Transformative Innovation:
What 'Totally Radical' and 'Island+Bridge' Mean for NOAA Research

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Abstract of Dissertation

Transformative Innovation:
What 'Totally Radical' and 'Island+Bridge' Mean for NOAA Research

In the research presented here, I refocus the recent policy discourse on so-called 'transformative research' on a more holistic concept of 'transformative innovation'. The products are three independent but closely interrelated papers. First, drawing upon an extensive review of the literature, I develop the argument that Federal R&D intended to transform should be 'totally radical,' encompassing the totality of the innovative process required for actually radical outcomes, rather than potentially radical activities. Second, I look at how R&D efforts are organized to produce innovation at three federal programs with a track record of transformation (DARPA, ARPA-E, and ATP), and I develop an island+bridge model for describing and comparing the transformative nature of federal R&D efforts. Third, I use this model to investigate the central research division at the National Oceanic and Atmospheric Administration (NOAA) to understand how it is managed relative to transformative innovation, and what improvements could be made to this end.

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1

Introduction

Change. Big and small, continuous and discontinuous, predictable and unpredictable, for better and for worse, we make change and it makes us. The organizations that specialize in research and development (R&D) exist to make and respond to change – to make the world a different, hopefully better place. But how do we choose which changes to make or respond to? How do we choose which R&D efforts will get us closer to the outcomes we desire? Bozeman & Sarewitz noted that, 'from the beginning, the central problem in science policy was recognized by workers in the field as the problem of choice' (2011, p. 2). Similarly, Koizumi observed, 'the challenge for the science of science policy (SoSP) community is to provide policymakers with more tools, and the right ones, to affect the government's numerous resource allocation decisions' (2011, p. 289). The same problem was identified much earlier by V. O. Key with respect to the public budgeting process more generally, that is, 'on what basis shall it be decided to allocate x dollars to activity A instead of activity B?' (Key 1940, p. 1137).

In the research presented here, I wanted to address the discourse on so-called 'transformative research', a policy focus for the last decade to renew government's role in innovation. The use of

this new term implies that we have a new choice for research portfolios: transformative or not. Prima facie, it would seem to mean aiming for big change, rather than small. Of course, we would all like to predict which R&D investments will yield larger returns, but this is not realistic. I wanted to explore what the term 'transformative' means and what it types of choices it might actually allow. Given that we cannot know the future, that we do not know beforehand which R&D efforts will result in transformation, are there ways we might be able to say that activity A is more likely to transform than activity B?

These choices matter. Society faces the increasingly 'wicked problems' (Rittel & Webber 1973) of growth in a competitive global economy and sustainability in a finite global ecosystem. Global trends like population growth and climate change are making finite supplies of food, water, and energy increasingly uncertain. Then there are the changes that we cannot anticipate: the high-amplitude, low-frequency 'black swan' events that we know are increasingly likely to occur, but that we cannot predict (Taleb 2007). I believe innovation can solve the problems we face, but the quality of our choices about innovation rests upon the quality of our understanding about how it occurs, and what we can do to influence it. I come to this research recognizing that our collective understanding of innovation as a phenomenon has developed significantly over the years since many of the government agencies that specialize in R&D were first established. Our improved perspective should influence the nature of the public institutions we task to innovate.

From Vannevar Bush's (1945) voyage to *The Endless Frontier*, we have a chart of the innovation process that seems accurate, but, like other old maps, if we look a little closer, we find that it oversimplifies. Basic research does, in fact, benefit society in unpredictable ways, and often requires funding with no particular expectations, no direction, and no strings attached. Yet this approach segregates the activities and the actors of science from technology, and science and technology from economy and policy. It implicitly undervalues the connections that must exist

between these communities in order for knowledge to flow and, therefore, for innovation to occur. Bush's linear model of scientific and technological value creation was useful in the modern industrial era, but clinging to it in our time limits our ability to understand and manage innovation. As a model of economic value creation, the linear model is an anachronism.

For decades, the science policy and management literatures have treated research as but one part of larger systems of innovation; I extend that logic to the realm of transformation. Value is realized when scientific knowledge is taken out of isolation and put in the context of other specializations. Innovation occurs across complex, fragmented networks of expertise rather than in single, hierarchical organizations (Funtowicz & Ravetz 1993, Hill 2007). Specialized knowledge can be both a blessing and a curse (Rammert 2006). If we are to take those who aim to transform at their word, if policy is to create breakthroughs for economic growth, and if science is to be 'socially robust' rather than 'merely reliable' (Gibbons & Nowotny 2000), then we must move the discussion beyond transformative *research*, and think more broadly about transformative *innovation*. I understand innovation from the post-Bush perspective of policy and management, that is to say, it is the creation of value from the use of new knowledge or technology, not simply the creation of new knowledge or technology itself.

This dissertation is composed of three independent, but closely interrelated papers. Paper 1 establishes a theoretical perspective that transformative innovation must be 'totally radical', that is, they must have clear objectives for broad benefit, and must be organized to achieve them. Paper 2 details an 'island+bridge' model of innovation, derived from the practices of three federal agencies: the Defense Advanced Research Projects Agency (DARPA); the Advanced Technology Program (ATP), once housed within the National Institute of Standards and Technology (NIST); and the Energy Advanced Research Projects Agency (ARPA-E). Paper 3 applies this model to assess the innovative potential of a fourth agency: The National Oceanic and Atmospheric

Administration (NOAA). The results of Papers 2 and 3 are grounded in data derived from interviews of 46 managers across the four agencies. (Detailed results of data analysis and model synthesis are provided as tables and figures in each paper individually, as well in the Appendix.)

In Paper 1, I review the literature on transformative research and place it in the context of the literature on innovation as an evolutionary system. What I find is that those advocating for transformative research largely use the term from the perspective of the linear model of innovation. The desire for 'ground-breaking' or 'paradigm-shifting' research stems from a continued focus on what is traditionally referred to as fundamental or basic research. Research such as this occupies an important place in society, but it is not a new concept, and does not directly address economic growth – the oft-stated justification for such research. While it may yield the potential for transformation, it does not necessarily yield actual transformation as did, for example, the Apollo Program, the Manhattan Project, or the development of ARPANET. Arguments for greater federal investment in transformative research, are, ironically, arguments for more of the same research advocated by Bush.

I develop the argument that Federal R&D intended to transform should be 'totally radical', encompassing the totality of the innovative process required for actually radical outcomes, rather than potentially radical activities. I define transformative innovation as *work organized to realize concrete visions of change beyond the research community*. This definition has three components. First, the revolutions should be outside 'the Balkans' of niche scientific fields (Kuhn 1969/1996, p. 92). An R&D program embodies transformative innovation if it is intended to be radical in its benefits to society and economy, not merely in its challenge to prevailing scientific paradigms. Second, the objectives must be more that 'milquetoast.' An R&D program embodies transformative innovation if it is driven by a clear and tangible objective, not discovery of 'unknown unknowns'. Third, the focus should be on recipes, rather than ingredients. An R&D

program embodies transformative innovation if it has organizational potential as well as individual potential, and can combine all the capabilities necessary to achieve the objective.

In Paper 2, I build upon a key conclusion of Paper 1, namely, that innovation implies organization, and perhaps the only way to say that R&D might be transformative, prospectively, is by looking at how it is organized. As such, I look at how R&D efforts are organized to produce innovation at three federal programs with a track record of transformation (DARPA, ARPA-E, and ATP), and I develop a model for describing and comparing the transformative nature of federal R&D efforts. The driving question is: *what are the organizational factors that foster transformative innovation in the federal R&D programs that succeed in creating it?* Here, I build on the work of Bonvillian and Van Atta (2011); their insights provided the basis for questions that structured interviews with managers at the three model agencies listed above, and for the codes I used to analyze those interviews qualitatively.

The result is a model based on Bennis & Biederman's (1997) metaphor that every innovative group is 'an island with a bridge'. The island+bridge model offers a general account of factors concerning organizational structure of the three model agencies at the institutional level, as well as the management of programs and projects at a personal level. The model is not intended to be a revelation of the 'secret sauce' of the ARPA-model. It should, however, prove useful as a framework within which to conduct diagnostic assessment of other federal R&D programs to see how close they are to the model agencies in a number of different ways, and therefore what kinds of options they might have were they to try to emulate the model agencies' approach. (In fact, this is the sort of analysis that is recounted in Paper 3.)

The model itself is assembled as a typology of organizational heuristics (i.e. cognitive shortcuts) derived from source material and nested under either the concept of the island or bridge.

ISLAND is a metaphor for those aspects of R&D programs that isolate them from operational concerns as well as other, more traditional research concerns. It implies differentness and specialness in a range of factors spanning science, engineering, personal attitudes, and program administration. Heuristics nested under ISLAND include: STAY SMALL, FEEL FREE, HERD NERDS, and BE BETTER. The hallmark of the island is the ability of the manager, and the program as a whole, to be deliberately but delicately disruptive: transforming systems, while still working within a system of controls. BRIDGE is a metaphor for those aspects of programs that connect them to broader institutional contexts. R&D is integrated with operational concerns by creating multiple opportunities for precise communication among interested parties. Heuristics nested under BRIDGE include: MAKE IT MATTER, MEET AND GREET, KEEP CLEAR, and LISTEN AND LEARN. The hallmark of the BRIDGE is frank and frequent face time: regular, detailed conversations focusing on direction and performance and, specifically, the iterative negotiation of project visions and metrics.

In Paper 3, I use this island+bridge model to frame an investigation of the central research division at NOAA, the Office of Oceanic and Atmospheric Research (OAR) to understand how it is managed relative to transformative innovation, and what improvements could be made to this end. The driving question here is: *to what extent is NOAA's central research division managed like federal R&D agencies that routinely create transformative innovation?* At NOAA, interviews were conducted with the lead manager, director, or deputy of each one of OAR's eleven laboratory divisions, seven program office divisions, and eleven of thirteen cooperative institutes. Interview questions were exactly the same as those given to the model agencies. Responses were analyzed using the same codes to mark the presence of the same factors. New codes were created for emergent factors.

The results of this assessment were not surprising. OAR, as a whole, is not organized like DARPA and its analogs. OAR's program offices, however, were somewhat closer to the island+bridge model than its laboratories or cooperative institutes. Analysis of the OAR responses yielded a host of emergent codes for factors that did not turn up at the model agencies. Most of these had to be clustered under a new nest of heuristics under the metaphor of LAND, which was only implied by the original island+bridge model. LAND is a place where people strive for stability, rather than change. It represents the more typical constraints of the bureaucracy that the model agencies were designed to get away from, including well-established interests of federal laboratories and government-sponsored science more broadly. The two main heuristic on LAND are: FARM FACTS and MAKE SAUSAGE. The former implies being a steward of science as the 'seed corn' for future applications. The latter points to the non-scientific work of politics, policy, and administration and the organizational values of growth, consensus, and temperance.

At OAR, heuristics on the LAND are generally more active than those on the ISLAND or on the BRIDGE. As such, we should not expect transformative innovation from NOAA Research. However, OAR does demonstrate the capacity for taking ideas and inventions over the finish line, that is, for innovation. This is strongly evident from the drive to TARGET OPERATIONS, but also present to a modest degree in all of the other heuristics on the ISLAND and BRIDGE. Assuming that NOAA wanted to engender more transformation though its central research division, I identify five key distinctions NOAA might consider to move closer to the island+bridge model: 1) program offices, versus laboratories and cooperative institutes; 2) programs in a special unit, versus programs in a science division; 3) negotiated cooperative agreements, versus open-ended grants; 4) excited temporary managers, versus expert permanent scientists; 5) tackling risks though specificity, versus assuming risk though ambiguity. Taken

together or individually, these are choices that NOAA can make to build upon the strengths that it already has to increase its chances of transformative innovation.

Papers 2 and 3 are based largely upon qualitative analysis and synthesis. As such, it is incumbent upon me, the researcher, to disclose what I perceive to be my professional and epistemological biases. I have spent both my undergraduate and graduate years studying the historical, philosophical, economic, and political interdependencies with science and technology. My academic specialization has been in public science policy and, more particularly, federal policy for space and environmental science. I have spent the last 10 years working at the corporate offices of the NOAA providing analytical support for the Chief Economist, the Director of Strategic Planning, and the Assistant Administrator for Program Planning and Integration. Currently, I serve in OAR's office of Policy, Planning and Evaluation, where I develop strategies, business processes, and evaluation schema for R&D programs.

My epistemology can be summed up in two words: language matters. Often, the challenges faced by organizations as they strive to innovate are differences in meanings and vocabularies among a multitude of diverse specialists. I approach the topic of transformative innovation from the paradigmatic perspectives of interpretive and critical theories. Knowledge is a social construction of the inter-subjective experiences within a given community, as mediated through language (Berger and Luckmann 1967). If we are not careful, communicative distortions and asymmetries across communities can lead to power differentials and political alienation (Habermas 1971). Within a public administration context, disagreement over outcomes and outputs can create principal-agent problems where individuals subvert authority in favor of their own ends (Wilson 1989). I understand organizations, including federal research bureaucracies, as systems of humans in power relations that are structured by professional discipline. I see the problem of transformation as one of integrating different types of understanding and, concomitantly, shifting

power from those who benefit from the status quo to those who benefit from change (Sapolsky & Taylor 2011).

Lastly, my academic and professional careers have both been deeply multidisciplinary and my interests tend toward the semantic and the symbolic. I have found that in order to make sense of complex concepts to myself, to colleagues, and to time-strapped superiors, it was necessary to become more fluent in visual language. Thus, the keystone of this research has been a large, evolving concept map of organizational heuristics, which, itself was too large to include in this document, but sections of which are included as figures throughout. Herbert Simon wrote, 'it is far easier to calculate the rational response to a fully specified situation than it is to arrive at a reasonable specification of the situation' (Simon 1985). My intent with the research presented here is to provide at a reasonable specification of the topic of transformative innovation for others to examine, critique, and build upon in order to make rational policy decisions about innovation.

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2

Totally Radical:

From Transformative Research

to Transformative Innovation

Summary

The National Science Board and the US Congress want more Federal research to be *transformative* research. Until now, discussion of what this really means has mostly focused on research, per se, and has not addressed innovation as a complex system. 'Transformation' describes changes in society and economy, not merely changes in science and technology. It entails connections among individuals, institutions, and between research, development, and nascent applications. Serious efforts to transform are *totally radical* — they span the totality of the innovative process to produce actually radical outcomes, rather than maintain potentially radical activities. Transformative innovation is specifically organized to realize concrete visions of change beyond the research community. Being totally radical about innovation means creating the social conditions within which new technologies can thrive.

Transformative Research

'Transformative research' is the term du jour in US science policy. The National Science Board (NSB) — the policy setting body for the National Science Foundation (NSF) — found that NSF was not encouraging enough research that was revolutionary, that 'transforms science by overthrowing entrenched paradigms and generating new ones' (National Science Board 2007, p. 8). This notion gained broad traction in the 2007 National Academy of Science (NAS) report, *Rising above the Gathering Storm*. This report claimed that, given economic globalization, the US was not conducting enough transformative research, and that the federal research enterprise, as a whole, does not systematically plan for transformative research (National Academy of Sciences 2007). Congress has set policy for science agencies to create strategies for transformative research in the 2007 America COMPETES Act (US Congress 2007), as well as its 2010 reauthorization (US Congress 2011).

Is 'transformative' a meaningful, measurable distinction? Or is it just a euphemism for *cool* or *sexy* in the professional language of policy discourse? Either way, we ought to take the semantics of this term seriously. The concept of transformative research deserves closer examination because it has become the basis for real choices. And as Bozeman & Sarewitz have observed, 'from the beginning, the central problem in science policy was recognized by workers in the field as the problem of choice' (2011, p. 2). Basic or applied? Research or development? And now: transformative or not? This new distinction might enable more effective science policy choices. Or it may be used as a political device to justify research that has no real merit.

