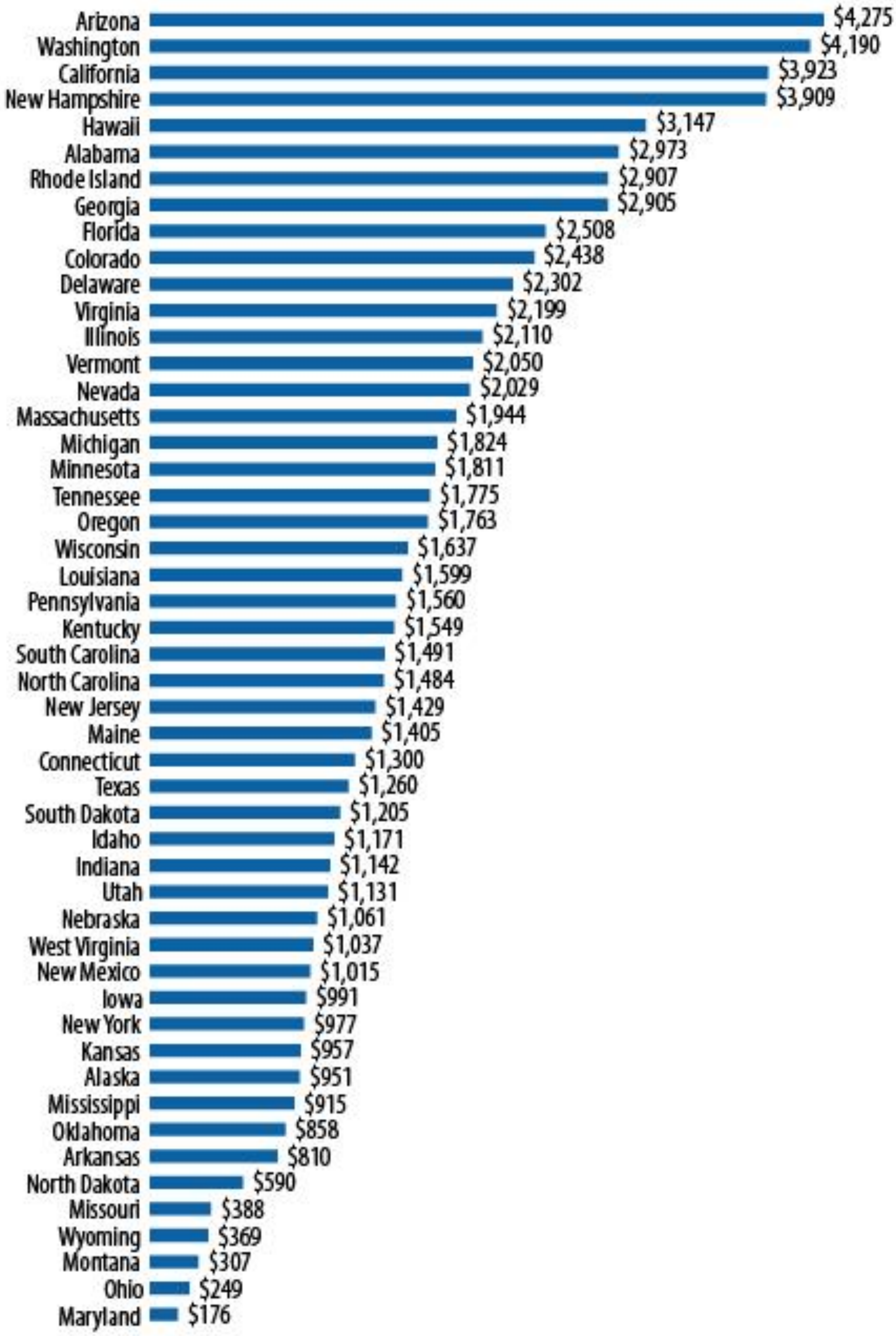

Figure 3.6 Change in average tuition at public, four-year colleges, inflation adjusted, FY08 to FY13. Source: http://www.cbpp.org/cms/?fa=view&id=3927

THE CHANGING DEMOGRAPHICS OF THE COLLEGE-BOUND POPULATION

The Production of Human Capital

The supply of and demand for higher education reveals looming deficits in student numbers at the undergraduate and graduate levels due to a weakening in the pipeline from high school through college to graduate school and the decreasing affordability of graduate education (Carnevale et al., 2010). Smaller cohorts, higher costs of education, and poorer labor-market outcomes are producing instability in the projected numbers of individuals ready for and available to pursue and sustain the system of higher education.

While the public concern about the affordability of higher education highlights the plight of undergraduates, the national research enterprise is fundamentally dependent on graduate students to fill classrooms, perform teaching duties, and staff research programs. Hence there is a link between an affordable undergraduate education and a pool of undergraduate candidates able to matriculate through graduate school.

The number of college-bound students is no longer inexhaustible; today, continuing increases in the number of students seeking a graduate degree are far from certain (Figure 3.7). The falling number of graduate school enrollees is masked by the continuing flow of temporary residents seeking graduate degrees from U.S. universities (Figure 3.8). To understand these trends requires dividing graduate degree-seeking students into two groups: U.S. citizens and permanent residents versus temporary residents. Seen in this way:

> *"The Council of Graduate Schools (CGS) today reported a 1.0% increase in first-time enrollment between Fall 2012 and Fall 2013. Despite the gains in first-time enrollment, total graduate enrollment fell 0.2% between Fall 2012 and Fall 2013 following a 2.3% decline in the previous year. Total graduate enrollment was about 1.7 million students in Fall 2013. While first-time enrollment of U.S. citizens and permanent residents shrank by 0.9%, the increase of 11.5% in first-time enrollment of temporary residents was enough to push the overall rate of change into positive territory. Temporary residents represented one-in-five new graduate students at U.S. programs in Fall 2013." (Allum, 2014).*

The sluggish trend in Master and Ph.D. enrollments must be seen in the light of similarly sluggish growth rates in occupations in higher education that require advanced degrees. In an assessment of Bureau of Labor Statistics data on projected job openings 2010–2020, the Council of Graduate Schools reports that job growth is expected to be insufficient to absorb the number of qualified candidates as projected over the decade. Academic careers are no longer as attractive as they once were, and universities are often accused of using part-time lecturing positions to substitute for high-cost full-time, tenured positions. The Council's analysis further suggests that the BLS figures undercount the number of existing graduate degrees in the labor market and hence the future demand for jobs by graduates over the decade (Bell, 2012).

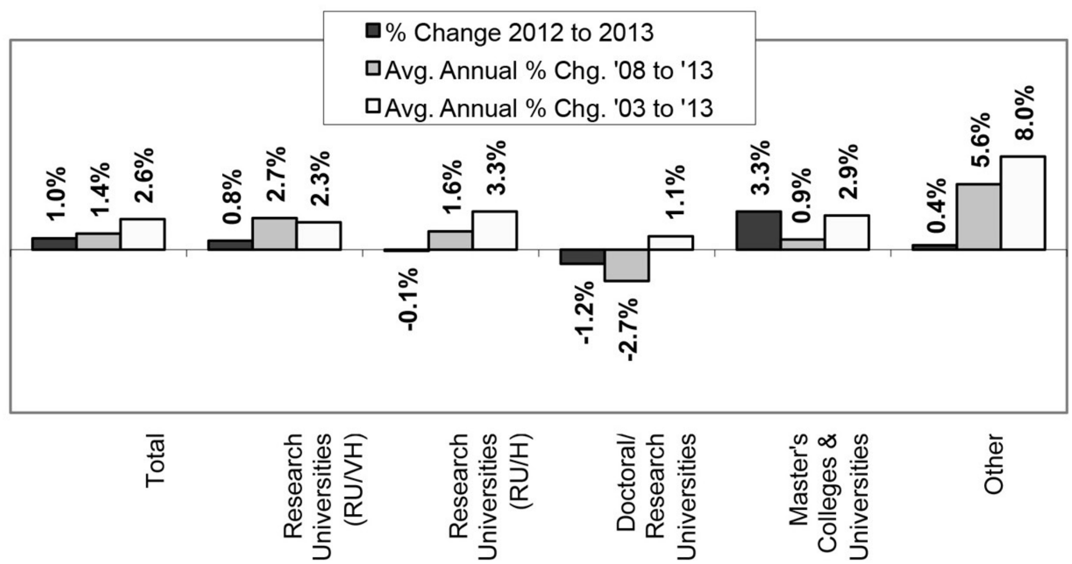

Figure 3.7 Trends in first-time graduate enrollment by Carnegie Classification, Fall 2003 to Fall 2013. Source: Allum (2014).