Research endeavors are inherently oriented toward change — in knowledge, technology, institutions, etc. — but what does it mean to choose transformation? Such choices must be

something more than the usual dilemma of research versus operations, chance versus certainty. We can agree that no attempt at making change is certain to work as planned, or even work at all, but such attempts are necessary for economic growth and social progress. The real dilemma is, once we have decided to invest in innovation, where do we focus our effort more specifically? Federal agencies can create the potential for transformation through new understanding, as their research divisions typically do, or they can create actual transformation through new or renewed organizations, as was done in prior generations with the Apollo Program, the Manhattan Project, and The Defense Advance Research Projects Agency (DARPA). These are the clearest examples of Federal research transforming the world. They are central to America's national mythology.

Management and science policy literature is replete with distinctions between research and development and the larger systems of innovation of which they are a part. We can extend that distinction to innovation toward transformative outcomes. If policy is intended to create breakthroughs for growth, we must move beyond mere transformative *research*, and think holistically about transformative *innovation*. This means understanding transformation as a result of the connections among institutions, individuals, and between research, development, and nascent applications. Federal R&D that is intended to create transformation cannot just be *cool*. Rather, it must be *totally radical,* that is, it spans the totality of the innovative process, and aims to produce actual radical outcomes, not simply maintain potentially radical activities. The demarcation 'transformative' most accurately describes those investments that are organized to make specific changes in the economy, environment, and social welfare.

Context and Concepts

In the sections that follow, I discuss the economic context for the recent interest in transformative research, review several attempts to define it, and place the concept of transformation within the

discourse on innovation theory, particularly the distinction between the modern linear model of value creation and the post-modern systems (or 'connected') model. Then, using the National Science Board's criteria for transformative research for NSF as a touchstone *('driven by ideas that have the potential to radically change'),* I offer criteria for transformative innovation to mission agencies that are serious about effecting transformative change through R&D *(organized to realize concrete visions of change beyond the research community).* I conclude by addressing two implications: the importance of strategy, and not serendipity, for planning transformations; and the importance of embracing politics as a means of achieving transformations.

Social and Economic Motives

Political interest in transformative research is relatively recent. It stems from the concern that America has dwindling capacity in the type of research that generates dramatic results — particularly compared to other nations. At stake is the nation's competitive advantage in the global economy as an innovator and a 'first-mover'. Federal R&D policy administration is often to blame; portfolio management at science agencies has been criticized as too risk averse and for not yielding the high payoff results of prior generations (National Academy of Sciences 2007).

Arguments for transformative research are made on the basis of social welfare, and comport with well-established economic theory that knowledge is itself a factor of production. Abramovitz (1956) explained that innovation, rather than traditional factors of production (capital, labor, natural resources), was the modern driver of economic growth. Solow (1957) demonstrated that '87.5 per cent of the increase [in labor productivity was] attributable to technical change and the remaining 12.5 per cent to increased use of capital'. The bulk of growth may be due to 'better recipes, not just more ingredients' (Romer 1990). Technological change is part of economic systems, and by investing in knowledge we can increase economic productivity and net social welfare (Nelson 1959).

The call for government to sponsor transformative research also rests upon economic theory that there is a market failure for 'better recipes'. The benefits of investments into innovation are not always entirely appropriable by the investor, i.e. there are positive externalities, or so-called 'spillovers' (Nelson 1959). There may be a disincentive for firms to invest in the research and development necessary to spawn innovation; as such investments are inherently risky (Arrow 1962). Scientific and technical knowledge display the qualities of a public good. Firms will engage in the production of scientific and technical knowledge only to the degree that they can prevent other firms from appropriating it, which imposes additional costs. Thus, there is a tendency within the private sector to innovate below the Pareto optimal level for society, and it may be the responsibility of government to fill this void.

Calls for transformative research are made within a historical context of policies to improve, or maintain US competitiveness. In the 1980s, the federal government actively addressed increasing competition from Japan with fiscal policy, education policy, and partnerships with the private sector, ultimately paving the way for the recovery of the 1990s (Hughes 2005). Similarly, Japan's concerns over its own relative competitiveness, and image as a technological 'follower', led to government intervention aimed at the computing and microelectronics industries (Fong 1998). In the United States' current situation, long-term competitiveness seems to be endangered by growing science and engineering capacity in China and India. Tassey (2010) has argued that the US has underinvested in R&D for the last few decades, compared to other industrial nations. China is now in second place to the US in the numbers of scientific papers published as well as those cited, and is rapidly approaching the US (Leydesdorff & Wagner 2009).

If social and economic concerns are the motivators, however, then policy must include more than research in its treatment of innovation. Romer notes that, over the 20th century, 'the policies and

institutions in the United States made possible a trend rate of growth of income per person that was significantly faster than the trend that had pushed Britain into the position of worldwide leadership in the nineteenth century' and that 'if we arranged our institutions optimally, growth in the United States could take place at an even higher rate to which we have become accustomed' (Romer 2000 p. 9). Innovation is best facilitated by policies that treat it as a system of institutions interacting across professions and across sectors.

If transformation is the goal, the research enterprise must be addressed holistically, from the 'front-end' of innovation with research to the 'back end', with adoption and diffusion (Weiss & Bonvillian 2009). In moving from transformative research to transformative innovation, we must first take a closer look at what these terms mean, how they differ, and what it means to organize innovation efforts that are totally, rather than partially radical.

Phantoms and Fuzz

From the perspective of policy and administration choices, there are no clear criteria with which to select transformative research. At a 2012 National Science Foundation (NSF) workshop in Arlington, VA on the meaning and implications of transformative research, the overriding opinion was that it was not at all well defined. Per the final report, 'participants expressed some concern that the term identified a phantom phenomenon; that it can only be defined retrospectively; or that it was more a term of advertising than of substance' (Frodeman & Holbrook 2012 p. 2). Many believed that the meaning should be left open for multiple interpretations. Hughes et al. summarize the problem of identifying transformative, high-risk/high-reward research in the words of photographer Ansel Adams: 'There's nothing worse than a sharp image of a fuzzy concept' (Hughes et al. 2011, p. 3).

The reference point in the discourse on transformative research is the definition that the National Science Board recommended to guide grant-making decisions at the NSF:

> Transformative research is defined as research driven by ideas that have the potential to radically change our understanding of an important existing scientific or engineering concept or leading to the creation of a new paradigm or field of science or engineering. Such research also is characterized by its challenge to current understanding or its pathway to new frontiers (National Science Board 2007 p. 10).

Remarking on this definition and a survey of other commentary by NSB and NSF, Dietz and Rogers found that transformative research, as policy terminology, 'is broadly applied and differentially applied. It is a term in pursuit of a concept' (Dietz & Rogers 2012 p. 22) and that:

> It can be defined or identified by one or more (probably more) of the following fifteen characteristics:
>
> 1. driven by ideas, discoveries or tools,
> 2. disruptive to existing theories or perspectives,
> 3. results in radical changes in our understanding of existing concepts,
> 4. leads to the creation of a new paradigm or field,
> 5. can redefine the boundaries of science, engineering, or education,
> 6. can be approached from different pathways,
> 7. can lead to new techniques or methodologies,
> 8. does not fit comfortably into existing funding mechanisms,
> 9. is not necessarily step-by-step,
> 10. doesn't fare well when reviewed by parties invested in the status quo,
> 11. initially difficult to interpret,
> 12. difficult to recognize at the time of creation,
> 13. not necessarily risky,
> 14. not necessarily interdisciplinary, and
> 15. you know it when you see it (Dietz & Rogers 2012 p. 24)

Absent a specific, usable definition, Dietz and Rogers offer four metaphors to characterize discussions of transformative research. The market metaphor takes the perspective of the investor, who tries to maximize reward over risk by improving processes of selection. The evolution metaphor takes the perspective of the steward who tries to guarantee the diversity of the science and technology ecosystem by increasing variation. The fashion metaphor reflects the perspective of the curator, who capitalizes on research by keeping abreast of trends. The frontier

metaphor evokes the view of the explorer, who creates new possibilities by expanding the scope of the known (Dietz & Rogers 2012).

Beyond metaphors, however, Lal assumes the stance that transformative research is a social construct intended to inspire and motivate, and that what really matters 'is how it can be identified prospectively, managed during its conduct, and evaluated retrospectively' (Lal 2012). Making decisions in the design, administration, and assessment of R&D policy requires that we explicitly acknowledge our mental models of how innovation happens. Policies based on different assumptions of how innovation occurs may satisfy different outcomes and yield different impacts; they might be more or less transformative, or transformative in different ways, or to different people.

Stuck in the Pipeline

Weiss and Bonvillian summarize three basic models of innovation. The *linear* model (also known as the *'pipeline'* model) as espoused by Vannevar Bush and as institutionalized by NSF, reflects a supply-side orientation and emphasizes fundamental scientific knowledge — research pushes application. The *induced* model also assumes a linear progression, but in the other direction: demand from the external economic and policy environment determine the course of innovation — applications pull research. The *connected* model takes a non-linear, systems approach: what determine the course of innovation are the organizations and the institutions that link the forces of supply to those of demand (Bonvillian 2006).

It is clear that advocacy for transformative research is not couched in the model of induced change; the motivations in American competitiveness assume that there is a government role in fixing the market failure for the production of radical science and technology. The induced model is also inconsistent with the popular mythos of American innovation. As Steve Jobs

famously said, ‘A lot of times, people don’t know what they want until you show it to them’ (Reinhardt 1998). Henry Ford remarked that if he had asked the Model T’s customers what they wanted, they would have said, ‘a faster horse’. Radical change is rarely, if ever, demand-driven. In Dosi’s view, demand-driven pull for new technology rarely results in radical innovations that create new trajectories (Dosi 1982). Similarly, Christensen notes that innovations that are truly disruptive never occur by trying to satisfy existing markets, but by creating new ones (Christensen 2003).

The language of transformative research might also appear to represent a break from the pipeline model in favor of the connected model. Proponents of transformative research do not recommend ‘fundamental research’ or ‘basic research’. Instead, transformation occurs via ‘paradigm change’, a specific reference to Kuhn’s idea that scientific knowledge evolves within social systems. As described in *The Structure of Scientific Revolutions*, Kuhn suggests that most research occurs in periods of normal science, when scientists reinforce the current paradigm, and engage in ‘puzzle-solving’ to tie up the loose ends. Normal science is occasionally punctuated by periods of revolution, when the community shifts from one paradigm to another, slowly at first and then en masse (Kuhn 1969/1996).

Dosi extended Kuhn’s evolutionary perspective from science to technology. He described ‘technological trajectories’ as the vectors along which innovation occurs within a paradigm, as shaped by social and institutional selection pressures (Dosi 1982), similar to phenotypic vectors selected for in biological evolution. Others have specifically addressed how technological evolution is a specific case of Darwinian laws of change (Mokyr 2003; Ziman 2003). Schumpeter was perhaps the first to offer this analogy in his discussions of innovation as creative destruction:

> The opening up of new markets, foreign or domestic, and the organizational development from the craft shop to such concerns as US Steel illustrate the same process of industrial mutation — if I may use that biological term — that incessantly revolutionizes the economic structure from within, incessantly destroying the old one, incessantly creating a new one. This process of Creative Destruction is the essential fact about capitalism (Schumpeter 1943/1994, p. 83).

Starting with Schumpeter, many economists have come to think in terms of techno-economic paradigms that are periodically disrupted by technology-driven waves of economic growth and decline. Such waves, first suggested by Nikolai Kondratieff, occur on about a fifty year cycle, and have included the industrial transformation from craftwork to factory work in the late nineteenth century, to mass production and marketing in the middle of the twentieth century, and the change to an information and knowledge economy in the late twentieth century. In each wave, 'technology does not advance incrementally, but rather bursts onto the scene irregularly with clusters of breakthrough technologies, and resultant transformations from one kind of society to another' (Atkinson 2004, p. 18).

For several decades now, underlying much of the science policy literature has been the assumption that scientific and technological advance is part of an evolving socio-economic system of innovation, from the search and selection of 'routines' (Nelson & Winter 1982) and technological trajectories (Dosi 1982), to path dependence and 'lock-in' (Ruttan 2006; Rycroft & Kash 1999) and, more recently, national (and other) systems of innovation (Lundvall 2010; Nelson 1993), the Triple-Helix relationship between government, public, and academic sectors (Etzkowitz & Leydesdorff 2000), and Mode-2 knowledge production (Nowotny, Scott, & Gibbons 2001). Nelson summarizes well the commonality in this work: 'in virtually all fields one must understand technical advance as proceeding through the work of a community of actors', including but not limited to those performing R&D (Nelson 1993, p. 14).

However, while the discourse of transformative research employs the results of a systems perspective—that is, a focus on the spectrum of change from radical to incremental—it does not yet represent an adoption of the connected model. The NSB definition and recent interpretations prescribe radical change as challenging to *scientific* understanding, which is assumed to spur new technology, and then the next wave of economic growth. How one step leads to another is not addressed, calling to mind the infamous cartoon of a mathematical proof in which a crucial step is labeled, 'and then a miracle occurs'. The NSB definition emphasizes radical shifts in understanding, not in the connections among actors, nor the connecting institutions. Unaddressed by most discussions of transformative research is the proverbial 'valley of death' between research and applications, indicating a continued reliance on the linear model.

Another signal of the pipeline perspective is the frequent use of the word 'potentially' to qualify 'transformative research', as in NSF's merit review criteria for 'potentially transformative concepts' (The National Science Foundation 2010a, p. 67) and the stated purpose of the March 2012 workshop: to understand 'potentially transformative research' (University of North Texas, Center for the Study of Interdisciplinarity, 2012). The word is employed to guard against false assumptions of certainty, but this qualification distances the research from the transformation. It emphasizes that, while discovery and invention are likely to happen, application is not. In so doing, it reinforces the fallacy of the linear model that 'more knowledge is always better' (Sarewitz 1996). Its continued use is a normative cue to continue focusing efforts on the 'front end' of innovation rather than the 'back end'.

The four metaphors Deitz and Rogers offer, while helpful in that they are more tangible, are also limited to the potentially transformative. There is no metaphor that captures the perspective and transformative feats of the best transformers we have known: the Wright Brothers, Thomas Edison, J. Robert Oppenheimer, Werner Von Braun, Steve Jobs, or even, ironically, Vannevar

Bush himself as an organizer and manager of R&D for the war effort (Bonvillian 2009). Their contributions were in harnessing potential to create new realities by managing connections. The archetypes of the investor, the steward, the curator, and the explorer must be joined by *the entrepreneur*, one who has not been inculcated by conventional wisdom (Baumol 2004) and who can align multiple forces and bring them to bear on a single strategic focus. Hwang and Horowitt (2012) liken such people to keystone species within an ecosystem: they keep things connected, actively bridging social distances and reducing the transaction costs within a system of innovation.

Innovation as a System

Instead of dodging the valley of death and ignoring our most historic transformers, federal policy makers and program administrators should move beyond transformative research to a systems perspective of transformative innovation. The word 'innovation' can mean different things to different people, so it is helpful at this point to review what it means to those who have studied it closely. Most modern definitions reflect the connected model:

> A novel creation that produces value (Nagji & Tuff 2012, p. 4).
>
> The intersection of invention and insight, leading to the creation of social and economic value (US Council on Competitiveness 2005, p. 8).
>
> The process by which individuals and organizations generate new ideas and put them into practice (National Economic Council, Council of Economic Advisers & Office of Science and Technology Policy 2011, p. 7).
>
> The processes by which firms master and get into practice product designs and manufacturing processes that are new to them, if not to the universe or even to the nation (Nelson 1993, p. 4).
>
> The implementation of a new or significantly improved product (good or service), or process, a new marketing method, or a new organisational method in business practices, workplace organisation or external relations (OECD 2005, p. 46).