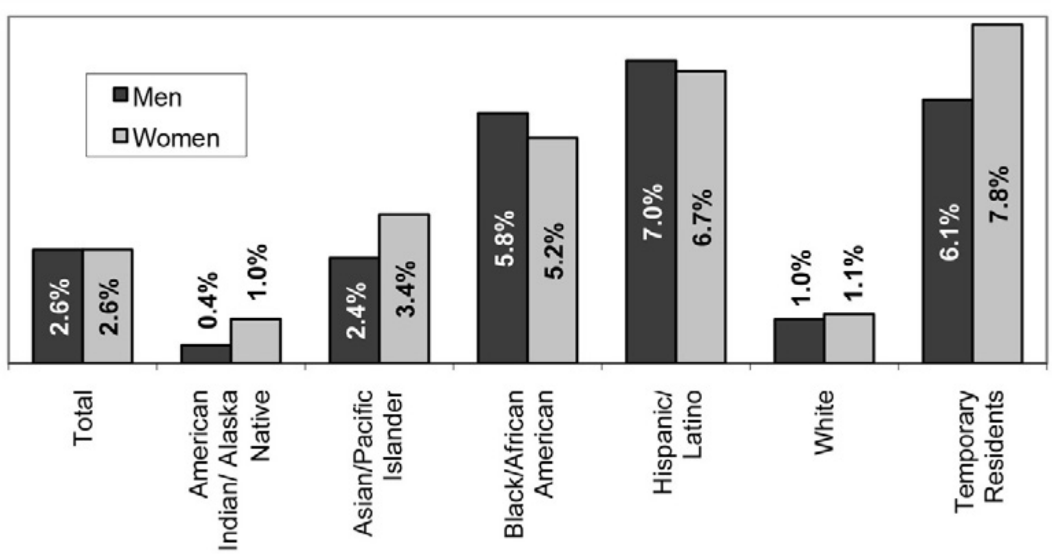

Figure 3.8 Average annual percent change in first-time graduate enrollment by citizenship, race/ethnicity, and gender, Fall 2003 to Fall 2013. Source: Allum (2014)

COMPETITION IN THE GLOBAL MARKET FOR R&D AND HIGHER EDUCATION

Knowledgeable observers suggest that the U.S. is entering an era in which the economic stimuli accompanying prior investments in discovery are diminishing—an era that has been termed "technological statis" (The Economist, 2013a). This view is not held simply by popular writers and an occasional off-beat scholar, but peppers the conversation of policy economists and business leaders (Porter and Rivkin, 2012; Howard and Laird, 2013; Gordon, 2014; Summers, 2014; Summers and Balls, 2015). The range of causes aside, many economists have argued that the great U.S. innovation machine has reached middle age and is slowing relative to its younger and more nimble, newly industrializing counterparts (The Economist, 2013b; Summers and Balls, 2015). Calls to follow new directions are challenging the status quo, with beneficiaries of the existing system pressuring to stay the course while members of the next generation of scholars and practitioners are arguing for support of transformational practices and emergent modalities in science. Important influences include “the rise of the rest,” a phrase coined by the late Alice Amsden to recognize the role of the newly industrializing countries and their contributions to the world’s growing R&D capabilities. She along with others speaks to the emergence of geographic configurations of corporate R&D and the resulting supply chains that anchor a new geography of innovation founded on transformative research practices and new modalities of inquiry significantly beyond what was previously considered possible.

Evidence is mounting that the forces of globalization are enabling the formation of new sites of innovation, which are in turn pushing to equalize inventive ability between the Global North and the Global South (Matthews, 2014). Examples from China, India, and South Korea suggest countries of the Global South are applying lessons from macroeconomics, targeted industrial investment, venture financing, and human capital formation to forge inimitable expressions of autonomous innovation. With long histories of strong and tested state-led development policies and an unbridled quest for global leadership, countries with high and stable rates of economic growth have increasingly abundant concentrations of resources with which to compete, whether it be in the form of human resources or flows of capital (Amsden, 1992). These in turn are being targeted toward the design of systems capable of supporting transformative research (Figure 3.9).

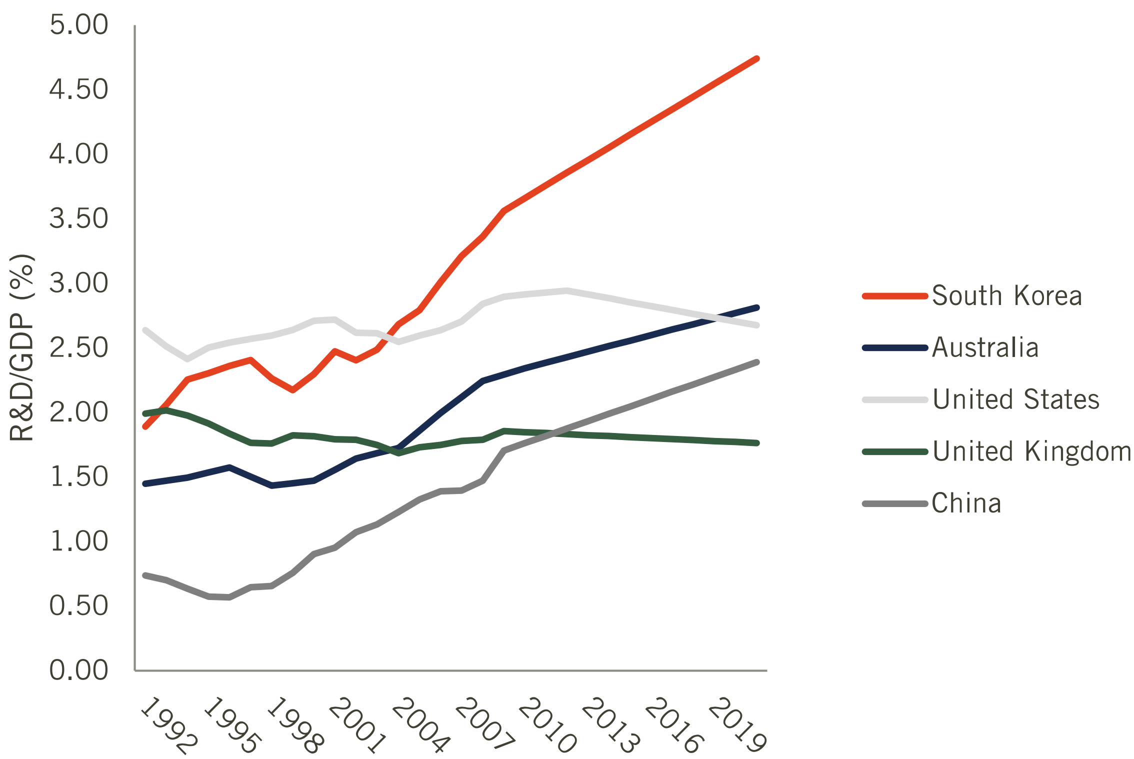

Figure 3.9 R&D as a percentage of GDP. Source: Hicks and Atkinson, 2012.

Nations around the world are building their own higher education systems, offering incentives to retain the best students to fill classroom seats and benches in laboratories (Freeman, 2013a,b). This growth of university systems across the globe is yielding startling increases in the worldwide availability of skilled labor. Nations experiencing rapid rates of GDP growth over the past two decades have been investing in higher education and R&D operations. A comparison across countries in the expenditure for R&D tells a powerful story of increasing competitiveness of countries in the Global South. Freeman (2013a,b) shows that China and other developing countries have significantly increased the share of GDP that they spend on R&D. Countries of the Global South are also outcompeting industrialized countries in the production of academic publications. Freeman further shows that China made a particularly large gain in its share of value-added activities in knowledge- and technology-intensive sectors, leading to large increases in exports of goods with high added value.

Thus, even if the current flow of foreign students into U.S. Ph.D. programs stays the same, the production of Ph.Ds. globally is likely to grow. Moreover, should the flow of foreign students into U.S. graduate programs slow, empty seats will need to be filled by U.S.-born students.

The location of demand for talent is also shifting as corporations decouple laboratory and testing functions previously co-located in high-wage labor markets like the U.S. Favorable circumstances abound offering support for and occasional subsidies to enhance the mobility of formerly "geographically sticky" (Markusen, 1996) laboratory operations. In turn these same conditions are enabling new business supply chains to form in places like India, China, and Korea where technical skills are less costly than in the U.S.