Innovation is not particular to any form of technology. The novel element can be anything — a product, a process, or a way of organizing — but it must be used and must create value. Innovation is, thus, more than ideation and invention, though these are essential ingredients. Rather, it is about the **implementation** of ideas and inventions. Invention is the application of knowledge to create something practical, whereas innovation is the system in which inventions are disseminated for societal use (Bonvillian, lecture notes 2012). Freeman and Soete differentiate between invention and innovation in terms of market transactions:

> An invention is an idea, a sketch or a model for a new or improved device, product, process or system. Such inventions may often (not always) be patented but they do not necessarily lead to technical innovations. In fact the majority do not. An innovation in the economic sense is accomplished only with the first commercial transaction involving the new product, process, system or device, although the word is used to describe the whole process (Freeman & Soete 1997, p. 6).

But whether or not there is a commercial aspect to it, innovation involves both a producer and a consumer, not either one alone; together, both are innovators. Innovation is, therefore, something bigger than research and development. It includes the design of products, diffusion into regular practice, and effective managerial processes.

Nelson has pointed out that R&D only consumes a small portion of the resources required for innovation (Nelson 1993). Annual studies by Booz & Company of the most innovative companies in the world consistently find little evidence for the simple, causal relationship between money spent on R&D and innovation (Jaruzelski & Holman 2011). Ruttan has described how the historical industrial advances of machine tools and aircraft occurred independently of the fundamental science of the time, for example: 'The knowledge employed by the Wright brothers and other early aircraft designers drew almost entirely on craft and engineering knowledge and practice' (Ruttan 2006, p. 35). Moreover, in case studies of a number

of industries, Rycroft and Kash (1999) found it impossible to determine exactly what role R&D actually plays in complex technologies.

Indeed, the direction of influence is often the reverse of what the linear model tells us. Not only will new scientific knowledge spur advances in technology, but new technology frequently stimulates advances in science (Nelson 1993; Stokes 1997). Innovation is a two-way street. The advent of the scanning electron microscope, for example, led cellular biologists to see organelles more clearly, reinterpreting as ribosomes what they had previously understood as microsomes (Rheinberger 1995).

In the context of innovation, transformation is related to and, in fact, dependent on other R&D concepts that begin with the prefix 'trans-'. At mission agencies such as NASA and NOAA, the term 'transition' is frequently used to describe the process of establishing a research technology in an operational setting (Goodman et al. 2004; Serafin, Macdonald, & Gall 2002). In the field of medicine, 'translational' research is aimed specifically at increasing disciplinary mixing and maintaining a problem focus in medical research rather than a solution focus. The National Institutes of Health has established the National Center for Advancing Translational Sciences (NCATS) to remove the barriers between medical discoveries and treatments for patients (National Institutes of Health 2011). Also, introducing a new journal for translational medicine, Science Editor-in-Chief Bruce Alberts wrote:

> Too often, information with the potential to improve human quality of life is available only through silo-like channels. For example, cardiologists who only attend specialized meetings and read the cardiology literature, but not the physics or computer science literature, can miss an important breakthrough that could advance their own research. To stimulate innovation, we must intentionally catalyze the mixing of scientists and clinicians from different disciplines, knowing that new approaches will emerge from their interactions (Alberts 2009, p. 206).

Inasmuch as transformative innovation can be characterized by the connected model, and yields benefits for stakeholders beyond the immediate research community, it is by necessity what Gibbons and Nowotny call 'transdisciplinary', i.e. it 'contributes to a joint problem solving that is more than juxtaposition; more than laying one discipline alongside another' (Gibbons & Nowotny 2000, p. 68). In a similar vein, Hill describes our post-scientific society as one in which, 'cutting-edge success depends not on specialization, but on integration — on synthesis, design, creativity, and imagination' (Hill 2007, p. 82). Economic and political success is determined less by analytic work within disciplinary or professional specializations (even as these capabilities grow across the globe), and more by the synthetic work that transcends and integrates the work of disciplinary and professional specializations.

Transformative Innovation

To summarize, then, what it means to be transformative, I offer this distinction made by Bozemann and Sarewitz in their description of R&D driven by public values: 'the capacity to produce innovation, rather than just the innovation products themselves' (Bozeman & Sarewitz 2011, p. 11). But perhaps the best way to understand innovation that is transformative is by example. The Apollo Program and the Manhattan Project — two of the US's most memorable instances of government-created innovation — did not generate potential, they generated results. They did not create new fundamental understanding; they connected fundamental understanding already in place. They did not overthrow scientific paradigms; they entrenched them. They did not create new pathways; they followed old pathways to the point of their intersection. Neither was *potentially* transformative; both were *actually* transformative: they effected radical change in economies, politics, and culture. Through purposeful integration of expertise, they secured American competitiveness for decades.

The NSB definition serves as the centerpiece of the discourse on transformative *research*, and it offers a useful framework to understand transformative *innovation* in terms of the connected model (Bonvillian 2009; Weiss & Bonvillian 2009). As noted above, most of the text in the NSB definition describes the nature of radical change that results from the research: scientific concepts or fields that challenge current understanding, i.e. a change in paradigm. The grammatical core of the definition, however, is that it is 'driven by ideas that have the potential to radically change' something. From the perspective of innovation, rather than research, what does it mean for change to be radical? To be driven by ideas of change? To have the potential to make radical change happen?

First, transformative innovation means that the vision is radical to a broad range of stakeholders. The vision of change and the pathway to its realization should involve communities beyond the research community. Second, it means being driven by ideas that are tangible. We should be able to develop a vision of change and a plausible path from research to innovation. Third, it means having organizational potential as well as individual potential. We must be able to believe that the institutions and managerial culture can actually make progress toward that vision of radical change.

Revolutions Outside the Balkans

Kuhn defines a scientific revolution as 'a transformation of the world within which scientific work was done' (Kuhn 1969/1996, p. 6). He notes that transformative research may only be a professional transformation not for society at large, but for scientists themselves, and often a small community within science. Scientific revolutions 'need seem revolutionary only to those whose paradigms are affected by them. To outsiders they may, like the Balkan revolutions of the early twentieth century, seem normal parts of the developmental process' (Kuhn 1969/1996, p. 92).

Similarly, the NSB definition places emphasis on changes in 'understanding', 'concepts' and the fields of 'science and engineering'. It does not address how radical change exhibited in these domains might cause or coincide with a radical change in others. But radical change need not be limited to the more conceptual realms of science and engineering, especially when social and economic motives drive it. Transformative innovation means R&D that transcends its original context to have impact upon another discipline or another profession. To the extent that transformation is intentional, it should be intended for applications and stakeholders 'outside the Balkans'. In this way, we might understand radicality as the breadth of impact.

The idea of radical versus incremental change is not new. As previously noted, Kuhn and Dosi discussed normal versus revolutionary science and technology, respectively. Radical and incremental are terms introduced by Abernathy and Utterback (1978). Porter (1985) made the distinction between continuous and discontinuous technological evolution. Christensen (2003) has distinguished between disruptive and sustaining innovations; Taleb (2007), between exogenous events — often scientific or technological in nature — that are black swans and white swans.

What these dichotomies share is that, with radical types of change, the state of affairs after is qualitatively different from that before. The change is sharp rather than smooth, unpredictable rather than predictable — perhaps completely unexpected. Yet not all radical change is the same; it can be exhibited in different domains. The change can be one of scientific knowledge and concepts, technological artifacts and capabilities, institutions and modes of business, or personal and socio-cultural meanings. Lundvall stresses the importance of making these distinctions and provides some tangible examples:

> When distinguishing between incremental and radical innovations, we may refer, primarily, either to the technical or to the economic dimension. Some innovations, incremental in technical terms, may have a crucial impact upon the economy. This will be true for a small technical change solving a bottleneck problem of strategic importance (the introduction of vehicles with inflated rubber wheels in agriculture did not represent any radical technical break but it had a dramatic impact upon the productivity of this sector). On the other hand, an innovation very radical in technical terms, and signaling a new technological paradigm, might for technical reasons be premature and have a very limited impact on the economy (it took the Babbage version of the computer, obviously a radical innovation in technical terms, more than a century before it had any economic impact at all). It follows that many radical innovations will be radical only in one of the two dimensions while remaining incremental in the other dimension (Lundvall 2010, p. 12).

Christensen's distinction between innovations that are disruptive and those that are sustaining is between new and old business models, value propositions, and customer groups (Christensen 2003). Christensen's use of the word 'disruptive' also distinguishes a radical change in technology from a radical change in business, that the former need not cause the latter, and that the latter is often more critical. According to Verganti, however, it is not technological or business model innovation that matters most for radical innovation. 'People do not buy products but meanings. People use things for profound emotional, psychological, and sociocultural reasons as well as utilitarian ones' (Verganti 2009, p. 4). Radical innovation can just as often concern symbols, identities, and emotions as technologies, function, and performance. The Apollo Program and Manhattan Project were both wrought with Cold War symbolism. Such radical outcomes are seldom the focus for research and development, but often are the focus for 'design-driven' approaches to innovation.

From the perspective of the connected model, transformation is not merely invention, but innovation; not simply having a radical idea but making an idea relevant and responsive to the larger community. A radical change in one sphere of science and technology might or might not cause a radical change in other spheres. Agencies attempting to engage in transformative

innovation ought, therefore, to aim for outcomes relevant to a broad number of stakeholders and to focus on how best to facilitate changes across domains, rather than within them.

Beyond Milquetoast Objectives

Transformations may, indeed, be impossible to predict, but agencies can be strategic about them. Transformative innovation has rich and tangible objectives at its core, as opposed to undirected research or research with what one researcher, paleoanthropologist (and blogger) John Hawks, calls 'milquetoast' objectives:

> I don't want there to be a pool of money set aside for 'high-risk' or 'transformative' work. I want the agencies to set transformative objectives and to fund projects in accordance with them. If my scientific objectives don't match those of the agency for my research area, I want to know that so I don't spend time on fruitless applications. If a grant agency has milquetoast objectives, I want a transparent process by which ordinary scientists can participate in changing those objectives (Hawks 2011).

A transformation being planned for is more likely to occur if there is a clear outcome envisioned, and if the path to get there is well conceived; that is, if we can see the logic leading from R&D to outcomes. Hughes et al. place the high-risk-high-reward programs that they evaluate within a theory of change (Hughes et al. 2011; Rogers et al. 2000). Van de Ven calls this process managing ideas into 'good currency' within an organization, and recommends program planning techniques to do this (Van de Ven 1986).

This does not mean that planning for a radical vision is undertaken in the same way as we would for incremental improvement, for example, by setting and progressing through a series of technical readiness levels. Radical innovation at DARPA, for example, is driven by collaborative and iterative vision development, rather than stage-gate reviews of performance, and it is typically enabled by decisions of small groups of leaders, rather than consensus-seeking boards, committees, or task forces. Carleton describes how radical R&D is often driven by technical

visions, which 'describe the future state of technology concepts that rely on engineering knowledge to build' (Carleton 2010). Lynn and Akgün found that having a clear vision, 'a well-articulated, easy to understand target' (Lynn & Akgün 2001, p. 375) was significantly associated with new product success for radical innovations.

Being prospective about transformation requires a push for some particular radical innovation, which is different from innovation that is not driven at all, where the outcomes are not merely unpredictable, but genuinely unknowable. Transformation may occur serendipitously as the result of an undirected research program, but there is no criterion that we can use, ex ante, to choose one program over another. We can, of course, play the odds by diversifying R&D portfolios: choosing fewer projects with predictable outcomes and more projects with unpredictable (or truly unknowable) outcomes. But we can also be intentional about transformation by driving toward clear ideas of radical change.

In other words, transformative research is what others have called *purposive* research (Shapley & Roy 1985), *strategic* research (OECD 2002), or *use inspired* research (Stokes 1997). In the absence of certainty, as Stokes (1997) states, it is still possible to make choices strategically:

> The logic of classifying research on the basis of intended goals rather than known achievements rests on the fact that policy has to do with choice — the choices facing individual scientists, the choices facing those who match resources to alternative research uses at the retail or wholesale level. All of these require ex ante judgments under the uncertainty that is an inherent part of research yet to be done. Although the historian of science will in due time be able to give far more assured judgments as to which research proved in fact to advance the general understanding of a field and which in fact led to significant use, only a framework that deals ex ante with goals of research can serve the needs of science and technology policy (Stokes 1997, p. 78).

Implicit in the NSB definition is the assumption that we can be prospective about transformation, beyond assigning likelihood to serendipity, by knowing the ideas of change that drive research. The NSB's hypothetical researcher does more than simply acknowledge that doing new things

may yield transformative results; she has a 'driving idea' of what form that transformation might take. For Kuhn, paradigms are abandoned only when there is another available for adoption; even if she cannot foresee the outcome with precision, when she intentionally chooses a novel direction for her research, the researcher is driven by an alternate paradigm in the back of her mind or by an alternate technological trajectory that she wishes to see realized. For transformative innovation, the driving idea is made explicit, and it is the standard by which all progress is judged.

Recipes Rather Than Ingredients

Transformative research is also known as 'high-risk-high-reward' (National Academy of Sciences 2007; US Congress 2007). The American Academy of Arts and Sciences notes that in potentially paradigm-changing research, 'the probability of failure is significant' (American Academy of Arts and Sciences 2008). In our present context, 'potential' often seems to warn us of risk rather than mitigate it, and to temper expectations rather than help make informed choices. NSF director Arden Bement (2007), on the other hand, characterized transformative research as having a 'reasonable chance' of success. 'Potential' need not emphasize doubt; it can evoke plausibility and trust. The possibility of payoff is as real as the risk. Instead of merely accepting the odds, how might we improve them?

We can start by understanding the sources of uncertainty. Hughes et al. describe risk as associated with four kinds of novelty in research. *Technical* risk arises from 'equipment or techniques that have not been proven or are extraordinarily difficult'. *Experiential* risk arises from 'knowledge of fields beyond the researcher's previously demonstrated area of expertise'. *Multidisciplinarity* risk arises from 'a unique and unprecedented combination of perspectives, disciplines, or approaches', and *conceptual* risk arises from 'fundamental ideas [that] are at odds with prevailing wisdom' (Hughes et al. 2011, p. 7). In addition to the risk of failure, there is also

the *risk of success* that stems from the resistance of change to a paradigm or trajectory; those who have a vested interest in the status quo could lose power if things changed radically (Bonvillian & Van Atta 2011).

But we ought not to use our determination of high-risk as a proxy for high-risk-*high-reward*. Research that has technical, experiential, multidisciplinary, or conceptual risks — or that challenges economically or politically favored incumbents — could just as likely be high-risk-*low-reward*. In her analysis of one federal program, Lal has found that research perceived to be risky by peers was not strongly associated with transformative outcomes (Lal 2012). Potential to transform cannot be a function only of risk, and policy to encourage transformation should not simply be concerned with making riskier choices. As noted above, we can always choose to set a proportion of less versus more predictable investments (such as setting R&D at 5% of GDP, or transformative research at 50% of R&D expenditures), but we can also work to change the less predictable into the more predictable — to find potential and increase potential, thereby lowering the risks of investments believed to have high reward.

We might think that potential is about something we can be relatively certain of: the number and quality of individual scientists and engineers. This is consistent with contemporary economic arguments for STEM (science, technology, engineering, and mathematics) education. Romer (2000), for example, has argued for education policy that produces a strong supply of scientists and engineers (and, inversely a low cost of this kind of labor) as a prerequisite for innovation-driven economic growth. We might also think that diversity of talent is also a reliable indicator of potential. Indeed, interdisciplinarity is often cited as a marker of radical innovation. Moreover, it simply makes sense — especially from the perspective of a connected model — that grand endeavors will require a greater number of different skill sets.

However, there is no evidence that quality and diversity of talent are, by themselves, a trigger of transformative outcomes. In fact, Lal provides evidence that suggests that they are not (Lal 2012). While quality and diversity of talent may be necessary, they are not sufficient. Achieving radical outcomes demands deliberate institutional mechanisms for enabling stakeholders of multiple disciplines to work together. We can say that R&D efforts have the potential for transformation only to the extent that such mechanisms are in place. Transformations have higher potential when we have — to extend Romer's metaphor from innovation to the *management of innovation* — better recipes, i.e. the ways people and talent are combined through organization and management, as well as better ingredients, i.e. people and their respective talents (Bonvillian 2009).

Scientists and engineers cannot produce transformative innovation by themselves. In addition to the talent of those who conduct the R&D, we must also consider the talent of those who manage the R&D. Baumol (2004) reminds us that, while mastery of science and technology is essential for incremental innovation, a different type of background — often less educated — produces the imaginative, unorthodox thinking of entrepreneurs and breakthrough innovations. Carleton describes how program managers at DARPA must be 'technical visionaries' (Carleton 2010). The so-called 'great groups' responsible for radical innovations contain not only superb people (often generalists rather than specialists), but also strong leaders (Bennis & Biederman 1997).

If radical change implies 'revolutions outside of the Balkans', it necessarily involves connections. Value is realized not only through specialized knowledge of scientists and engineers in isolation, and not even merely through their combination, but in *how* they are combined. Transformative innovation requires conscious decisions about how R&D is organized and managed. From this perspective, the challenges become: How does an agency balance the need to isolate R&D activities from operational concerns while keeping them connected enough to be effective? How

can specialists of diverse expertise within organizations work together strategically? How can multiple organizations work together effectively on national or global innovation challenges that transcend parochial perspectives of mission agencies? These are central rather than peripheral issues for agencies attempting transformative innovation.

Discussion

An R&D program embodies transformative innovation if it is intended to be radical in its benefits to society and economy, not merely in its challenge to prevailing scientific paradigms; it is driven by a clear and tangible objective, not discovery of 'unknown unknowns'; and it combines all the capabilities necessary to achieve that objective. Where these factors are combined, we should expect transformative innovation. (Table 1 summarizes how the perspective of transformative innovation differs from that of transformative research.) We can choose transformative R&D efforts based on which problems they aim to solve, how well they define those problems, and how well they are organized to solve them, not based on scientific discipline or theme. This comports well with Sarewitz's finding that 'the logical coherence of the public values a research program uses to justify it activities is related to the capacity of the program to achieve those values' (Sarewitz 2011, p. 346). We can increase transformative potential by improving the way programs are managed, not (necessarily) increasing their budgets. Transformative federal R&D cannot occur without organizations that embody transformation.

Table 1: Transformation from the perspectives of research and innovation

	Transformative Research	**Transformative Innovation**
Radical	Radical change in fundamental scientific understanding	Radical change in broad social and economic welfare

Driven	Driven toward discovery of the unknown and unexpected	Driven toward a clear vision of an discrete outcome
Potential	Potential may or may not be realized by someone else	Potential can only be realized by organizing diverse talent

Such a perspective is not new. In a broader sense, these factors are the fundamental principles underlying any attempt at strategic planning and management. What Bozeman and Sarewitz said of federal research in general should hold doubly true for programs that are specifically intended to create radical social and economic outcomes:

> Given all the complexity, for any program(me) we can still ask: What public values are sought? Are the strategies for linking institutions, network actors, and individuals viable? Is the causal logic sound? Are the human and institutional resources in place? (Sarewitz 2012, citing Bozeman 2002)

The case presented here reinforces the argument that, even though the process of innovation occurs in a complex ecosystem, it is pragmatic to plan and evaluate R&D programs and it is especially so for purportedly transformative R&D programs. Indeed, what enables transformation is the fact that programs are strategically managed with respect to outcomes.

Serendipity and Strategy

It may not sit comfortably with many in the research community that transformative innovation could have more to do with strategic management of the investments in R&D, than simply increasing those investments. Serendipitous discovery occupies an almost holy place in scientific culture. However, while it is an essential element of transformation, serendipity need not preclude strategy. Further, the sources of serendipity extend beyond efforts to gain fundamental understanding about the natural world.

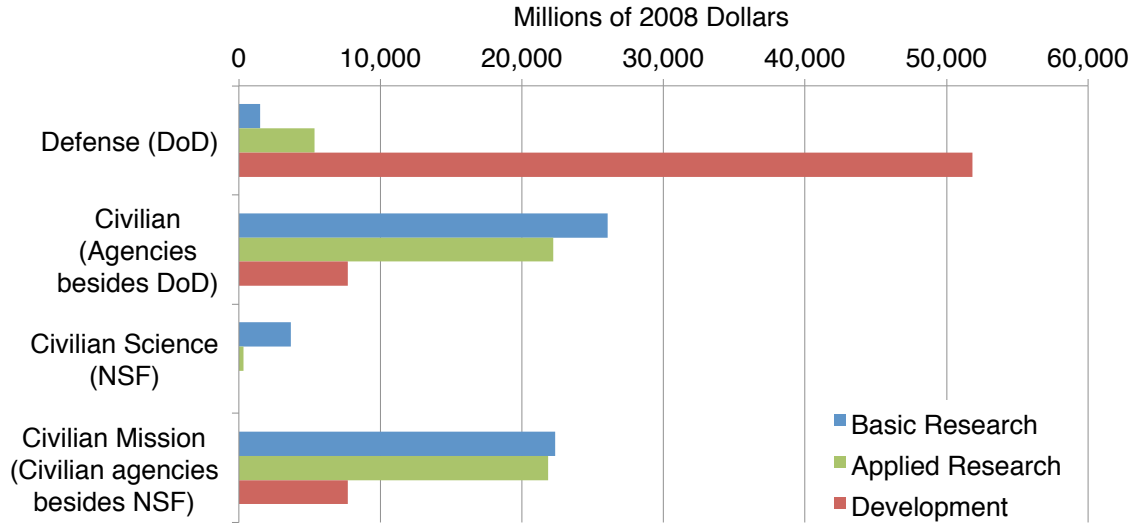
The popular understanding of R&D, rooted in the linear model, is that radical breakthroughs result from unexpected results in so-called basic research. The objection to being more strategic is that, while we may increase the predictability of particular outcomes, we reduce the potential for unexpected outcomes. The opportunity cost of strategic R&D is, allegedly, decreased potential for transformation associated with basic research. Some argue that tangible objectives are constraints imposed upon research, and that fewer constraints will more often result in serendipity, or, alternatively, that the value of serendipity in fundamental understanding is greater than that of application.

Such arguments may be intuitively appealing, but they are not cogent. Serendipity is important for innovation. We should create space for it to happen and invest willfully in projects that have little chance of success, and in some that have no foreseeable outcomes at all, but we should be strategic about the constitution of portfolios and balance them with respect to risk and reward.

For the sake of argument, let us accept the linear categories of basic research, applied research, and development. Basic should presumably come with more risk and more serendipitous benefit than applied research, and applied research more than development. How does the federal R&D budget stack up as a portfolio of investments? Is it well-balanced? For defense related R&D, the portfolio distribution is as one might expect: most funds for development, far fewer for applied, and fewer still for basic (see Figure 1). Yet in civilian agencies, representing half of federal investments in R&D, the proportionality is flipped: most for basic, less for applied, and far less for development — and this has grown more extreme over the past decade (National Science Foundation 2002, 2010b, 2012). As important as serendipity is for transforming the mission of civilian agencies, current policies favor opportunities to get the unexpected over opportunities to do something about it. As Hackett et al. (2008) note, being strategic does not ignore serendipity, it organizes programs to capitalize on serendipity.

Serendipity is unavoidable, and the places in which it can emerge are more numerous from the perspective of innovation than from the perspective of research. There is no reason to suppose that serendipity arises asymmetrically across domains of expertise. We should not expect the unexpected any more often in physics than in engineering or in product design. Serendipitous results are black swans that emerge from the depths of our uncertainty. They are, for all intents and purposes, random. We should expect them to occur with the same level of unpredictability in every kind of endeavor, in every field of specialization. While the randomness of basic research may result in the kinds of deep understanding that could ultimately impact a very wide range of applications, the randomness of strategic research occurs across a broader spectrum of specialized fields. Moreover, that randomness arises within a system more likely to exploit it.

Figure 1: Balance of the Federal R&D portfolio. Estimated Federal obligations for R&D, by selected agency and character of work: fiscal year 2008 (National Science Foundation 2010).



Being strategic about transformative innovation is more than setting tangible objectives and managing portfolios; it is also about executing programs. Matso (2012) delves into strategic

execution of 'usable science' programs in his case studies of applied research programs at the Cooperative Institute for Coastal and Estuarine Environmental Technology and the National Estuarine Research Reserve System, both partnerships between the National Oceanic Atmospheric Administration and the University of New Hampshire. While these programs were intended to produce science that was immediately applicable to coastal resource decisions, Matso found that requests for proposals were designed to build capabilities to generate scientific understanding, not to engage decision-makers in the research process. He recommends programs confront the choice between knowledge creation and diffusion, between credibility and utility, and between natural scientists and experts in collaboration and translating science.

In grant design, for instance, we can choose between the R&D activities aimed at solving problems, and the community building activities of defining what problems need to be solved. When applications are received, reviewers who understand engagement should be factored in to a greater degree than it often is (Matso 2012). Transformative innovation requires aligning resource information with activities defined as either creation or diffusion. This distinction can also be used to create a more appropriate scheme of performance evaluation, e.g. bibliometric indicators of scientific merit, and qualitative indicators of knowledge transfer.

Politics and Professions

If US Federal research agencies are to be, as suggested here, more specific and determined about producing transformative innovations, they must focus on some challenges rather than others, and work with certain partners rather than others. The more disruptive the change, the greater is the potential for a redistribution of power from those who benefit from the status quo to newcomers. As Sapolsky and Taylor point out, 'innovation may benefit society, but it has its victims, and these victims fight back' (2011, p. 33). Policy decisions about transformative innovation become ideological. Political support and resistance are major factors shaping the ability of federal

agencies to enable transformations to happen. The Manhattan Project and Apollo Program, to continue with previous examples, both were Presidential choices with tremendous consequences for the United States (and the world). A few other examples illustrate the point further.

Consider The Defense Advanced Research Projects Agency (DARPA), which 'was formed to address the problem of transformative innovation' (Bonvillian & Van Atta 2011, p. 470). Federal defense agencies, and DARPA in particular, were central to the major innovation waves of the twentieth century, from interchangeable parts and mass production to aircraft, nuclear energy, space transportation and remote sensing, as well as the computer and the internet (Ruttan 2006). The interactive graphical user interface, often held up as an example of the private-sector innovation of Xerox, Apple and Microsoft, had its beginning in publicly-funded Defense Department R&D (Fong 2001). DARPA has garnered political support throughout its multi-decadal history because its objectives are clearly in the interest of national security, a policy area in which there is significant agreement in American politics.

On the other hand, there is the Advanced Technology Program (ATP), once housed within the National Institute of Standards and Technology (NIST), but now defunct. ATP has had a fate very different from DARPA. It was established in 1990 to 'bridge the gap between the research lab and the marketplace… through partnerships with the private sector' (Advanced Technology Program 2011), but by focusing on bridging the valley of death for particular technologies and working with particular partners in the private sector, ATP was perceived by many to be interfering with the free market by 'picking winners'. Congress attempted multiple times to abolish it, eventually succeeding in 2007 with the America COMPETES Act in 2007 (US Congress 2011). The Program was altered to address these concerns and rechristened as the Technology Innovation Program (TIP), but was defunded and shut down with the FY 2012 Appropriation Act (Technology Innovation Program, National Institute of Standards and

Technology 2012). According to Christopher Hill, 'ATP's major problem [was] that it operates at the nexus of the debate over the appropriate role of the federal government in support of commercial industrial innovation' (Leath 2002).

The fate of a third agency, the Energy Advanced Research Projects Agency (ARPA-E), has yet to be determined. As its name suggests, ARPA-E is an attempt to apply the DARPA business model to the development and transfer of energy technology, but the political environment into which it has entered is markedly different from that of defense. Energy policy is deeply political and the federal government has historically taken an active and interventionist role. Rarely has policy been technology-neutral; forces on the political right tend to favor R&D to advance technology based on fossil and nuclear fuels, while those on the left favor renewable sources and energy efficiency (Weiss & Bonvillian 2009). Energy is a 'complex established legacy sector', and despite the fact that US Presidents have been calling for energy independence for decades, there has been little success in moving energy technology advances into commercial application because of the difficulty of driving costs down to competitive levels (Bonvillian & Van Atta 2011). As Weiss and Bonvillian (2009) point out:

> Most new energy technologies face a more difficult challenge than innovations in, say, the IT industry… Energy technologies… will parachute into occupied territory, with highly cost-efficient and subsidized legacy technological competitors waiting, ready to open fire. This landscape could be characterized as an 'anti-commons' (Weiss & Bonvillian 2009, p. 14).

The kind of R&D that has the best potential to realize transformation also carries with it the greatest political risks for advocates, not to mention economic costs for incumbents. In the US, at least, explicit industrial policy (outside the defense and agriculture industries) tends to engender controversy. Thus, transformative innovation programs tend to be created in response to existential threats: the atom bomb as response to an hypothetical Nazi weapon, under Roosevelt; both DARPA and Apollo in response to Sputnik, under Eisenhower and Kennedy, respectively;

ATP in response to economic competition from Asia, under Clinton; and ARPA-E in response to challenges of energy security and climate change, under Obama. Can federal efforts at transformation be made not in response to crisis, but in anticipation of new opportunities? Can they be organized not from the top down by chief executives, but from the bottom up, by stakeholders? These are questions for future transformation advocates to answer.

There are also political issues to contend with at street-level of federal bureaucracies. Shifting an agency's focus from *potentially transformative* to *actually transformative* will certainly be at odds with the prevailing professional interests and institutions. Just as those on the political right might condemn such a choice as being antithetical to the free market, those in traditional research programs might label it as 'anti-science' because the goal would not be to increase the stock of knowledge. Furthermore, it may not be easy to change incentive structures within organizations from a focus on scientific merit, as judged by peers, to a focus on value created, as judged by those who might apply research results. This explains why DARPA and ARPA-E are managed outside of existing research divisions, recruit program managers from outside of government, and only fund extramural projects (Bonvillian & Van Atta 2011).

At a personal level, transformative innovation connects communities of experts with different professional cultures, languages, and values. While there is great potential to realize the latent synergy that exists within cognitively diverse groups, there can also be tension. Scott Page has shown how groups with diverse expertise can outperform groups of specialists on complex tasks, assuming members can communicate well enough to overcome any hindrances from their diversity (Page 2007). Similarly, Hollingsworth and Hollingsworth (2011) found that while major discoveries made in biomedical R&D over the twentieth century were facilitated by moderately high scientific diversity in research institutions, major discoveries were constrained by hyper-diversity, which inhibited effective communication among actors in different fields. In

this regard, a fruitful avenue for future research might be to understand and improve knowledge transfer and sense-making (Weick 1995) within groups of hyper-diverse specialists.

Conclusion

Advocates of transformative research seem to use the term from the perspective of the linear model of innovation. The desire for 'ground-breaking' or 'paradigm-shifting' research stems from a continued focus on what is usually called fundamental or basic research. Research such as this occupies an important place in society, but it is not a new concept, and does not directly address the oft-stated concern for economic growth. While transformative research may yield the potential for transformation, it does not necessarily yield actual transformation. Arguments for greater federal investment in transformative research, are, ironically, arguments for more of the same.