Today the U.S. is just one of many countries around the globe where research can be conducted (Porter and Rivkin, 2012; Bremmer, 2014). The sentinel case is in drug development. Michael Herper of Forbes reports that the development of a new drug costs upwards of $5 billion (Herper, 2013). To reduce the cost and time to bring a product to market, U.S. and global drug manufacturers are engaging Contract Research Organizations (CRO) in China and India to complete critical and costly phases of the drug development process. Like turnkey microelectronics development of the 1970s to the 1990s, China has developed a world-class capacity to execute critical components of the drug development system.

THE ROLE OF TRANSFORMATIVE RESEARCH

Looking over the past sixty years the pursuit of new knowledge and the nature of scientific inquiry have changed fundamentally. The scientific community currently finds itself in an era of inquiry where the work of science is global in scale, multidisciplinary in scope, and unbounded by national geographies. Moreover the types of problems underlying today's research questions are of a profound and unique nature, calling for joint investigation and cooperation in line with causal forces and resulting consequences. These grand challenges—climate change, global food supplies, energy security, geopolitical conflict, scarcity of strategic minerals, deadly epidemics, rapid urbanization toward a global urban future—all point toward a new modality of science. Leadership now will come from shifts in the model of R&D performance.

Like many endeavors, research and development is increasingly being judged on more than simple flows of inputs and outputs. Emphasis is now on outcomes and the consequences of investigative effort. This intersectionality represents new requirements and practices as innovators and early adopters, once juxtaposed in linear fashion, pursue the act of discovery through synergistic feedbacks between these groups.

Synergism is the new modality of discovery and it will serve to redefine the conceptual valuation of our approach from individual or comparative to collective advantage. The acts of problem identification and specification are growing in complexity as the scale and scope of challenges encompass far-reaching dimensionality. Reducing the spread of threats such as Ebola necessitates boundary spanning as a core competency and starting condition; neither time nor resources are sufficient to wait out a process of scientific discovery advancing through stages of the unknown to the known.

Translating across disciplines will remain one of the core challenges in this era of synergistic science. Over the past sixty years, the U.S. has passed through distinct eras of discovery, each in their own way presenting challenges of translation and transformation. Now the practice of translation and transformation must be a core tenet of future science. Moreover, the U.S. will be at an advantage as an English-speaking nation as long as English remains the international language of science.

Calls for the redesign of the national research enterprise often correlate with budgetary exigencies. Looking back, however, other equally powerful forces have redefined the trajectory of innovation: wars, disasters, profound discoveries, new institutional practices, and social objectives such as expansion of medical sciences. Today's call for transformative research can be understood as reflecting the convergence of several of these forces.

At the same time, we are living in a time of global unrest. Two decades of war and unresolved conflicts coincide with state-led development practices of a potentially new kind. Today, national governments are operating beyond their geographic borders in pursuit of national economic development objectives, including market and commodity resource domination. The growing number and scale of multi-billion dollar sovereign wealth funds of countries such as China or Saudi Arabia, financed either from foreign exchange flows earned through the sale of goods and services or from petroleum dollar-denominated revenues received from high-priced barrels of oil, are erecting new patterns of relations that are challenging postwar era institutions such as the World Bank; arrangements emplaced after an era of global conflict and aimed with the goal of global stability and widespread progress. Secure in the belief that free trade aligned with democratic ideals would bring forth global economic growth and geopolitical stability, the liberal ideal represented in institutions dating from the Bretton Woods meetings are now open to at least some question. Much has occurred since the late 1940s. The premises underlying the original accord have shifted and the development challenges of Asia loom large in comparison with the rest of the world. Under the leadership of the Chinese, the recently created Asian Infrastructure Investment Bank marks an effort to refashion global leadership roles and attendant development institutions to be more in line with 21st Century partnerships and development challenges. The recent signing on of Britain, France, Germany, Italy, and (reluctantly) the United States suggests that original signatories to the Bretton Woods agreement also see that it is time for a change.

What of the governance of science in this emerging era? Through periods of peace and conflict, the pursuit of knowledge has remained inviolate, even as it has been refashioned to meet the ever-changing needs of society. Transformative research should be at the forefront of conversations today; as in the past, the language and understanding not only transcend conflict but often serve as the translation medium during periods of societal change.

To break with convention is not easy, and to confront unconventional forces of change is even more challenging. At an operational scale, proponents of resculpting the research enterprise take aim at past and current practices. These issues form just one layer of needed change that is structural in nature. Far deeper meaning is accompanying the message of change. The push for transformative research is not just a call in support of more innovation, nor is it simply a reaction to a shift in political will to finance basic research.

Accompanying transformations of the external environment are changes in scientific problems and their study. An emerging field of research on team science is beginning to yield an understanding of a set of key factors present in effective collaboration (Stokols et al., 2008). The prototypical model of science, one in which the research efforts of the lone scientist are enabled by, and to some extent rewarded with research funding, is giving way to a model based around teams and small groups. While the lone scientist model is not disappearing, it is evolving in a recognizable era of collective or team science in which problems under investigation are complex and system-wide in scale.

Support for team science is rising alongside calls for transformative research—in other words, a continuing search for game-changing practices and investigations of far-reaching ideas (Pennington et al., 2013). These two circumstances are converging and unfolding in a period of reduced expectations. Economic and political events serve as reminders that for the foreseeable future, global leadership is variable and fleeting and hence national policies to maintain and strengthen a nation's current position necessitate deliberate and planned interventions.

A new design, ethos, and strategy are required at this moment. To this new trajectories and imperatives must now be added to ensure that as the unsurpassed position that the U.S. has held since the 1950s melts into the global research enterprise, exploration and innovation are fostered in the interests of both material and intellectual transformation.

4

FOSTERING TRANSFORMATIVE RESEARCH TODAY

The three questions of the committee's charge are all to some degree retrospective, asking the committee to examine past events and practices in the geographical sciences. Accordingly Chapter 2 focused largely on the past, addressing the questions of the charge directly, and building on the discussions, inputs, and evidence presented to the committee at its workshop. Rogers' model of the diffusion of innovation provided a useful framework for a discussion of the various stages of transformation, and for efforts that could be made at each of those stages to foster the dissemination and adoption of novel concepts and approaches. Five examples of transformation in the geographical sciences were discussed and a variety of themes emerged: the importance of individuals both in the creation of the transformative idea and in its dissemination; the importance of technology and infrastructure in stimulating and enabling transformation; the importance of open communication and sharing, both within the geographical sciences and across existing or perceived disciplinary boundaries; and the importance of funding across a range of scales. Chapter 2 concluded that transformation had long been characteristic of the geographical sciences, and provided many suggestions for how that tradition might be continued, and potentially enhanced.

By contrast, Chapter 3 reviewed the current state of higher education in the U.S., and its research arm, as an essential ingredient to any discussion of how to foster transformative research. It identified four distinct challenges, all of which threaten to shrink the role that universities and colleges have played in fundamental research since World War II, and thus eventually to undermine the strength of the U.S. economy. It thus stressed the essential value of fundamental research, and especially the value of transformative ideas, and cited many warnings to the effect that the post-war engine of U.S. research dominance may be flagging.

With this background, this fourth chapter addresses the second part of the third question of the committee's charge: how transformative research "can be fostered in the geographical sciences." Chapter 2 established that transformative research has been of critical importance to the geographical sciences in the past, and Chapter 3 established that transformative research is of critical importance to the U.S. economy at a time when higher education faces severe and largely unprecedented challenges. In this final chapter the committee presents ideas and recommendations that it believes will help to guide the Geography and Spatial Sciences (GSS) program at NSF as it plans its future.

Throughout the chapter the committee assumes that fostering transformative research is desirable. There are, of course, good reasons for arguing against such a strategy. Transformative research carries high risk, almost by definition, requiring reviewers to accept what may well be a less-than-complete research plan, great uncertainty about the nature of the results, and the possibility of negative results and failure. A proposal for transformative research almost inevitably requires the reviewer to trust the investigator to an unusual extent, and may in some cases expose the funding agency to negative comment in the press or on Capitol Hill, or from other researchers, when an award is announced. This may be especially true in the social sciences, which are periodically subjected to critical examination in Congress (Lempert, 2013). In a zero-sum world, sequestering funds to support proposals that are deemed transformative may in turn penalize more conventional but nevertheless important and useful work. At the same time the NSB report and related commentaries from the NIH Director and other sources cited in Chapter 1 make a compelling and exciting, and at times almost passionate case for greater support for transformative research, despite the associated risks.

This chapter is structured as follows. The next section addresses education, and the possibility of a long-term encouragement of transformative research among students through increased emphasis on critical, independent thinking in the research-formulation process. This is followed by a discussion of the research culture in the geographical sciences, and how it might be more conducive to transformative research; by a section on the issues raised by traditional practices in academic career advancement; and by a section on practices in research funding.

INITIATIVES IN EDUCATION

For research to be transformative in the NSB report's definition, it must "challenge" or "radically change" existing thinking, practice, or concepts. As such, it strikes to the heart of a fundamental paradox in education: the need on the one hand to convey the accepted and established knowledge and practices of a field, but on the other to encourage critique, skepticism, and independent thinking on the part of the student. Critique, skepticism, and independent thinking require confidence on the part of the student, and a willingness to challenge teachers and peers; and may sometimes require teachers and peers to endure a level of discomfort. Some cultures are clearly better at this than others: Confucianism, for example, a strong system of beliefs within traditional Chinese culture, encourages respect for elders and may well frown on youthful independence of thought; the effects on Chinese science have been discussed at length by Gong (2012). Critique, skepticism, and independent thinking may also be lacking in European concepts of *apprenticeship*, the notion that practitioners pass on their knowledge and skills to the next generation through close, often one-to-one relationships. What is needed may be a new concept of apprenticeship in the practices of critique, skepticism, and independent thinking, where success may well be uncomfortable for the mentor, since it will need to be measured in the degree to which the student challenges the mentor.

Recent interest in the pedagogic approach sometimes termed *discovery-based learning* or *student-centered learning* provides one possible approach to this dilemma. Rather than being instructed, the student is encouraged to experiment, and to construct knowledge from scratch; the instructor's role focuses on facilitation rather than direction. The concept is succinctly expressed in the mantra (often attributed to Benjamin Franklin but also traceable to many others including the early Chinese philosopher Xunzi): "Tell me and I forget. *Teach me and I* remember. Involve me and I learn". But though these ideas have gained some traction in the educational system, the goal of encouraging students to think critically, creatively, and independently remains elusive. Discovery-based learning can be inefficient and time-consuming, if every fact has to be established from first principles, and one is reminded of Bertrand Russell's anecdote: presented with Euclid's axioms as a student, he was initially skeptical, but agreed reluctantly to accept them when it was pointed out that only then could he be introduced to what lay beyond.

How do the geographical sciences fare on this dimension of encouraging critical thinking through the educational process? Many geographical scientists first encounter the discipline after their freshman year, since geography is virtually absent, or treated very superficially, in primary and secondary education in the U.S. Thus in some ways they come to the discipline with a mind that is not encumbered by established ways of thinking in their adopted discipline. At the same time the distinct perspective that the geographical sciences offer—the emphasis on framing in space and time, and on integration of human and physical processes—may well encourage students to take a fresh and critical look at what they have previously learned in other disciplines.

As Susan Hanson noted in her keynote address at the workshop, the U.S. system is productive at least in part because it is "complex, dynamic, pluralistic, decentralized, competitive, meritocratic, and entrepreneurial". Educators in the geographical sciences would do well to place similar emphases in their pedagogy, especially with respect to the first three descriptors, if they are to prepare students to contribute to transformative research.

NSF has many programs and an entire directorate (Education and Human Resources, or EHR) devoted to STEM education, though the size of these efforts is inevitably small in relation to the overall magnitude of the U.S.'s entire investment in education. GSS receives proposals under the REU (Research Experience for Undergraduates) and RET (Research Experience for Teachers) programs, and also participates with EHR and other directorates in making additional types of awards in support of geographic education at all levels. While these awards often give students valuable lab and research experience, expose them to the planning and management of research, and train them in research methods, the awards might also be seen as opportunities for fostering transformative research, by engendering the kinds of critical, creative, and independent thinking that such research requires. As the committee noted in Chapter 2, it is easy to trace the influence of charismatic and provocative mentors on the individuals who in turn contributed and led new transformative research.

Yet many students must pass through their graduate years without ever encountering the kinds of mentorship that might lead them to be transformative in their own careers. To address, this, PIs might be encouraged to include and even emphasize relevant characteristics in their proposals:

- How and to what degree will students supported by the proposal be exposed to the concept of transformative research?
- How and to what degree will students supported by the proposed award be encouraged to think critically, creatively, and independently?
- How and to what degree will they be exposed to the nature and impacts of prior transformative research in the geographical sciences?
- How and to what degree will students with backgrounds in other disciplines be encouraged to learn and apply the perspectives of the geographical sciences to problems?
- How and to what degree will students with backgrounds in the geographical sciences be encouraged to learn and apply the perspectives of other disciplines to problems?
- How and to what degree will students with backgrounds in the geographical sciences be encouraged to apply the perspectives of geographical research to problems in other disciplines?

Students should be informed about the concepts of transformative research and the importance of such research. As noted in Chapter 1, transformative research requires the existence of an initial state, a paradigm or set of practices that is to be transformed. It follows that the initial state will be replaced, and that existing knowledge and practices will be abandoned. Thus students should understand that transformative research implies a process of *unlearning* that may be as important as the new learning; and the rejection of old ideas and techniques may be as important a part of the transformative process in a science as the identification of new ones. Long-abandoned ideas in science, such as the ether or phlogiston, are now encountered in science courses only as historic curiosities, although they are useful examples in the history and philosophy of science. In the geographical sciences abandoned ideas, such as environmental determinism, still live on to some degree in courses on the history of the discipline or on geographic thought. Other old ideas periodically resurface as they are found to be useful to some new direction of research, or are rediscovered by researchers who are asiduous in searching the literature.

Finally, it is important for students to understand that the identification of new questions may be as important to transformative research as the discovery of new knowledge. What is most valuable about a new technology in science may not be how it allows us to answer old questions more effectively, but how it allows questions to be asked that have never been asked before. For example, the application of high-performance computing in the geographical sciences (see, for example, cybergis.illinois.edu or Wright and Wang, 2011) is initially seen as a way of speeding up operations, allowing analyses to be performed in half, a tenth, or a hundredth of the time. But such speed-up is not in itself transformative; the long-term value of high-performance computing in the geographical sciences will only come when it is possible to ask and answer an entirely new set of questions by thinking beyond the mindsets, conventions, and assumptions of the past and the constraints that have been imposed, consciously or subconsciously, by traditional serial computing. In short, new questions may be more valuable to science than old answers.

Recommendation 1: GSS should examine the degree to which its awards, especially those in support of geographic education, foster the potential for transformative research among the students who benefit from these awards, and encourage PIs to give attention to such potential in their proposals.

THE RESEARCH CULTURE

Research in the geographical sciences is housed in many disciplines, primarily geography but also computer science, cognitive science, statistics, and engineering, and also in the disciplines that apply the knowledge of the geographical sciences, including all of the social and environmental sciences and increasingly the humanities. As Chapter 2 demonstrated, the geographical sciences are already a multidisciplinary culture, in which collaboration across the boundaries of the traditional disciplines is not only common but encouraged, and in which transformative ideas have often stemmed from such collaboration. The walls of the geographical sciences are inherently permeable, if indeed they exist at all, though objective evidence of such permeability would be very hard to assemble, especially in comparison with other disciplines.

There are many ways, however, in which the diversity and pluralism of the geographical sciences could be strengthened, and note has already been made of the importance of diversity and pluralism in fostering transformative research. The community of researchers in the geographical sciences falls a long way short of “looking like America”, despite NSF’s strenuous and longstanding efforts in this direction. Women, GBLT, and ethnic minorities may still be underrepresented, especially in certain areas of the geographical sciences, despite the potential for such groups to bring new, transformative ideas to the research table. Recent data from NIH shows that the age distribution of funded PIs is strongly weighted in favor of older researchers, and does not match the ages at which researchers tend to make their most significant discoveries (Harris, 2014).

Older, more experienced faculty can play a vital role in mentoring those still early in their careers. A prominent, well-funded late-career scholar who now regularly encounters the argument that scarce funding would be better awarded to those who have yet to make their mark might do well to partner with a young scholar with new but perhaps less well developed ideas and no history of funding.

International collaboration could also be strengthened. While collaboration with Europe and many Commonwealth countries is common in the geographical sciences, and collaboration with researchers in China is increasing, collaboration with many other parts of the world remains adversely impacted by differences in language and research culture, problems with travel and communication, personal security, and the lack of bilateral or multilateral funding programs. Yet such collaborations could be enormously stimulating, bringing a host of new ideas and perspectives. International links might be cultivated through non-governmental organizations that operate internationally.