Making transformations real requires something else – something totally radical. Transformative innovation, in contrast to transformative research, entails revolutions outside the Balkans, objectives that are not mere milquetoast, and a focus on the recipes rather than ingredients. We would aim for radical change in broad social and economic welfare, not fundamental scientific understanding. We would be driven toward a clear vision of a discrete outcome, not discovery of the unknown and unexpected. We would understand potential as the opportunity for value that can only be realized by organizing diverse talent, not as a warning that value may or may not be realized by someone else. Adopting such a stance inherently means being more strategically minded and more politically engaged than is typically the case for federal R&D programs.

We should, of course, remove as many bureaucratic impediments to R&D as possible; there is no surer killer of innovation than red tape. And we should take care to ensure that the costs of

managing R&D toward tangible objectives of broad application do not outweigh the benefits. However, we should not assume that a strategic approach of managing to objectives, in and of itself, precludes the realization of objectives that are transformative. It may actually increase the odds, as the type of R&D that is organized with respect to outcomes is more likely to achieve them. Moreover, being strategic does not stop with well-crafted strategic plan, but continues managing programs, and portfolios of programs, with respect to the objectives that have been set.

Agencies that are serious about producing transformations through R&D must not only be organized to realize concrete visions of change beyond the research community, they must also be willing to face controversial issues head on by engaging both their allies and rivals in the policy process. Transformative innovation requires talented scientists, engineers, and managers, as well as masters of political advocacy. It demands the growth of transdisciplinary cultures and the development of capabilities for collective problem-solving among hyper-diverse professionals. Being totally radical about innovation means paying attention to human beings as much as their creations, and creating the social conditions within which new technologies can thrive.

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3

Island+Bridge:

Organization for Innovation

for Transformation

Summary

Achieving radical societal outcomes demands institutions that allow groups of people to tackle large, complex problems. This paper explores the organizational factors at play in transformative innovation, as realized by three federal agencies designed to create it: The Defense Advanced Research Projects Agency (DARPA), the Advanced Research Projects Agency for Energy (ARPA-E), and the Advanced Technology Program (ATP, now defunct) at the National institute of Standards and Technology (NIST). Using a mixed methods approach, I develop a model of innovation based on the island+bridge metaphor for how these agencies work. The result is a descriptive and graphical account of organizational heuristics – managerial rules of thumb – at play at these agencies. By attending to the institutions of innovation, we can improve the efficacy of programs at any scale, such that even federal research programs that are small can increase their chances of creating changes that are big.

Introduction

Federal innovation policy options often take the form of initiatives that are budgetary allocations for domain-specific work. The Obama Administration's BRAIN initiative, targeting neuroscience, is the most recent example (Executive Office of the President 2013). Within the National Science Foundation and at mission agencies, the distribution of federal research grants typically follows this model: allocate resources by discipline or application, and hope for the best. However, innovation policy can be more than deciding which scientific domains to shower with resources – especially if policy is intended to producing outcomes for society. In addition to budgetary questions about funding by field, we can address administrative questions about how R&D is conducted.

This is particularly true of 'transformative innovation,' that is, work *organized to realize concrete visions of change beyond the research community* (Sen 2013). Achieving radical societal outcomes demands more than money, it demands institutions that allow groups of people to tackle large, complex problems. Transformations require — to extend Romer's (1990) popular metaphor — not just better ingredients (i.e. knowledge and talents), but better recipes (i.e. ways of combining knowledge and talent). By attending to the institutions of innovation, we can improve the efficacy of programs at any scale, such that even federal R&D programs that are small can increase their chances of creating changes that are big. Moreover, in framing budgetary choices for research endeavors, we might consider organizational factors in addition to disciplinary commitments.

This paper explores the organizational factors at play in transformative innovation, as realized by three agencies designed to create it: The Defense Advanced Research Projects Agency (DARPA), the Advanced Research Projects Agency for Energy (ARPA-E), and the Advanced Technology

Program (ATP, now defunct) at the National institute of Standards and Technology (NIST). I begin with background on the topic of organizing innovation in general and transformative innovation in particular. Next, I detail a mixed methods approach to grounding our understanding of managerial experience at DARPA, ARPA-E, and ATP. The result is a descriptive and graphical account of organizational heuristics – managerial rules of thumb – at play at these agencies.

Organizing Innovation

I assume that innovation is a product of organization and, further, that different organizational factors can produce different types and degrees of innovation. Organizational structure is the factor that has received a particularly large amount of attention in the literature. According to the Organization for Economic Cooperation and Development's *Oslo Manual: Guidelines for Collecting and Interpreting Innovation Data*…

> A firm's organisational structure can affect the efficiency of innovation activities, with some structures better suited to particular environments. For example, a greater degree of organisational integration may improve the co-ordination, planning and implementation of innovation strategies. Organisational integration can work particularly well in industries characterised by incremental changes in knowledge and technologies. A looser, more flexible form of organization, which allows workers greater autonomy to make decisions and define their responsibilities, might be more effective in generating more radical innovations (Organisation for Economic Co-operation and Development 2005, p. 31).

One of the most common organizational structures for radical innovation is the segregation of innovators from normal business operations in a distinct organizational unit that has greater freedom from the bureaucracy. Lockheed Martin's 'Skunkworks' (Lockheed Martin Corporation 2012), Xerox's Palo Alto Research Center (PARC 2012) are among the most notable examples of this, and this is the general philosophy underlying any organization's choice to have an R&D division that is distinct from its other business units.

The research of Ettlie, Bridges and O'Keefe (1984) in the food processing industry suggested that radical innovation requires organizational partitions and the concentration of technical specialists, along with implementation through centralized decision-making by more supportive top managers and organizational generalists. More recently, Argyres and Silverman (2004) found that firms with centralized rather than decentralized R&D (that is, those with corporate labs and corporate funding) generate innovations that have a higher level of impact, and impact upon a broader range of technological areas. They argue that this is due to the fact that research efforts are decoupled from the immediate concerns of operational units and that the transaction costs associated with internal R&D coordination are reduced.

Another common structure is one with less hierarchy: an open innovation network. Rather than conducting all R&D in-house, organizations are increasingly relying on the R&D conducted elsewhere (e.g. at universities or start-up companies). In the private sector this occurs through licensing agreements or buy outs, in the public sector through grants and cooperative agreements. An open model assumes that one does not have to originate ideas in order to benefit from them and that many the most knowledgeable people work somewhere else (Chesbrough 2006). The R&D conducted within one organization may have greater potential for application at a different organization, or its application may rely heavily upon complementary capabilities of a partner. Either way, the boundaries between the R&D functions of organizations is more porous than in a traditional, closed model. The open innovation model is increasingly popular in the private sector, where there is increasing competition from a globalized market and where the return on investment for internal R&D at large, mature companies has been in decline (Jaruzelski & Holman 2011).

Some innovation models are isolated and open at the same time. As Bennis and Biederman write of the many groups responsible for transformative innovation, every one 'is an island, but an

island with a bridge to the mainland' (Bennis & Biederman 1997, p. 206). The island is the refuge for experimentation and failure, and the bridge is the conduit for the transfer of knowledge and technology. New technological capabilities make their way out, requirements, resources, and other sorts of influence must make their way in.

The island-with-a-bridge metaphor suggested by Bennis and Biederman seems to describe DARPA, which 'was formed to address the problem of transformative innovation' (Bonvillian & Van Atta 2011, p. 470) during the Cold War, the centralized R&D units of US Army and Navy became too focused on fundamental principles and produced innovation that was too incremental. DARPA is most well known for developing the ARPANET (precursor of the Internet) and stealth aircraft. Even the interactive graphical user interface, often held up as an example of private-sector innovation of Xerox, Apple and Microsoft, had its beginning with DARPA funding (Fong 2001). The hybrid island+bridge model was also the basis for a number of innovative organizations in the federal sector that were modeled on DARPA, including ARPA-E and ATP, as well as the Intelligence Advanced Research Projects Activity (IARPA), and In-Q-Tel, a non-federal partner of the Central Intelligence Agency (Cotell 2006; Yanuzzi 2000).

Organizing Innovators

DARPA has been uniquely and systematically successful at transformative innovation, according to Bonvillian, because it was consciously managed not only at the institutional level, but at the personal levels as well. At the personal level, the island+bridge model at DARPA has manifested itself as 'great groups' of superb people who work well together under great leaders who keep them separate from external concerns and focused on a common goal (Bennis & Biederman 1997). Though it is separated from operational concerns as a formal institution, DARPA is still intimately connected to the outside world though people. Bonvillian suggests that the qualities of

intra-personal interaction characterize successful innovation not only at DAPRA, but also at Edison's Menlo Park lab, the MIT Rad Lab, and Bell Labs:

> The rules include ensuring: a highly-collaborative team or group of great talent; a non-hierarchical, flat and democratic structure where all can contribute; a cross-disciplinary talent mix, including experimental and theoretical skills sets networked to the best thinking in relevant areas; organization around a challenge model; using a connected science model able to move breakthroughs across fundamental, applied, development and prototype stages; cooperative, collaborative leaders able to promote intense, high morale; and direct access to top decision makers able to implement the group's findings (Bonvillian 2009, p. 218).

This distinction between institutional and interpersonal management parallels a more general distinction made by Lam (2005) between organizational innovation that focuses on institutional structuring and that which focuses on group cognition, that is, the thinking and learning of an organization's members. 'Great groups' are not just full of talented people, but 'full of talented people who can work together' (Bennis & Biederman 1997, p. 202). Organizational innovation for transformative results must be, in significant ways, a psycho-social phenomenon of overcoming institutional momentum (North 1990), changing cultures and cognitive processes (Scott 2008), and collective sense-making among individuals (Weick 1995).

Organizational learning and group cognition may be particularly challenging within science agencies. The unique language of scientists and engineers is not always shared with their stakeholders, leadership, or with colleagues of other disciplines. Increasingly, innovation occurs across complex, fragmented networks of expertise rather than in single, hierarchical organizations, and specialized knowledge can weigh us down as much as it lifts us up (Rammert 2006). Porter (1985) noted that the transaction costs of coordination increase with greater specialization, and he suggested that organizations must have a language system to make interrelationships tangible, and for members to discuss and analyze them. Van de Ven notes:

> Innovation is not an individual activity — it is a collective achievement. Therefore, over time there is also a proliferation of people (with diverse skills, resources, and interests) who become involved in the innovation process. When a single innovative idea is expressed to others, it proliferates into multiple ideas because people have diverse frames of reference, or interpretive schemas, that filter their perceptions. These differing perceptions and frames of reference are amplified by the proliferation of transactions or relationships among people and organizational units that occur as the innovation unfolds. Indeed, management of the innovation process can be viewed as managing increasing bundles of transactions over time (Van de Ven 1986, p. 597).

In addition to the talent of those who conduct the R&D, therefore, we must also consider the talent of those who manage the R&D. Baumol (2004) reminds us that, while mastery of science and technology is essential for incremental innovation, a different type of background — often less educated — produces the imaginative, unorthodox thinking of entrepreneurs and breakthrough innovations. Carleton describes how program managers at DARPA must be 'technical visionaries' (Carleton 2010). The so-called 'great groups' that are responsible for radical innovations contain not only superb people, but also strong leaders (Bennis & Biederman 1997).

Methods

To investigate the island+bridge model further, I examined the work of three federal agencies: The Defense Advanced Research Projects Agency (DARPA), the Advanced Technology Program (ATP), and the Advanced Research Projects Agency for Energy (ARPA-E). My driving question was: *what are the organizational factors that foster transformative innovation in the federal R&D programs that succeed in creating it?* For the purposes of this research, I assume transformative innovation implies that efforts are 'organized to realize concrete visions of change beyond the research community' (Sen 2013 and Chapter 2, above). I chose DARPA, ATP, and ARPA-E as model agencies because all three were created and run from this perspective. ATP and ARPA-E were both created based on the DARPA model at different points in recent past and, like DARPA,

were formed in response to national crises. For DARPA, it was the start of the cold war in the 1950s; for ATP, it was economic competition from Asia in the 1980s; and now, for ARPA-E, it is energy security and climate change.

Further, there is good reason to believe that all three agencies posess a real capacity for transformation.. DARPA's reputation for transformative innovation has been discussed extensively in the literature (Bonvillian 2006; Bonvillian & Van Atta 2011; Fong 2001; Ree, Van Atta, & Deitchman 1990; Van Atta, Lippitz, Lupo, Mahoney, & Nunn 2003) and is widely known. ATP was not aimed at outcomes as radical as DARPA. (As one manager said in interview, 'you can't be asking a small company to change the world for the cost of new six bedroom in Potomac, Maryland... it's not reasonable'.) However, it has a well-demonstrated record of success in creating technological disruptions that yielded broad benefits (Advanced Technology Program, Economic Assessment Office 2007; Kerwin & Campbell 2006; Ruegg & Feller 2003). Indeed, as will be discussed the Findings section, ATP conducted program evaluations to demonstrate economic benefit to counter political resistance. ARPA-E is only a few years old and does not have a track record yet, but has been closely modeled on DARPA, particularly in its focus on bringing advanced technologies to scale, and it organizational separation from the Energy Department's Science Division, and in the active, evidence-based approach to management (Bonvillian & Van Atta 2011; Boysen & Gur 2012; Haynes 2013).

By examining these 'model agencies', I sought to find general truths – heuristics – about how transformative innovation programs in are organized and managed, and develop a model for describing and comparing the transformative nature of federal R&D efforts. We can think of heuristics as commonly held, internal narratives – some conscious, others subconscious. They are simple rules (Davis, Eisenhardt, & Bingham 2009) or cognitive shortcuts that organizations learn when facing decisions with limited information, time, and analytical capability (Bingham &

Eisenhardt 2011). They are helpful at explaining organizational behavior 'when there is high heterogeneity in experiences, high unpredictability, and relatively few experiences' (Bingham & Eisenhardt 2011, p. 1459). While the research revealed a multitude of factors at play in the three agencies, I am ultimately interested in the logic and practices that underlie factors, rather than the specific forms factors take. Thinking in terms of heuristics that connect factors, rather than factors themselves, enabled generalization of findings across cases, and a greater possibility that findings could be applicable to organizations beyond the three studied.

To understand the organizational and managerial heuristics at these three agencies, I relied primarily upon the insight and experience of managers at these agencies responsible for running programs.[1] Using a modified grounded theory approach (Creswell 2007), I conducted semi-structured interviews with managers at each of the three agencies and collected material published by the agencies regarding their strategies and business processes. I then transcribed, coded, and analyzed the source material to create a model of the system that is 'grounded' in the data collected. My methods differ from traditional grounded theory in that I did start not with a blank slate, that is, I did not build a model of transformative innovation inductively. Rather, I used *abductive* reasoning: an iterative process of comparing empirical data with existing theory. As Coffey and Atkinson (1996) describe it…

> Abductive inferences seek to go beyond the data themselves, to locate them in explanatory or interpretive frameworks. The researcher is not content to try to slot them into existing ideas, for the search includes new, surprising, or anomalous observations. On the other hand, such strange phenomena are not used only to disconfirm existing theories: they are used to come up with new configuration of ideas. There is thus a repeated interaction among existing ideas, former findings and observations, new observations, and new ideas (Coffey and Atkinson 1996, p. 156).

[1] It should be noted that there are other perspectives on the operations of these agencies that I did not attempt to access. Research performers and those who apply and benefit from the results of research, for instance, could offer new or different insights.

I wanted to draw upon the real-world understanding and personal experiences of those who have managed such programs. At the same time, I acknowledged that investigations into the so-called 'DARPA model' are not new, so I wanted to make use of studies already performed. In particular, I drew upon the comparative analysis of DARPA and ARPA-E by Bonvillian and Van Atta (Bonvillian & Van Atta 2011), who document a comprehensive account of organizational factors – on both the institutional and personal levels – that they believe contribute to the innovative nature of the agencies. These factors (listed in Table 2) served as an initial, relatively unstructured model that I sought to develop further. Interview questions for managers at the three agencies and initial analytical codes were based upon the list.