The relationships between academic geographical scientists, industry, and the military and intelligence communities have long been a source of debate. Mapping, GIS, and related services are now a multi-billion-dollar industry, but many academics are hesitant to develop links, fearing that the objectivity of their research will be compromised by the commercial objectives of industry. Yet Esri, for example, employs close to 5,000 people worldwide, a large proportion of them reasonably described as geographical scientists, and reports annual expenditures on in-house research in the hundreds of millions of dollars, a figure that is two orders of magnitude greater than the budget of NSF’s GSS program. Although figures are inevitably hard to come by, the military and intelligence community must employ many more, and invest much more. Cloud and Clarke (1999) argue that many of the most significant and transformative advances in GIS and remote sensing originated in this community, and Chapter 2 reinforced this conclusion. But the debate over the Bowman Expedition of a few years ago (http://americangeo.org/bowman-expeditions/) underscores just how hesitant many academics feel about interaction with this vast and well-funded domain.

Much could be done to foster increased interaction with industry, the military, and the intelligence community. Exchange and internship programs could provide increased opportunities for exposure to new and potentially transformative ideas. GSS could encourage PIs to increase interaction, and to include representatives of these communities on advisory boards, at workshops, and in webinars. Esri and other companies could be encouraged to provide more opportunities for academic researchers in their annual user conferences, and academics could lobby for an increased role in the United States Geospatial Intelligence Foundation, the National Geospatial Advisory Committee, and other cognate organizations.

Behind these ideas lies a broader concern for the openness of science, and the free and timely exchange of new results and new questions. As Chapter 2 made clear, open collaboration has been critical in the development and dissemination of many transformative ideas in the geographical sciences. Open Science has recently become a compelling and rapidly growing movement, urging open access to journals, the use of open-source software, and open sharing of data. Of course no one is in favor of closed science, whatever that may mean, but it is easy to be in favor of science being more open than before. Transformation in science, in the form of openness, can only help to foster transformative research.

NSF has a long history of support for workshops, and other small gatherings of the scientific community. These workshops can be invaluable in giving NSF early access to new ideas, and opportunities to develop new funding programs, or redirect existing ones, so that such ideas can be pursued. Moreover, NSF rules allow program officers to provide the level of funding needed for a small workshop quickly, without the delays of external review. In the geographical sciences, a long series of *specialist meetings* organized by the National Center for Geographic Information and Analysis, many of them held in Santa Barbara, have brought together 30 to 40 researchers at a time to discuss and share new ideas in an area of cutting-edge geographic information science. Some 50 such meetings have been held over the past 25 years. The groups are multidisciplinary and international, and strenuous efforts are made to encourage participation by under-represented groups. Industry, the military, and the intelligence community are frequent participants. Such meetings act as community-builders, redirecting researchers toward collaborative, timely, and potentially transformative research on cutting-edge ideas.

Several foreign examples now exist of the value of multidisciplinary, multi-sector research collaborations in GIS and remote sensing, though no comparable project yet exists in the U.S. In Australasia the Australia/New Zealand Cooperative Research Center in Spatial Information brings together academic, industry, state and federal governments, and local entities to undertake high-impact, collaborative research that can be demonstrated to lead to accelerated industry growth, improved social well-being, and a more sustainable environment.

More fundamental, however, is the question of how the research community identifies and prioritizes its research topics. What makes a researcher identify one topic as “interesting” and reject another? How many potential topics are rejected out of hand, or subconsciously, because they do not fit within preconceived notions of what is “interesting”? If the existing practices of science were established by a community that was dominated by white males, what might they look like if the community had been more representative of America, or dominated by Hispanics, for example? Would this have given us a different conception of “interesting”, and greater success in fostering transformative research?

To a degree the process of topic selection is clearly opportunistic, driven by access to novel sources of data and new tools. For example, the discovery of the Dead Sea Scrolls between 1946 and 1956 led to a burst of new discoveries in the humanities, and answers to questions that had mostly not been formulated previously. But to graduate students the selection of a dissertation topic is often daunting, requiring careful navigation through a minefield of issues, some of them objective, such as access to data and tools, but many of them decidedly subjective, including the established practices and agendas of their advisors.

Recommendation 2: GSS should continue to emphasize NSF policies and programs that are designed to increase ethnic, age, and gender diversity among its awardees.

Recommendation 3: In the interests of fostering transformative research, GSS should also recognize the importance of research collaboration between nations, between disciplines, and between academics, industry, government, and the military and intelligence communities.

CAREER ADVANCEMENT

It has been common in recent years to blame the system of academic career advancement for many of the limitations and ills of the academy. Evaluations of candidates for advancement are seen as excessively dependent on the individual's research, at the expense of excellence in teaching or public service, especially at Research I universities. More relevant here is the view that evaluations are excessively discipline-centric, working against contributions at the edges of disciplines, where the committee has argued that many transformative ideas can be found. They also focus on the traditional system of academic publication and dissemination, emphasizing journals, books, and refereed conference proceedings, which have their own rules of content, and working against contributions in the form of tools, data, outreach, and institution-building, all potential contributors to transformational research.

Recently the process of research evaluation has become even more quantitative, through the use of readily available bibliometrics, and less dependent on the independent, detailed, and qualitative evaluation by a candidate's immediate peers. Evaluation based on bibliometrics may encourage a candidate to partition contributions into least publishable units, to seek co-authorship wherever possible, and to place emphasis on journals with high impact factors instead of those most likely to communicate results to the most interested colleagues. Referring to one of the most important advances in computer science in recent years, as measured by its impacts on society, the recent NRC report *Furthering America's Research Enterprise* (NRC, 2014) noted: "Bibliometrics, for example, would not have flagged the supporting citations in the patent application for (Larry) Page's Google search algorithm as particularly high impact during the years surrounding the initial appearance of those publications. (Moreover,) Page's discovery of the algorithm itself was first reported in *Computer Networks*, an archival journal with a relatively low impact factor (a measure of the average number of citations of articles published in the journal) of 1.2, as determined by the Institute for Scientific Information." (Chapter 4, p. 14). Thus, "…metrics can limit the possibility of transformative innovation by fostering an avoidance of failure to make the metrics look good." (Chapter 4, p. 15)

Reliance on bibliometrics has begun to penetrate many critical parts of the academic system, including the hiring, promotion, tenure, funding, and annual evaluation processes. Some graduate schools expect their students to have published a minimal number of papers prior to graduation. Department chairs may go so far as to formalize expectations regarding the number of papers published per year, and papers required for tenure. All of this may leave junior scholars with little incentive to pursue high-risk topics

Career advancement practices may also emphasize individual activity at the expense of collaboration, especially across disciplinary boundaries, another form of academic activity that the committee has identified as important in fostering transformative research (though the committee would not argue that collaboration is a necessary condition for transformation, and Chapter 2 provides many examples of the influence of individuals). These practices stress sole authorship and lead authorship, and candidates can be criticized for being named last in a long list of coauthors, on the grounds that this may indicate only a peripheral contribution, although it may also reflect a last name that is towards the end of the alphabet. Junior faculty who build collaborations with other disciplines, and benefit from the stimulus and cross-fertilization that results, may in the end be penalized in a discipline-centric system. Organizing workshops, building networks of colleagues, and pursuing large awards of external funding are all significant contributions that can foster transformative research, but all are often discouraged at early career stages when the potential for truly original ideas and discoveries can be highest.

In short, the traditional methods of evaluating candidates for academic appointments and for career advancement may not provide the best indicators of potential for transformative research. Instead, "The key players in transformative breakthroughs often are well-trained researchers from diverse backgrounds who know the right people—and many of them. The right people are other talented researchers who can draw on their knowledge of diverse fields to bring fresh perspectives to stale problems." (NRC, 2014, Chapter 3, p. 6) And again, "Truly transformative scientific discoveries often depend on research in a variety of fields, from which connections can be made that lead to new ideas." (NRC, 2014, Chapter 6, p. 11)

While these arguments have been presented in the context of career advancement, they are also clearly relevant to the evaluation of research proposals and to efforts to foster transformative research in the geographical sciences. As noted in Chapter 1, there is as yet little research on indicators of potentially transformative research. The NRC report just cited includes powerful anecdotes, and the passages quoted are usefully substantive. Yet as noted earlier as Finding 4, research to date has not been able to discover characteristics capable of predicting the likelihood that an individual will produce transformative research. At this time, therefore, the committee chooses not to make a recommendation on the individual characteristics GSS might look for should it wish to encourage transformative research. Nevertheless, and despite the current lack of solid supporting research, Chapter 2 ended with the committee's consensus view on the individual characteristics likely to be conducive to transformative research, based on its analysis of the five case studies.

FUNDING PRACTICES

The suggestion was made earlier in this report that some of the pressure for increased emphasis on transformative research stems from a belief that the processes of proposal review are essentially conservative, working against projects that might involve high risk but might offer the potential for high return. Thus one way to encourage transformative research might lie in a review and perhaps revision of the proposal process. Section 1.3 noted that NSF has modified the rules it uses to evaluate submissions to its CREATIV program, one of the programs designed to foster transformative research, by raising the dollar limit on projects that can be approved without external review by more than an order of magnitude. Presumably this stems from the belief that external reviewers are more likely to be conservative than NSF's own program officers. The requirement that CREATIV proposals be approved by program officers from at least two directorates also helps to add confidence in what is essentially a stripped-down review process. Similarly, Chapter 1 noted that ESRC's program of support for transformative research in the social sciences also uses a novel review process, augmenting the traditional panel review with a "Pitch-to-Peers" session in which competing PIs review and discuss all of the shortlisted proposals in the current competition. Analogies to the venture capital industry are appealing in this regard, since somewhat similar mechanisms show up in the context of popular culture in such television programs as CNBC's Sharktank.

These innovations aside, the normal funding model has remained remarkably unchanged in recent decades. Proposals for research are generated according to the rules established by the funding agency, against deadlines that are typically annual or semiannual. Proposals are sent for external review, and then discussed in panels convened for the purpose. Panelists provide advice to program officers, who then make recommendations for funding to their agencies (in the case of NIH the tasks of peer review and award recommendation are handled by separate parts of the organization). While PIs of failing proposals may be advised to revise and resubmit, the process essentially ends with a simple yes or no decision. If the award is in the form of a cooperative agreement there may be specific reporting requirements and project reviews, but if the award is a grant there will be little accountability on the part of the PI, except for the submission of regular reports and the maintenance of regular accounting practices. Only in retrospect, after the project is completed and the results are published, is one able to determine whether the returns of the project justified the investment.

Some changes to this set of practices may be merited if the objective is to foster transformative research. In particular, this report has already noted the difficulty of judging a PI's potential for transformative research based on the conventional biographic information provided in a proposal; and the oft-expressed view that the peer-review process is essentially conservative. One alternative to the all-or-nothing nature of traditional practice could be what might be termed "progressive funding", in which PIs with promising ideas would first be awarded small seed grants through a streamlined review process. If the results were promising, a subsequent proposal could be made for a second, larger phase of funding. Keeping the initial award small would reduce the risk to the agency.

This idea is already implemented in programs such as SBIR (Small Business Innovation Research), which encourages collaboration between academia and industry and makes small Phase I awards and larger, longer-term Phase II awards. It is also common practice in major NSF programs such as the Science and Technology Centers (STCs), where pre-proposals are required and are reviewed by panels of peers. However in this case pre-proposals are used only to narrow the field—no award is made, and no research is conducted prior to the submission of a full proposal. And although the requirements for a pre-proposal are limited, many PIs would acknowledge that developing the pre-proposal involves almost as much work as developing the full proposal. Finally, NSF has programs such as RAPID and EAGER that are designed to support time-critical research, such as research in the aftermath of major natural disasters, and use an accelerated process of internal review, with the understanding that successful research funded under these programs might lead to a second phase of larger-scale and longer-term funding through other programs.

NSF is justly proud of its peer-review process, which is often lauded and widely viewed as the gold standard for funding agencies. But despite this, its review methods may no longer be as appropriate in the context described in Chapter 3, of diminished funding and steadily falling success rates, and the growing pressure to fund research that is truly transformative. Until solid research can be completed, these various ideas must be regarded as speculative, but worth investigating on a limited, trial basis.

Recommendation 4: In the interests of being more supportive of transformative research, GSS should work with other groups within and beyond NSF to explore and evaluate the novel approaches to research funding and proposal review discussed in this section.

CONCLUSION

Although there is no single, succinct, and all-encompassing definition of transformative research, two distinct themes emerged from the discussion of Chapter 1. First, transformative research has unusually high value or return that may be reflected in a variety of ways: the widespread redirection of research in an existing research community, the formation of a new research community or discipline, or the emergence of a new industry. The five case studies discussed in Chapter 2 provided ample evidence of this high value or return in the case of the geographical sciences. Second, transformative research carries unusually high risk to a funding agency, because its groundbreaking nature is difficult for PIs to visualize and for reviewers to evaluate.

The rational response to this duality is to maximize the return while minimizing the risk. In Chapter 2 Rogers' model of innovation diffusion was used to frame a discussion of the factors that may be helpful in maximizing return: open sharing of ideas, rapid dissemination, and the breaking down of institutional barriers that include the disciplinary stovepipes of academia. In Chapter 4 the committee extended these ideas in the specific context of the geographical sciences and NSF's GSS program, and also made recommendations designed to minimize risk. These include finding better ways of preparing young geographical scientists for transformative research, identifying ways in which the research culture of the geographical sciences can be made more conducive to transformative research, addressing aspects of the process of career advancement that inhibit transformative research, and exploring novel approaches to the development and review of proposals for funding transformative research.

None of these recommendations can directly address the concerns raised in Chapter 3 on the overall state of national science policy and performance. The geographical sciences are a very small part of the broader research enterprise, and though strides have been made in recent decades in achieving greater prominence for their central ideas, they will almost certainly remain close to invisible in the national debates over the four challenges elaborated in that chapter. Nevertheless, within the context of the geographical sciences the four recommendations herein do specifically address the four national research challenges articulated in Chapter 3 (Figure 4.1). The declining levels of national and state research funding (Challenges 1 and 2) can partially be offset through the development of more linkages and programs with the private sector and research partnerships with governmental agencies (Recommendation 3). This has been effective in the past growth of GIS and remote sensing as transformative sciences, as shown in Chapter 2, and will likely be just as effective and even more necessary in the future. The fostering of an open and collaborative system of innovation development and diffusion (Recommendation 3) will serve to help counteract the potentially stifling impact of competition for scarce research dollars (Challenges 1 and 2). Encouraging transformative research and targeted funding at the late inception/early diffusion stage can help to maximize governmental investment success and to offset overall reduction in funds. Targeting specifically the training of students in the nature and achievement of transformative research (Recommendations 1 and 2) will help to make sure that a larger proportion of highly educated individuals has the capacity to advance the geographical sciences and to offset both the potential declines in the absolute numbers of such highly trained individuals and the increasing competition by such students trained in other countries (Challenges 3 and 4). Increasing the diversity of the research community (Recommendation 2) serves not only to bring the wider range of perspectives that is important for the recognition of transformative research opportunities, but also serves to increase the pool of students for higher education (Challenge 3). In addition, greater diversity can promote increased engagement with the international research community and the exchange of ideas at the innovation and early diffusion stage. Finally, novel approaches to proposal review (Recommendation 4) have the potential to foster transformative research and thus ultimately to address Challenges 1, 2, and 4.