Table 2: Factors of innovation at DARPA and ARPA-E (Bonvillian & Van Atta 2011)

Program managers have tenure that is limited (not indefinite).
Program managers have experience in business (not just government).
Program managers have talent that is managerial (not just scientific).
Program managers are entrepreneurs and translators (not subject matter experts).
Program managers have power in choosing projects (relative to superiors).
Program managers have power to hire, fire, and contract (relative to other agencies).
Project proposals are required to show a plausible pathway to implementation.
Projects have specific challenges or visions that drive them.
Projects have specific challenges or visions that are co-developed with stakeholders.
Projects are executed externally (not internally).
Projects are managed with metrics (not 'stage gates').
Projects are managed as portfolios defined by risk.
Projects are managed as portfolios defined by complementary technology 'thrusts'.
Projects are managed as portfolios with proportional investments.
Program managers actively engage with allies, rivals, and others in the innovation ecosystem.
Projects often result in application.
Cost-effective adoption is a criterion for success.
The Program finds first adopters and creates markets.
The private sector is engaged by: participation in industry consortia, funding creative companies, a team dedicated to commercialization, awarding prizes, and leveraging a reputation for innovation.
The public sector is engaged by: using agency procurement for adoption, making agreements with operational lines in the agency (or others) for adoption, connecting the results of work to the stage-gate process of partners.

From this point, my approach to answering the driving question was threefold. First, I gathered and analyzed data to better understand potential factors affecting innovation. Second, I gauged the relative importance of the factors identified. And third, I developed a more refined model to integrate the most important factors that I found.

Data and Coding

Interviews provided the majority of data for this study. Interviews were conducted with seventeen managers across three agencies, six from DARPA, five from ARPA-E, and six from ATP. Fourteen were former managers, while three currently held their positions. All interviewees were directly engaged in managing programs as their full time responsibilities, though not all had the title of 'program manager,' or its equivalent at ARPA-E, 'program director'.[2] At ATP, programs were shared between a technical manager and a business manager; interviews were conducted with both. One participant was an office director who oversaw multiple programs, and another participant was an assistant program director. One of the program managers interviewed had served in this capacity at two of the agencies, and two had also served as either the director or acting director of their respective agencies. Nearly all interviews lasted 60 minutes or longer.[3]

Data were derived from 5-6 interviews and 2-4 strategic documents made public by the agency. (Table 3 provides a list of data sources by type.) Interview data took the form of transcribed audio recordings, or typed notes made available to participants for editing. Textual data were also garnered from publications of these agencies that described their missions and business

[2] For the purposes at hand, I will refer to both as 'program managers', though it is noteworthy that one interviewee from ARPA-E found this distinction important; his role was to direct, not manage.
[3] Two interviews lasted 30-40 minutes, due to participant availability.

processes (Advanced Research Projects Agency - Energy 2012; Defense Advanced Research Projects Agency 2013; National Institute of Standards and Technology 2006; Van Atta 2008), as well as written presentation material delivered by program managers that addressed interview questions directly (Boysen & Gur 2012; Haynes 2013; Leheny 2007). These raw data were analyzed using computer assisted qualitative data analysis software (CAQDAS) called HyperResearach[4], yielding information on coded excerpts from the source material, organized by case (DARPA, ARPA-E, and ATP). This information was further analyzed and organized in Microsoft Excel. Data visualization and concept modeling were performed in OmniGraffle Pro.

Table 3: Number of coded excerpts by agency and type of source material

Source Material	**DARPA**	**ATP**	**ARPA-E**
Interview Transcripts or Notes	6	6	5
Published Strategy or Process Documents	2	1	2
Presentation Materials	1	0	2
Coded Excerpts	**474**	**507**	**487**

Coding of data was performed in four phases. Initial coding aligned responses broadly with the questions and hypotheses stated above. The second phase saw the generation of higher resolution codes to categorize types responses to each question. The third phase was one of 'horizontal' coding, where excerpted material was reviewed within codes (and across sources) to add, remove, or refine codes further. Finally, the fourth phase was quality control, in which coded excerpts were revisited to correct errant coding and remove duplicates. Theoretical codes — those I

[4] HyperResearch was chosen because it runs native to Macintosh OSX, and offers an simple, well-designed, and intuitive interface (particularly important when coding for hours at a time). While not as full-featured as other CAQDAS packages, HyperResearch offered exactly what I needed for this study: basic code-and-retrieve functions, as well as data organization and reporting by case.

assumed from the literature before conducting the analysis – were applied in the first and second phases, while emergent codes were applied in the second and third phases.

In this study, a code represents a factor that at least one interviewee (or the agency writ large, in the case of a public document) felt worth of attention in the course of discussion, and that I believed to be relevant to answering the research question. A coded excerpt represents a single instance of a factor being raised in discussion. If a factor was discussed several, independent times in an interview, it was coded as many times as it was raised.[5]

Analysis and Synthesis

The result of coding was 1466 textual excerpts (between 507 and 474 excerpts per agency), tagged by one or more of 163 codes. Codes were organized within 23 categories, corresponding to the initial list of factors identified above. Each code indicated the presence of a unique factor, some of which were mentioned many times at all three agencies, others only once or twice at one agency. The following step was, thus, to reduce the factors down from 163 to a more manageable number by isolating those in which I had most confidence.

To reduce the number of factors, I took a 'weight of evidence' approach. I assumed that I could be more certain that a particular factor was an important part of organizing transformative innovation if, in guided discussion (or in published documents on this topic), it were raised more often than other factors. Thus, the number of excerpts per code, per case, was my basis for

[5] Coding was performed in preparation for this analysis. When a topic was raised, the code for that topic was applied to that piece of text only once. Thus, if an interviewee discussed the use of prizes to engage external research partners, and raised the topic of prizes three times within the course of a few minutes (or a few sentences of notes), the code 'award prizes' would be applied only once. A code may be applied multiple times per document if it were raised again in answer to a different question, or different section of text (as in the case of a published document), or if it were raising a significantly different aspect of that topic from prior mentions. This method did demand the discretion of the researcher in some cases.

determining which factors had the surest signal. I could then order the codes from 'most certainly important' to 'least certainly important,' and focus on those above a certain cut off point. I did not, however, assume that there is one best way to sort and isolate them. Instead, I identified the top tier of factors in a few different ways, and used the combined result as the list of factors more likely to be important.

As such, I ran four simple, statistical tests of importance. For each test, the list of 163 codes was sorted by different statistical values, and only those codes with values in the top third of list, thusly sorted, would pass.[6] Passing any of the tests identified a factor as 'important', relative to those that did not. Passing more of the tests indicated that the factor is more 'robust', and therefore more likely to be important relative to those factors that passed fewer tests.

I started with those codes with *high total numbers*. The first test, therefore, was simply a sort of all codes by total number of excerpts across all cases (DARPA plus ARPA-E plus ATP). Next, I wanted those codes where numbers were *not only high, but highly consistent*. That is, I wanted those codes that had a) a large number of total mentions, and b) a small difference among the number of mentions for each case. So, the second test sorted by the arithmetic product of the total and the inverse of the standard deviation (the latter being a measure of similarity). The third test sorted by the highest minimum number, to identify those codes that had relatively high values in even the case with the lowest number of excerpts. (This test revealed those codes for which there was at least one excerpt in each of the three cases.) The fourth test sorted by highest

[6] In choosing how to define the 'top tier', I did not assume too much quantitative precision. Thus, I opted for a scale of high, medium, or low and chose those codes that were high. I initially looked at the distributions across codes for natural breaks, but the distributions were too smooth. I chose to determine importance though these multiple tests in order to create more defined cut-offs. Codes that were not found to be important on any test are clearly lower total group numbers, lower case numbers, less consistently high numbers across cases, and do not have at least one excerpt in each of the three cases within a group. (See Appendix for complete list of codes with excerpt values and associated statistics.)

maximum number, to identify those codes for which there was a particularly high value in at least one of the cases, indicating factors that may be salient, even if they are not universal.

This process yielded 96 factors as 'important' out of the original 163. (Details of test results are provided in Table 4. A full list of codes, with excerpt quantities by case, associated statistics, and test results, is provided in Appendix 1.

Table 4: Number of codes that passed each test of importance

Tests of Factor Importance	**Codes**
Test 1: 'Total is high'. (Sort by Total)	55
Test 2: 'High and highly consistent'. (Sort by Total / StdDev)	55
Test 3: 'At least *X* in all cases'. (Sort by Minimum)	68
Test 4: 'At least *X* in one case'. (Sort by Maximum)	61
4 out of 4 tests passed (most robust)	30
3 out of 4 tests passed (more robust)	47
2 out of 4 tests passed (less robust)	66
1 out of 4 tests passed (least robust)	96

After analysis of code information came the synthesis of factors represented by the codes. After isolating the 96 important factors, I recombined them into a typological model by clustering similar or related factors together. Developing the model was a process of defining and organizing clusters visually as a concept map (Bryson 2004; Novak & Cañas 2008). Clusters were formed organically and spatially. Clusters represent culturally pervasive heuristics that seemed to underlie constellations of factors. They were derived as the commonalities of clustered

factors, or of clusters of other heuristics.[7] Model development was an iterative process; heuristics were defined and redefined as factors were combined and recombined.

The final model (presented in Figure 2) is the culmination of the research process: reviewing and interpreting the literature, interviewing managers and reading agency publications, coding and note taking, analyzing and organizing data, visualizing and linking concepts, and defining and describing those concepts. The model is intended to serve as starting point for additional diagnostic research – a qualitative baseline, against which we might compare the set of organizational heuristics that underlie other R&D endeavors, or with which we might direct quantitative causal analysis through statistical analyses of relationships between heuristics and outcomes.

Findings

Within the initial frame of reference for the study, several topics of discussion stood out. The most frequently addressed topics included: the talent of the manager (versus the researcher); the nature of the manager's role; the nature of the bureaucracy within which the manager works; how projects are chosen; the definition of project portfolios; the use of challenges or visions to define and guide projects; the 'who' and 'how' of project execution; how project results are applied; as well as the 'rules of engagement' for interacting with allies, rivals, and others. (A summary of coding results, per the 23 code categories of initial factors, is provided in Table 5.)

A number of emergent codes were applied to chronological steps in the program and project life cycle.[8] Understanding this lifecycle is helpful context for the other factors. At the program

[7] In some cases, individual factors underlie more than one heuristic, but, for the purpose of reporting and visualization, each was associated with a single, primary heuristic.

level, the first step is for senior leadership, the agency director and office directors, to define a general area for research, primarily by technology and application, and hire a program manager to run the program. At ATP, the objectives of the program were predefined when the manager arrived. At DARPA and ARPA-E, the manager takes a more active role: he or she engages in a 'deep dive', engaging the broader scientific and engineering community to develop a program vision, pitches the concept to leadership, then defines more specific domains for project portfolios. Program managers at these two agencies stay in their positions for only a few years, while those at ATP stay on for longer.

At the project level, agencies solicit project concepts from the external community, through meetings or white papers, to inform their development of funding opportunity announcements. Funding opportunities are announced, proposals are received, and the manager chooses among project options with the help of a team of independent reviewers using well-defined criteria. Selected proposals result in a negotiation between the manager and the performer on project visions and performance expectations, which are codified in formal agreements. As projects are executed, the manager and his or her staff conduct regular reviews to check on progress, at least once a year. Projects end, typically, after three to four years. Managers support technology transfer as successful projects come to completion, and thereafter.

With respect to the list of initial factors, the data confirmed nearly all of them, with a handful of exceptions. First, while managers at DARPA and ARPA-E reported that they have power to hire, fire, and contract (relative to other agencies), there was a strong signal that ATP managers were not particularly flexible in this regard. Second, projects are not managed though standardized stage gates, such as Technical Readiness Levels, but they are managed though well-defined

[8] The high numbers here are largely due to the fact that these were often re-coded excerpts from those excerpts already coded within other categories.

'go/no-go' decision points that are specific to projects. Third, projects are frequently, though not always managed as portfolios defined by complementary technology 'thrusts'. This was particularly apparent in the case of DARPA 'technology' programs, as contrasted with its 'systems' programs. Fourth, it did not seem that any projects are managed as portfolios with proportional investments. Managers had an intuitive sense of where funding ought to go, but made judgment calls to allocate resources, not calculated percentages. Fifth, only DARPA engaged the private sector by awarding prizes, and with only two coded excerpts, this did not appear as an important factor. Sixth, as with awarding prizes, using agency procurement for adoption was unique to DARPA, and did not come up as an important factor in the other two agencies.

Table 5: Number of excerpts per initial code category, for all codes and important codes.

Categories for Initial Codes	Excerpts (All Factors Coded)				Excerpts (Important Factors)			
	DARPA	ATP	ARPA-E	Total	DARPA	ATP	ARPA-E	Total
A. Managers' tenure at program	11	7	8	26	4	1	3	8
B. Managers' experience (work, study)	18	20	12	50	16	17	11	44
C. Managers' talent (versus researcher)	37	32	28	97	27	23	23	73
D. Managers' hands-on relationships	9	5	16	30	7	5	16	28
E. Nature of managers' role	26	12	16	54	22	12	12	46
F. How projects are chosen	49	66	63	178	49	64	62	175
G. Nature of the bureaucracy	48	23	25	96	43	23	22	88
H. Use of challenges or visions	26	20	14	60	26	20	14	60
I. Execution of projects (how and who)	29	55	36	120	29	52	31	112
J. Performance measures, milestones	16	32	51	99	16	26	56	98
K. Project duration, termination	15	28	16	59	10	18	9	37
L. Definition of project portfolios	29	24	22	75	24	13	15	52
M. Resource allocation across portfolios	11	7	7	25	11	7	7	25
N. Relationships with programs' allies	15	37	26	78	8	16	12	36
O. Relationships with programs' rivals	10	10	6	26	3	9	4	16
P. Rules Of Engagement (allies, rivals, others)	21	27	16	64	14	15	12	41
Q. How often results are applied	11	11	6	28	5	4	2	11
R. How results are applied	24	11	14	49	24	11	14	49
S. Engagement for application (private sector)	11	10	20	41	9	10	20	39
T. Engagement for application (public sector)	11	1	4	16	5	1	2	8
U. Chronological Steps (program, project)	47	68	80	195	43	68	72	183

Important factors are discussed below in the context of the heuristics that they support. Factors fell easily either into how things are done 'on the island', or how things are done 'on the bridge'. From this perspective, bridge-based heuristics are as prevalent as island-based heuristics. Further detail is provided below, but this means that inter-personal communication and interaction to 'connect the dots' is as significant an aspect of how the model agencies function as is their distinction from other groups to focus on problems of science and engineering. Bennis and Biederman seem to have intended their island-with-a-bridge metaphor to describe that an innovation group is isolated from, but linked to, the larger organization of which they are a part.

However, the finding here is that the linkages themselves constitute key activities for the innovative group. The island and the bridge are two sides of the same organization, hence the designation of the model as 'island+bridge' (island plus bridge). Science and engineering activities are decoupled from old contexts to allow for deviation from the norm; social and linguistic activities recouple them into new contexts in the process of value creation. As such, the island+bridge model confirms and provides additional detail to Bonvillian's connected model of innovation.

Detailed findings are discussed below in terms of heuristics nested under 'the island' or 'the bridge', as shown in Figure 2. Heuristics are designated as two- or three-word phrases and described in terms of the important factors that were clustered to form them, as summarized in Table 6. Heuristics are addressed in no particular order, other than to show how they are nested together. Heuristics are identified by text in ALL CAPS. Underlying factors are identified by text that is *italicized*. Where possible, descriptions rely on the words of participants and, in a few instances, agency publications. As such, quotes in this section are taken directly from source material.