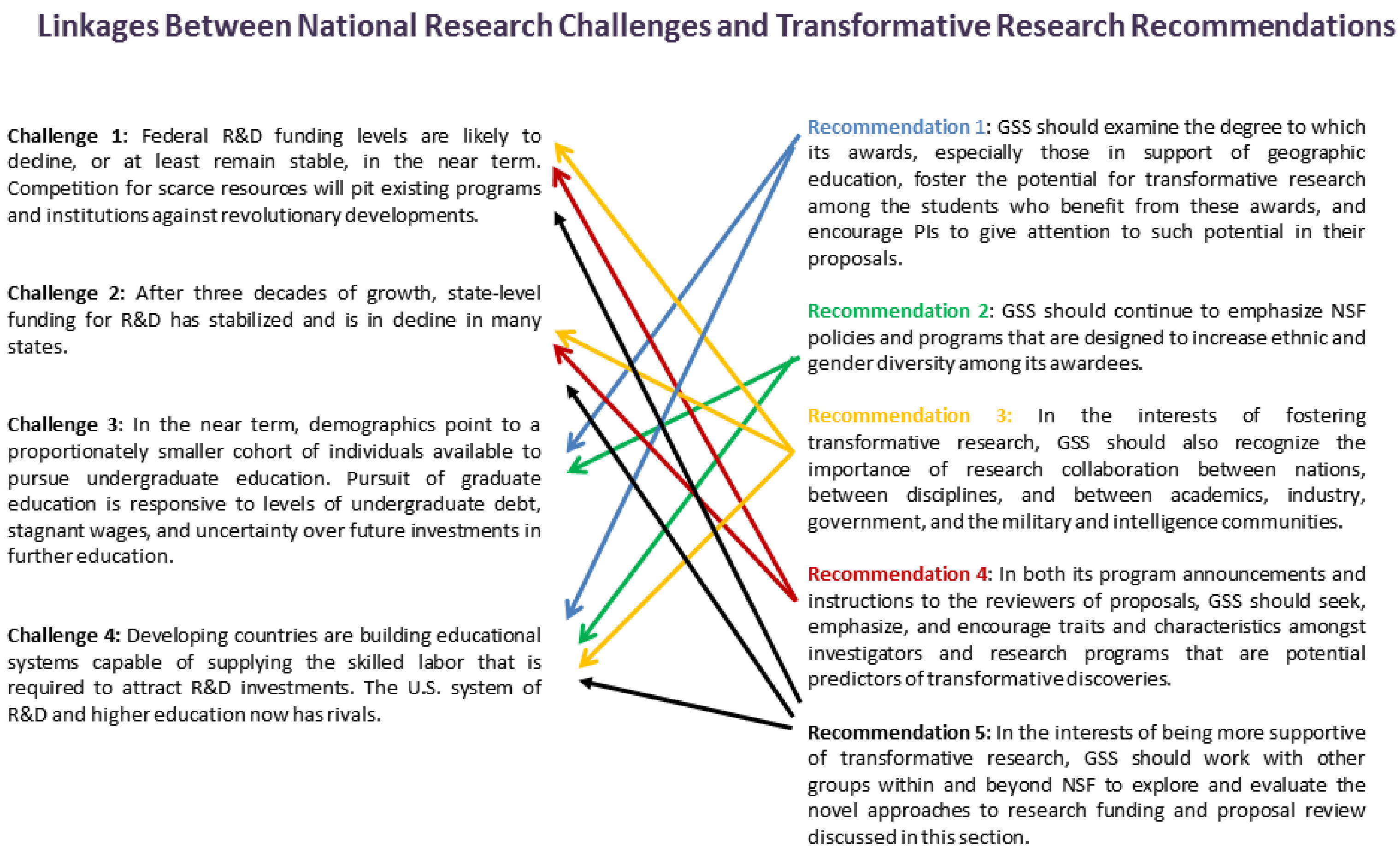

Figure 4.1: Linkages between the four challenges of Chapter 3 and the recommendations of Chapter 4.

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APPENDIX A

List of Acronyms

BLS	Bureau of Labor Statistics
CBO	Congressional Budget Office
CGIS	Canada Geographic Information System
CGS	Council of Graduate Schools
CREATIV	Creative Research Awards for Transformative Interdisciplinary Ventures program, NSF
CRO	Contract research organization
EAGER	Early-Concept Grants for Exploratory Research program, NSF
EHR	Education and Human Resources directorate, NSF
ERC	European Research Council
ERTS	Earth Resources Technology Satellite
ESRC	Economic and Social Research Council (UK)
FY	Fiscal year
GBF/DIME	Geographic Base File/Dual Independent Map Encoding
GBLT	Gay, bisexual, lesbian, transvestite
GDP	Gross domestic product
GIS	Geographic information system
GPS	Global Positioning System
GSS	Geography and Spatial Sciences program, NSF
HRHR	High risk, high reward
IBM	International Business Machines
INSPIRE	Integrated NSF Support Promoting Interdisciplinary Research and Education program, NSF
IPCC	Intergovernmental Panel on Climate Change
ISSC	International Social Science Council
MODIS	Moderate Resolution Imaging Spectroradiometer
NASA	National Aeronautics and Space Administration
NCGIA	National Center for Geographic Information and Analysis
NIH	National Institutes of Health
NOAA	National Oceanic and Atmospheric Administration
NRC	National Research Council
NSB	National Science Board
NSF	National Science Foundation
PCAST	President's Council of Advisors on Science and Technology
PI	Principal Investigator
R&D	Research and development

RAPID	Rapid Response Research program, NSF
RET	Research Experiences for Teachers program, NSF
REU	Research Experiences for Undergraduates program, NSF
SBIR	Small Business Innovation Research program
STEM	Science, technology, engineering, and mathematics
TIGER	Topologically Integrated Geographic Encoding and Referencing
UC	University of California
UK	United Kingdom
UN	United Nations
UNESCO	United Nations Educational Scientific and Cultural Organization
U.S.	United States
USGS	United States Geological Survey

APPENDIX B

List of Contributors

The following individuals provided information and advice to the committee:

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APPENDIX C

Workshop Agenda

IDENTIFYING TRANSFORMATIVE RESEARCH IN THE GEOGRAPHICAL SCIENCES

Beckman Center of the National Academies
100 Academy, Irvine, CA 92617

August 5-6, 2014

Final Workshop Agenda

Day 1 – Open Session (All Welcome)
Tuesday, August 5th
Huntington Room

8:00 *Breakfast*

8:30 Welcome and Introductions
Mike Goodchild, *chair, UC Santa Barbara*
Amy Glasmeier, *Mass. Institute of Technology*
Glen MacDonald, *UC Los Angeles*
Mark Lange, *National Research Council*

9:00 Keynote 1 — Susan Hanson, *Clark University*

9:45 Keynote 2 — Glen MacDonald, *UC Los Angeles*

10:30 Plans, Expectations, and Group Photo — Mike Goodchild, *UC Santa Barbara*

11:00 *15 Minute Break*

Panel Format: Each panelists will speak for 5 minutes followed by a moderated panel discussion and question period.

11:15 **Panel 1** – *Society, Polity, and Economy*
Robin Leichenko, *Rutgers University*
Diana Liverman, *University of Arizona*
Sallie Marston, *University of Arizona*
Laura Pulido, *University of Southern California*
Michael Watts, *UC Berkeley*
Amy Glasmeier, *moderator*

12:45 *Lunch*

1:45 **Panel 2** – *Methods, Models, and Geographic Information Systems*

Cindy Brewer, *Pennsylvania State University*
Mark Monmonier, *Syracuse University*
Shaowen Wang, *U of I, Urbana-Champaign*
May Yuan, *University of Texas at Dallas*
Mike Goodchild, *moderator*

3:15 *15 Minute Break*

3:30 **Panel 3** – *Environmental Sciences*

Ruth DeFries, *Columbia University*
Jeff Dozier, *UC Santa Barbara*
Dennis Lettenmaier, *Univ. of Washington*
Jim Randerson, *UC Irvine*
Dawn Wright, *ESRI*
Glen MacDonald, *moderator*

5:00 Discussion of Emerging Themes — Goodchild, Glasmeier, and MacDonald

5:30 *Adjourn to reception outside dining room*

6:00 Dinner in the Beckman Center dining room

Dinner Speaker — Irwin Feller, *Pennsylvania State University*

Day 2 – Open Session (All Welcome)
Wednesday, August 6th

Huntington Room

8:00 *Breakfast*

8:30 Welcome and Plans for the Day — Goodchild, Glasmeier, and MacDonald

9:00 **Panel 4** - Being Transformative

Irwin Feller, *Pennsylvania State University*
Ed Hackett, *Arizona State University*
Susan Hanson, *Clark University*
Bhavya Lal, *IDA Science and Tech. Policy Inst.*
Doug Richardson, *Assoc. of Am. Geographers*
Amy Glasmeier, *moderator*

10:30 *15 minute break*

10:45 Synthesis Session — Mike Goodchild, *chair*

12:45 *Adjourn and Lunch*

Figure C1: Workshop Participants, August 5-6, 2014, Irvine, CA
Top Row, left to right: Cindy Brewer, Douglas Richardson, Michael Goodchild, Shaowen Wang and Mark Lange
Bottom Row, left to right: Mark Monmonier, Jim Randerson, Ruth DeFries, Irwin Feller, Jesse Yow, May Yuan, Dawn Wright, Ed Hackett, Diana Liverman, Tom Baerwald, Michael Watts, Jeff Dozier, Laura Pulido, Sallie Marston, Susan Hanson, Nick Rogers, Glen MacDonald, Eric Edkin, and Amy Glasmeier
Not pictured: Bhavya Lal, Robin Leichenko, and Dennis Lettenmaier

APPENDIX D

Online Questionnaire

The questionnaire below was developed and used by the committee to gather information from key individuals and organizations.