Figures 3 and 4 are tree maps that provide additional detail on the degree of support for island and bridge, respectively, based on the frequency of underlying coded factors. In this diagrams, the area of the circle for each heuristic is proportional to the number of coded excerpts of the underlying factors. Figure 5, at the end of this section, displays a combined tree map of island+bridge. (Note that due to space restrictions on the printed page, neither figure can represents all 96 supporting factors themselves. Appendix 1 accounts for which factors align to heuristic clusters. A detailed tree map, including all important factors, accompanies this document as a poster.)

Table 6: Heuristics nested under **ISLAND** and **BRIDGE**, with summary descriptions.

ISLAND: Isolate activities from traditional contexts (both operations and basic research)
STAY SMALL: Limit the size and duration of projects and programs
DO IT ELSEWHERE: Fund activities externally, don't conduct them internally
DO IT FOR A TIME: Fund more activities temporarily, not fewer activities indefinitely
FEEL FREE: Avoid or cope with the bureaucracy of typical research organizations
DRIVE CHANGE: Critique the status quo, dream big, take action
SIMPLIFY MANAGEMENT: Flatten hierarchies, streamline processes, learn from business
NAVIGATE BUREAUCRACY: Accept necessary constraints, work smoothly within them
HERD NERDS: Focus the creative talent of scientists and engineers
HAVE TECHNICAL CHOPS: Have just enough knowledge of the science and engineering
LEAD ORCHESTRAS: Organize people and projects toward the big picture
BE BETTER: Embrace difference, attract excellence, stand out from the crowd
DESERVE TRUST: Empower managers who display creativity, fairness, and sound judgment
DO WHAT OTHERS WON'T: Take on challenges that most find too risky or even 'dumb'
SHOW THE WORLD: Demonstrate what is technically possible or cost-effective
BRIDGE: Connect activities to non-traditional contexts (both public and commercial applications)
MAKE IT MATTER: Direct activities to realize outcomes for society, not for science
TARGET STRATEGIC INTERESTS: Develop technologies/systems aimed at broad benefit
TARGET SPECIFIC USES: Tailor technologies/systems to public, commercial applications
MEET AND GREET: Socialize with stakeholders, engage in matters of business and policy
CREATE A COMMUNITY: Grow social networks to foster collaboration and generate ideas
GATHER SUPPORT: Forge relationships with research conductors and potential adversaries
KEEP CLEAR: Codify concrete results to negotiate terms and gauge success
SPECIFY VISIONS: Create tangible vision or challenge statements to direct work
SPECIFY METRICS: Define measurable results to determine when work is complete
LISTEN AND LEARN: Stay in a constant dialog with expertise and experience
SEEK INPUT: Rely on the knowledge and imaginations of potential partners
TRY AND ERR: Take diverse approaches, expect to fail, learn from failure

Figure 2: Heuristics on the **ISLAND** and the **BRIDGE** are organized as a taxonomy. Low-level heuristics (on the right) are more particular; each is defined by 3-12 underlying factors. High-level heuristics (in the middle) are more general; each is defined by 2-3 low-level heuristics. ISLAND and BRIDGE are most abstract: metaphors defined by 4 high-level heuristics each.

ISLAND

The island is a metaphor for those aspects of programs that isolate them from broader institutional contexts. The core concept of the island is that R&D is separated from operational concerns. This study suggests that, for transformative innovation, the island is also separated from other, more traditional research concerns. From the island, basic research is operational service that produces scientific knowledge, and is just as much a part of the mainland as other regularly

provided federal services. Yet the island is more than an organizational division with a separate budget and chain of command; it signals differentness and specialness in a range of factors spanning science, engineering, personal attitudes, and program administration. The first four heuristics apply to activities on the island: STAY SMALL, FEEL FREE, HERD NERDS, and BE BETTER.

ISLAND / **STAY SMALL**

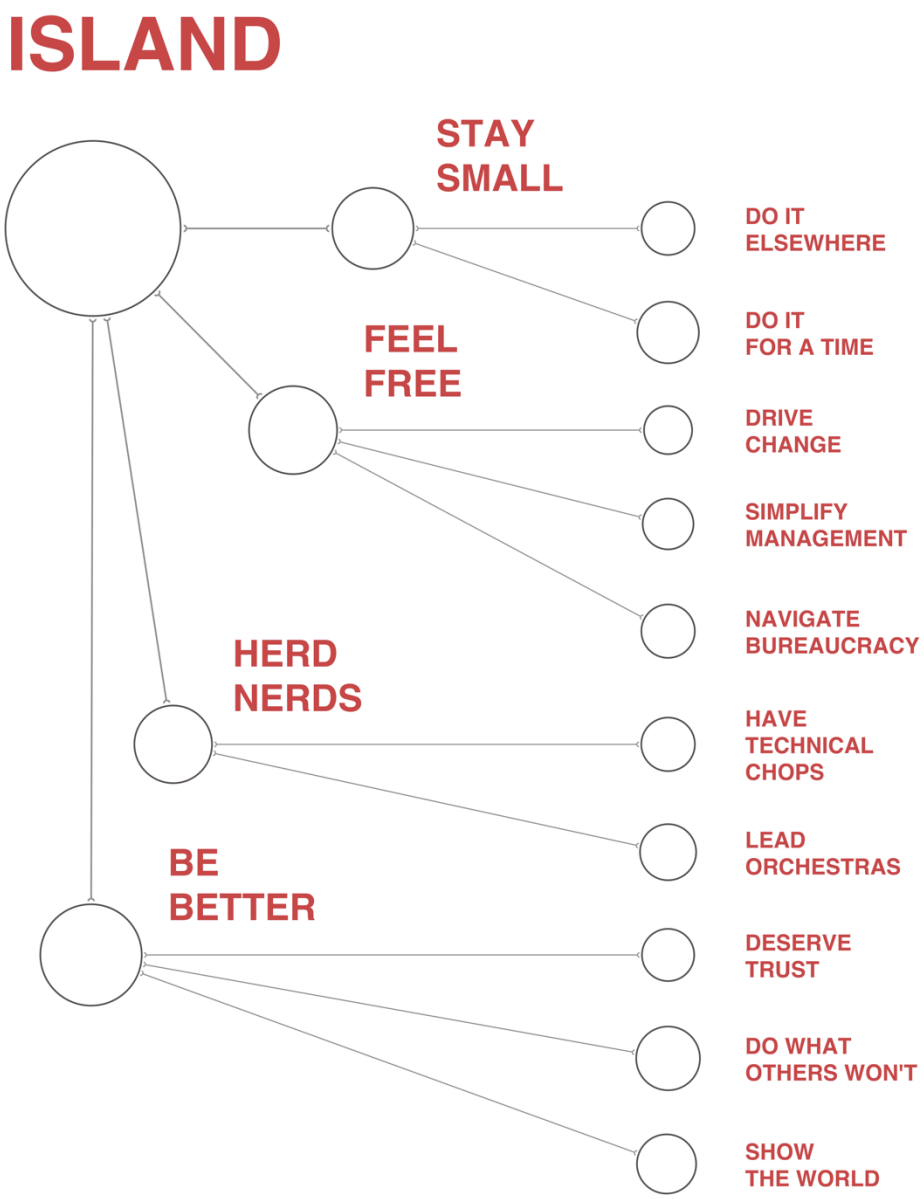
These agencies are lean and agile. Rather than running a few, big projects over long periods, they run hundreds of small projects for short periods. In contrast to typical mission agencies, these three agencies do not have their own bench researchers or laboratories, and projects last only for a few years at a time. Projects are executed externally (not internally) and program managers have tenure that is limited (not indefinite). At DARPA and ARPA-E, even the manager is expected to leave after a few years. There is little ability and no desire to establish internal fiefdoms to specialize in a particular brand of scientific research, nor to grow an empire with 'base' resources and vested interests of self-preservation.

ISLAND / STAY SMALL / **DO IT ELSEWHERE**

Projects at all three agencies are executed extramurally, either by *universities* or by *private industry*, including small, *creative companies* and joint ventures of two or more companies. ATP, in particular, encouraged the participation of *joint ventures*, and helped single applicants to find partners, as a means of encouraging 'broad benefit' (see TARGET STRATEGIC INTERESTS, below). None of the managers or materials indicated any in-house research capacity; all queries yielded some form of the same response: *not here*. One DARPA manager noted of his agency, 'It has no infrastructure to maintain, so it can pivot very easily. Government labs can't; they've invested in staff and equipment. That's very important. It doesn't tie you

down. If you see an opportunity to go on a tangent you can take it'. An important factor at both ATP and ARPA-E was the use of *cooperative agreements* as the vehicle for managing extramural research. These are time-bound agreements that codify expectations more specifically than a grant, but less specifically than a contract.

Figure 3: Heuristic taxonomy for **ISLAND** as a treemap. Circle area proportional to number of excerpts. Low-level heuristics sum the values of associated factors. High-level heuristics sum the values of those below. Values included for all important factors in I+B cases.



ISLAND / STAY SMALL / **DO IT FOR A TIME**

An important factor for two of the three cases, DARPA and ARPA-E, was *temporary tenure* for program managers. These two agencies hire program managers through the Intergovernmental Personnel Act (IPA), rather than as federal employees. Both agencies also make use of *Systems Engineering and Technical Assistance (SETA) contractors* to staff managers. One manager explicitly said, 'Regular turnover is an important factor'. Managers at DARPA and ARPA-E spent (or have spent thus far) *between 2 and 5 years* in their positions. The tenure of the majority of ATP managers was *five years or more*, though even one ATP manager noted temporary tenure as important.

Regarding the lifespan of projects, all managers and materials indicated that projects last *5 years or fewer*, with many ending within 3 or 4 years. Among all chronological steps in the life cycle of a project, *project termination* was discussed the most. Three important factors describe the conditions under which projects end. Projects may end when their particular *challenge is met successfully*, or they may simply end *when time runs out*, per the cooperative agreement. Alternatively, projects may be terminated at the discretion of the program manager before time runs out if he or she determines that the investigator is making *no progress and there is no 'Plan B'*. (For more on determining progress toward challenges, see SPECIFY METRICS, below.)

ISLAND / **FEEL FREE**

These agencies are free to be different. They are organizationally separate from departmental science or service divisions. They thrive on change – creating it and adapting to it. There is little satisfaction with the status quo, either of technological capability or of existing institutions. Program managers at DARPA, ARPA-E, and ATP are intent on using their programs to affect

transformation. Management at all levels keep programs moving forward, toward their visions, avoiding and surpassing obstacles. In general, there is impatience with the usual machinery of the federal government, and skepticism of undirected research. Program managers have experience in business (not just government) and talent that is managerial (not just scientific). These agencies are smaller, flatter, and less complex than other federal institutions and, where processes cannot be simplified further, managers work effectively to navigate within the government bureaucracy.

ISLAND / FEEL FREE / **DRIVE CHANGE**

Program managers at DARPA and ARPA-E tend to be *driven to change* the status quo. They often have 'fire in the belly' and a 'burning desire' to make an impact. They are described as *visionaries or dreamers*, often delivering on dreams, not just having them. One aspect of this drive toward change is the intent to understand the needs of adopters. At all three agencies, the program manager is viewed as an *entrepreneur*, which implies getting the funding to get things done, but also realizing that it is 'not just about the technology' (see MAKE IT MATTER, below).

At DARPA and ARPA-E (more so than ATP), the environment of getting projects funded and executed was also characterized as *brutal and fast-paced*. There is a great deal of competition for funding – among programs, as well as among projects with different approaches within programs – combined with an acceptance that tough decisions must be made quickly. This environment extends to relations with investigators who execute projects. One manager said that negotiating milestones is 'the most hectic part of the process'. Another at ATP noted that many of the oral reviews that were part of the selection process 'were real white-knuckle things'.

ISLAND / FEEL FREE / **SIMPLIFY MANAGEMENT**

One element of simplified management is a *flat hierarchy*; managers had regular interactions with the directors of their agencies, separated from them by only one level of authority, the office director. An aspect of this which cannot be understated, and which was particularly true of DARPA and ARPA-E, is that their directors reported to the secretaries of their respective departments, thus keeping their agencies independent from other divisions conducting R&D. A few participants noted that a flat hierarchy does not mean the absence of hierarchy. In fact, at the portfolio level, decisions about the allocation of resources typically happen *above the program manager*.

A particularly robust factor (i.e. on that passed all four tests of importance) is that bureaucratic processes are *streamlined*. From the perspective of those at the model agencies sound and efficient management can be an enabler of breakthrough results, not an obstacle to it. Many of the managers interviewed had *studied management* in addition to (or instead of) science and technology; some have master's degrees in business administration, one has a degree in engineering administration, and one is a certified public accountant. Another element of management efficiency is the use of *milestones and metrics* to track the progress of investigators (see also SPECIFIC METRICS, below).

ISLAND / FEEL FREE / **NAVIGATE BUREACRACY**

While processes are more streamlined at these agencies than others, they must still comply with federal rules governing the administration of their programs. Managers at ATP, in particular, recognized that the bureaucracy within which they found themselves was *not particularly flexible*. Functioning within their agencies, program managers rely heavily on each other and on the people in their offices. An important characteristic of successfully navigating the

bureaucracy is *teamwork*. One interviewee said that she had a close, hands-on relationships with 'other people at ARPA-E. They understood what you are working on. They were in the trenches with you. It's a 'band of brothers'. Camaraderie and respect also extended strongly to the agencies' *administrative staff*, including contracting officers and lawyers.

With respect to those applying for and executing projects, the manager often serves the role of *problem solver or facilitator*. To help their investigators navigate the process, program managers can draw upon their experience *working in government* and *working in industry*. Related to this is the manager's talent for *administration and bureaucracy*, ensuring that the investigators can focus on what they are best at. As one participant from ATP put it, 'you don't want a Ph.D. physicist spending 25 hours a week figuring out what forms to fill out so that they get their money'.

ISLAND / **HERD NERDS**

These agencies are, as one might expect, steeped in the culture of science. Managers, senior leadership, and researchers all share a passion for science and engineering, and have a great deal of experience in conducting research. These agencies draw upon the curiosity and experimentalism of those in scientific fields. They do not, however, focus on the science, per se. Managers take a transdisciplinary approach: attacking problems on multiple fronts. Program managers are entrepreneurs and translators (not subject matter experts). They see the big picture. They have the technical 'chops' (i.e. technical skills) to understand and guide the work of the specialists, as well as a systems perspective that allows them to orchestrate how individual experts work together and how all of the pieces of their programs function as a whole. Projects are often managed as portfolios defined by complementary technology 'thrusts'.

ISLAND / HERD NERDS / **HAVE TECHNICAL CHOPS**

Programs at these agencies are demarcated by technological application and build upon scientific discovery. Not surprisingly, an important factor of project choice is the *technical merit* of proposals, and an important factor of managerial talent is *technical direction*. Technical direction seems to be founded upon *technical expertise*. Most managers have *studied a STEM field* (science, technology, engineering, and mathematics) and possess a graduate degree. Many have also *worked in academia*. They have the 'scientific chops' and 'know what it's like in the trenches'. Managers have a *hands-on relationship with researchers*, and help them isolate and solve problems, and ask critical questions. One participant noted that having baseline knowledge of the science or technology at hand was necessary (though not sufficient) for running a program.

ISLAND / HERD NERDS / **LEAD ORCHESTRAS**

While managers have the chops, they are 'still up 30000 feet' and 'don't get into the nitty-gritty' in their role as manager. An important factor of managerial talent is *systems thinking*. Portfolios at DARPA are divided in terms of *systems versus single technologies*. All the agencies aim for systems innovation by developing portfolios with *complementary technological thrusts*. Managers should see 'how it all fits together' and 'how to get from point A to point B'. Managers need to have a breadth of expertise, including, but not limited to, scientific expertise. In the words of one manager, 'If you spent your entire life getting three PhDs, that wasn't enough. Most program managers had greater breadth of experience'. While 'not as smart' as those doing the research, managers are likely to be generalists and 'broad thinkers' with 'cross disciplinary skill sets' and the ability to 'step back' and 'hear other people out, accept their input'.