Online Questionnaire: Transformative Research in the Geographical Sciences

What This Is: The National Research Council is conducting a study to examine how transformative research has influenced the evolution of the geographical sciences and to provide insight into how transformative research evolved in the past so that it can be encouraged in the future.

The committee is seeking ideas and examples from experts in the field of geography and related disciplines and hope you would be willing to respond briefly to the set of questions below. Your comments will inform a report to be published by National Academies Press in 2015.

Why It Is Being Done: Transformative concepts result in significant advances by re-orienting existing fields, creating new fields, or providing new theoretical or technical frameworks. For example, in geography, transformations emerged from the human-environment tradition that described the human role in changing the earth in the 1950s, followed by the 'quantitative revolution' in the 1960s which brought a wide range of statistical approaches to the discipline, and later came a series of critical approaches such as political economy and feminism in the 1970s.

Transformative methods include the development of paleo-environmental techniques to reconstruct past environments, the applications of remote sensing to track land use and cover, and the emergence of geographic information systems to integrate and analyze a wide range of spatial data in the 1980s. More recently, the advent of new genetic techniques has transformed the way biogeographers are able to query biological systems. Transformations may also be driven by the urgent needs of society including the risks of hazards and climate change, conditions of social deprivation and inequality, and the challenges of globalization.

The history of science shows many transformative concepts were difficult to identify when initially introduced or were judged to be suspect. Some concepts failed due to poor timing or lack of empirical verification; competing narrative overwhelmed others; while still others were retained due to strong and energetic communities of adherents. A framework to help identify potentially transformative research would advance the national research agenda and provide valuable guidance to researchers and funding agencies.

Questionnaire: To help the committee better understand the multifaceted issue of identifying transformative research *a priori*, please respond to as many of the five questions below as you are able.

1. What is the most important transformation in geographical sciences over the past twenty years?

2. How has transformative research in your area of interest emerged in the past and were there early indications it would be transformative?

3. Is there past research that should have been transformative (in your estimation), but in hindsight was not?

4. How can transformative research be fostered in the geographical sciences?

5. In your estimation, what is potentially going to be the most important new transformative research initiative in the next 20 years?

Comments received by August 5th, 2014 will be available at the committee's next meeting, and comments will continue to be received up to October 27th, 2014 for consideration at the committee's final meeting.

The study is sponsored by the National Science Foundation. For the committee's membership and full statement of task, click here. Please note that any written comments submitted to the committee (whether by mail, e-mail, fax, or this comment form) will be included in the study's public access file.

APPENDIX E

Committee and Staff Biographies

Committee Members

MICHAEL F. GOODCHILD (Committee Chair and NAS), is Emeritus Professor of Geography at the University of California, Santa Barbara (UCSB), where he also holds the title of Research Professor. He also holds an affiliate appointment in the Department of Geography at the University of Washington. Until his retirement in June 2012, he was Jack and Laura Dangermond Professor of Geography and Director of UCSB's Center for Spatial Studies. He received his B.A. degree from Cambridge University in physics in 1965, his Ph.D. in geography from McMaster University in 1969, and has received four honorary doctorates. He was elected a member of the National Academy of Sciences and foreign member of the Royal Society of Canada in 2002, member of the American Academy of Arts and Sciences in 2006, foreign member of the Royal Society and corresponding fellow of the British Academy in 2010; and in 2007 he received the Prix Vautrin Lud. He was editor of Geographical Analysis between 1987 and 1990 and editor of the Methods, Models, and Geographic Information Sciences section of the Annals of the Association of American Geographers from 2000 to 2006. He serves on the editorial boards of ten other journals and book series and has published over 15 books and 500 articles. He was chair of the National Research Council's Mapping Science Committee from 1997 to 1999 and of the Advisory Committee on Social, Behavioral, and Economic Sciences of the National Science Foundation from 2008 to 2010. His research interests center on geographic information science, spatial analysis, and uncertainty in geographic data.

GLEN M. MACDONALD is the John Muir Professor of Geography at the University of California at Los Angeles with a joint appointment in ecology and evolutionary biology. He is also Director of the Institute of Environmental and Sustainability. Previously, he served in positions at McMaster University and Clare Hall of Cambridge University. The focus of Dr. MacDonald's research is long-term climatic and environmental change and the impact of such changes on plants, animals, and humans. He uses a variety of archives to reconstruct past climate and environments including fossil pollen, plant macrofossils, tree rings, fossil insects, elemental geochemistry, stable isotopes, population genetics, and historical documents, artwork, and maps. Dr. MacDonald has published over 140 peer-reviewed journal articles and numerous book chapters, reports, and other pieces as well as an award-winning text on biogeography (Biogeography: Time, Space and Life). He was elected a fellow of the American Association for the Advancement of Science and has won the McMaster University Award for Teaching Excellence and the UCLA Distinguished Teaching Award. Dr. MacDonald has served as the chair of the AAAS Geology and Geography Section, the co-chair of the NSF Paleoenvironmental Arctic Sciences (PARCS) Program, chair of the AAG Biogeography Specialty Group, and international coordinator (global change) for the International Boreal Forest Research Association as well as associate editor or editorial board member for the Annals of the American Association of Geographers, Geography Compass, Journal of Biogeography, and Physical Geography. He received an A.B. degree in geography with highest honors and distinction from the University of California, Berkeley, a M.Sc. in geography from the University of Calgary, and a Ph.D. in botany from the University of Toronto.

AMY K. GLASMEIER holds a professional masters and Ph.D. in city and regional planning from the University of California, Berkeley. In spring 2009, she became the Department Head of Urban Studies and Planning at MIT. She simultaneously serves as a professor of economic geography and regional planning. She has two books on policies to develop and expand technology industries. Her book, Manufacturing Time: Global Competition in the World Watch industry, 1750-2000, provides considerable perspective on how different modes of industrial organization and varieties of capitalism yield varying levels of competitive success of national systems of industrialization. She continues to research topics related to organizational learning, regional competitiveness, and technology development. She also is an expert on income inequality and regional development. Her most recent 2005 book, An Atlas of Poverty in America: One Nation, Pulling Apart 1960-2003, examines the experience of people and places in poverty since the 1960s. She wrote a series of papers on the spatial location of wounded soldiers from Iraq and Afghanistan. Dr. Glasmeier acquired data that allowed her to map the location of health care services and the soldier's home of record. Her analytic specialties include spatial analysis, social science research methods, and policy analysis. She is a member of MIT's energy initiative (MITEI) and is co-director of MIT's undergraduate minor in energy studies. Her work on energy includes studies of sectors, regions, and technologies. She has advised local, state, and federal officials on energy policy. Her current research compares energy systems and policy U.S., China and Russia.

National Academies Staff

MARK D. LANGE (Study Director) is a program officer at the National Academy of Sciences where he also directs the Geographical Sciences Committee. Dr. Lange is a geomorphologist with research interests in river and coastal processes, GIS, and science policy. He has directed Academies studies on land change science, geospatial technologies, and national research priorities in both the Earth and geographical sciences. His work has been funded by the National Science Foundation, National Aeronautics and Space Administration, U.S. Geological Survey, U.S. Census Bureau, U.S. Forest Service and U.S. Department of State. He was previously a Tyler Environmental Fellow, a Merit Fellow, and a Congressional Fellow where he managed federal environment and natural resource policy for a member of the U.S. Congress. He is a member of the Phi Kappa Phi academic honor society, the Association of American Geographers (AAG), the American Geophysical Union (AGU), the Commission on Coastal Systems of the International Geographical Union, and was the U.S. Representative to the 32nd International Geographical Congress in Cologne, Germany. He received the Reds Wolman award from the geomorphology specialty group of the AAG. He holds a Graduate Certificate in Geographic Information Sciences and Ph.D. in Geography, both from the University of Southern California.

NICHOLAS D. ROGERS is a financial and research associate with the Board on Earth Sciences and Resources, National Research Council. He received a B.A. in history, with a focus on the history of science and early American history, from Western Connecticut State University in 2004. He began working for the National Academies in 2006 and supports the Board on Earth

Sciences and Resources on a wide range of areas from earth resources to geographical and mapping sciences.

ERIC J. EDKIN is a senior program assistant with the National Research Council's Board on Earth Sciences and Resources. He began working for the National Academies in 2009 and has supported the board on a broad array of earth resource, geographic science, and mapping science projects.