The manager must also think about the constellation of individuals working with them and how they fit into a larger picture. A popular metaphor among interviewees for the role of the manager

was *orchestra leader or a movie producer*: someone who finds talented people and combines their abilities. Source material indicated that managers often assume the role of *coach or mentor* to investigators, as well as *translator* between the fields of science and application. A manager at ATP compared himself to an electrical 'transformer' for converting voltages. They are able to *motivate creatives*, transferring their own sense of purpose and urgency to the investigators. One participant said that, for talented scientists and engineers, the manager needs to 'provide the lubricant and the vision'.

ISLAND / **BE BETTER**

These agencies are exceptional. They embrace difference, attract excellence, and are not for the modest or meek. The programs and the people who manage and execute them have special abilities to do very unique things in order to tackle very significant problems. Managers are often the cream of the crop in their respective fields, and often possess both technical and managerial credentials. Program managers have power in choosing projects (relative to superiors) and, sometimes, power to hire, fire, and contract (relative to other agencies). Projects are managed as portfolios defined by risk. These agencies distinguish themselves by empowering managers that deserve trust, choosing and addressing problems that others will not, and showing others that solutions exist. While not captured explicitly in the data from source materials, the energy at the offices of these three agencies and the confidence of people who work at them is hard to deny.

ISLAND / BE BETTER / **DESERVE TRUST**

Managers in all cases are given a great deal of authority and take on many responsibilities to realize their vision of the program. For DARPA and ARPA-E, the data revealed a very strong signal for *empowered managers*, that is, managers who have a great deal of 'control and

accountability' and who 'drive the entire process' of a program. At both agencies, program conception begins with a manager's *pitch to the director* of his or her idea for what the program will accomplish and what approaches will be taken (see SIMPLIFY MANAGEMENT, above). All three agencies use selection panels to judge proposals of applicants, but at DARPA and ARPA-E, the manager has the final call on which proposals are funded. 'You work for the federal government, not Google, which means that you have to be fair,' said one manager. 'That doesn't mean just following the rules'.

An important factor at all three agencies was that there was *no micromanagement* and, in cases where there was, participants attributed to it poorer results (e.g. several participants cited the management style of DARPA Director Anthony Tether). Although many portfolio allocation decisions are made by the director, the department, or Congress, managers in all cases had a good deal of discretion at the project level. Across different approaches, *allocation was often a judgment call*. Managers 'didn't set up a percentage' for investments in different avenues. A DARPA manager enthused, 'Excitement is the criterion. That's a whacko idea or that's a great idea, let's do it'.

ISLAND / BE BETTER / **DO WHAT OTHERS WON'T**

A frequently discussed step in the project chronology was *selection with reviewers*, which had a particularly strong signal at ATP. Both ATP and ARPA-E define criteria on both the technical and the business sides to carefully choose which applicants' proposals to fund. At all agencies, though, a key criterion for choosing projects was that *no one else* would do it, and that others would think it was 'a dumb idea'. An ATP manager commented that, often, there are powerful ideas that private interests 'don't have the internal incentive to do, but the benefits to the nation would be really substantial if they are successful'. One ARPA-E manager choose things that

were technically possible, yes, but 'bat-shit crazy in other ways. One miracle is too few, but 3 is too many; we're looking for a 2 miracle program'.

The model agencies are aiming for the 'white space' that no other public, private, or academic organization is currently filling, or considering filling. The general attitude among interviewees is that choosing projects in this way *may not always result in application, but it's worth it* when it does. An ancillary benefit of this selectivity is that agencies are often able to use the *halo effect, i.e.* innovative reputations, useful in forming partnerships and leveraging resources.

ISLAND / BE BETTER / **SHOW THE WORLD**

These agencies are leaders. They have missions to *demonstrate, not refine* technologies, which means that some attempts will succeed while some, if not most, will fail. Underlying the choice of projects that no-one else will do is the notion of risk. *Choosing programs that are high risk* is a very robust factor, and the factor with the largest total number of coded excerpts. DARPA, specifically, has an approach to technology transfer that centers on *showing the world applications are possible*. ATP and ARPA-E, however, start technology transfer by *showing the world applications are cost effective*, not just possible. 'The model is to take it to the point where technical barriers that were impeding its ability to be commercialized have been resolved,' said an ATP manager. 'That was our purpose'.

Those at DARPA and ARPA-E see their agencies role as *throwing rocks* at their home departments from outside. They are largely independent of other agencies and focus on creating new capabilities, rather than responding to service requirements. They exist to change perceptions, often by illustrating the distance between where capabilities are now and where they could be in the not-too distant future. As one manager described a tactic that he has used,

‘Sometimes, if you take one little step, it doesn’t work; but if you take six giant steps, it’s hard to ignore’.

BRIDGE

The bridge is a metaphor for those aspects of programs that connect them to broader institutional contexts. The core concept of the bridge is that R&D is integrated with operational concerns. The model agencies are not merely being responsive to the needs expressed by stakeholders; indeed, they go in directions that others would not. Research is use-inspired (Stokes 1997) and it is disruptive (Christensen 2003). The bridge is necessarily a social space, where knowledge is exchanged between humans through dialog. Opportunities are created for frequent and precise communication among interested parties. The second four heuristics apply to activities on the bridge: MAKE IT MATTER, MEET AND GREET, KEEP CLEAR, and LISTEN AND LEARN.

BRIDGE / MAKE IT MATTER

These agencies are concerned with outcomes for society, not science. Use is the inspiration and the destination. One participant summed it up thusly: ‘We know this stuff works, but does it matter? If it doesn’t scale, it doesn’t matter’. Stakeholders are not those in research laboratories. ARPA-E and ATP consider their stakeholders to be in industry, and DARPA’s key stakeholders are in the military service branches. Benefits are intended to accrue more broadly than to one particular company or agency. Programs are created to address particular strategic interests, to improve the economy or national security. Projects are created with specific applications in mind, and these agencies monitor progress not only in terms of technological capability, but in terms of commercialization and technology transfer as well. Cost-effective adoption is a criterion for success. Project proposals are required to show a plausible pathway to implementation and often

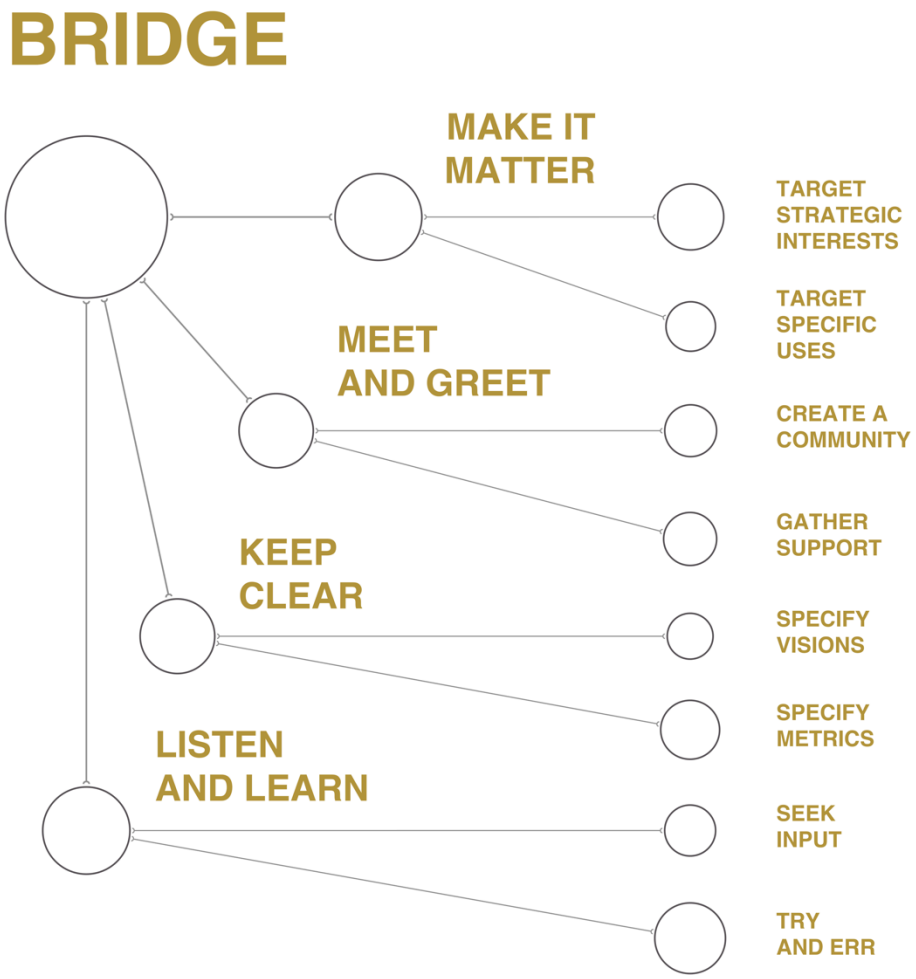
result in an application (though not always the one envisioned). Programs find first adopters and create markets.

BRIDGE / MAKE IT MATTER / **TARGET STRATEGIC INTERESTS**

This sub-cluster contains three very robust factors with some of the strongest signals of any in the study. As noted above, program portfolios are defined with particular *technological applications and industries* in mind. This was by far the most common way of describing portfolios across all three agencies. In choosing among projects to fund, managers and their selection committees look for applications that show that the proposed research has a *plausible path* to implementation. At ATP and ARPA-E this meant focusing on commercial application. During project selection, all three agencies (ATP and ARPA-E in particular) also look for applicants to make a credible case that the research will have *broad benefits***,** beyond those to the investigators and their institutions.

These agencies do not support science for the sake of understanding, but, rather, *build on basic research* to realize the potential of discoveries already made. At DARPA, strategic, national security interests are always in mind. At ATP, managers are consistently concerned with delivering economic benefits beyond what is appropriable to individual companies, and encouraged applications by joint ventures to this end. At ARPA-E, managers focused on bringing new energy technologies 'to scale' in order to create 'new cost curves'. They understood the value of their research as determined by the marketplace. One remarked, 'That's what all of this stuff is about: making something that everyone cares about'.

Figure 4: Heuristic taxonomy for **BRIDGE** as a treemap. Circle area proportional to number of excerpts. Low-level heuristics sum the values of associated factors. High-level heuristics sum the values of those below. Values included for all important factors in I+B cases.



BRIDGE / MAKE IT MATTER / **TARGET SPECIFIC USES**

The final step of the project life cycle is to *transfer technology to adopters*. In order to realize broad benefits, the technologies demonstrated by these agencies must be deployed. While none of the agencies conduct deployment activities themselves (many interviewees were clear that their agency's responsibility ends with demonstration), a significant part of their mission is to facilitate it. The modus operandi for DARPA is to get technologies adopted though *agreements with operations* and service divisions within the Department of Defense. Some of these agreements are formal, such as memoranda of understanding with the Army. Others are informal. One

DAPRA manager noted, 'We have people from the services embedded with us. Lots of dialog with them'. Another recounted how he 'was talking to generals [and] brokering arrangements'.

For ATP and ARPA-E, this means putting a premium on managerial *talent for commercialization* of new technologies, and a dedication to *manage the business side* of projects from beginning to end. ARPA-E requires both a 'tech-to-market plan' and an 'intellectual property management plan' as part of proposals. The agency has a dedicated *commercialization team* to guide award recipients as their projects evolve. In a similar vein, programs at ATP were dual-headed: led simultaneously by a technical manager and business manager. At ATP, award recipients were asked for post-project reports two, four, and six years after completion 'to document actual progress in commercializing the technology and impacts of the innovation to the company and society'.

BRIDGE / **MEET AND GREET**

An essential part of a program manager's job is to socialize, get out of the office and engage in matters of business and politics. Managers create communities to realize their vision of the program by connecting with peers, experts and other fellow innovators. They also engage and gather support from those who have the power to help or hinder the program. Most of the managers interviewed possessed a combination of charisma and straightforwardness that, this author imagines, must be very effective when they network with potential adopters in industry and in other government agencies, or when they confront skeptics in their own departments and in Congress. The private sector is engaged by participation in industry consortia, funding creative companies, teams dedicated to commercialization, and leveraging a reputation for innovation. The public sector is engaged by making agreements with operational lines in the department (or others) for adoption, and connecting the results of work to the stage-gate process of partners.

BRIDGE / MEET AND GREET / **CREATE COMMUNITY**

While staying small and not establishing empires within the federal bureaucracy, these agencies work hard to *build communities* externally. They grow informal social networks to foster collaboration with operations and industry, as well as to cross-pollinate ideas among those investigators working on different projects. Managers make introductions, bring people together through meetings, seminars, and workshops. 'We'd suggest collaboration' remarked a manager from ARPA-E. 'Being part of the community and bringing people together was really one of the more rewarding parts of the job'. At ATP, 86% of the projects involved collaborations, and 69% of the research performers stated that ATP was responsible 'to a large extent' for bringing about the collaboration.

These agencies do not attempt to artificially stimulate market demand for their technologies – other than by demonstrating their feasibility, that is. However, they do actively try to *find adopters anywhere* they can. They seek both *government adopters* and *commercial adopters*. One notable means to find them is to *attend industry consortia* within the technological domain of the program. A strong signal from DARPA (but not at all from the other two agencies) was the importance of *technology transfer through people*, that is, human beings are themselves a medium for transmitting tacit knowledge. 'Good ideas travel by osmosis into industry… though grad students, etc.', said one manager. Another said, 'Tech transfer is really a body-contact sport. Especially process technology'.

BRIDGE / MEET AND GREET / **GATHER SUPPORT**

Because these agencies have no permanent research staff or infrastructure, they are dependent on the capabilities of partners who will be conducting the work of the projects. Managers must, therefore, engage early and often with *partners in industry* and with *partners in universities*.

They also have *partners in their home departments' science divisions* – not necessarily to conduct research activities, but to consult on the science, technologies, applications, as well as to define common areas of effort, while minimizing programmatic overlap. At ATP, laboratory staff at NIST laboratories served on selection teams, and helped review hundreds of proposals every year. For this service, laboratories were reimbursed, freeing up money for them to do other research.

In terms of funding, however, *home department science divisions were competitors*. DARPA was created to be in 'contention and competition with the existing defense R&D establishment'. All of the model agencies seek to do what others will not, but do face opposition from a federal laboratory system that – in the views of many interviewees – sees the total R&D portfolio as a zero-sum game. An ARPA-E manager stated plainly, 'It was a turf war… They thought we were taking their money'. And further, 'There are people that do not want ARPA-E to be successful because it changes the expectations on everyone else'. Paraphrasing the perspective of NIST laboratory directors, an ATP manager said, 'you are corrupting the pure science we are trying to do'.

In dealing with intra-departmental rivalries and other sensitive (often existential) issues, the *agency director is a strong advocate*. Participants attributed the success of their programs to the talent of their directors to build support at high levels of government. One was described as 'unstoppable,' another was a 'genius' at dealing with Congress. Often, *Congress was an advocate* of the agencies; ATP and ARPA-E were created organically though legislation. In the case of ATP, *Congress was a political critic* – specifically, conservative Republicans with free-market ideologies viewed the agency's work as 'picking winners'. One tactic that ATP used to combat this criticism was to *evaluate the economic impacts* of its projects as part of normal practice. ATP housed an economic evaluation office that produced a great deal of evidence of the

value of their programs, yet this tactic did not save ATP from its ultimate, politically motivated demise.

BRIDGE / **KEEP CLEAR**

The activities of these agencies are applied research or development, not basic research. They are, to borrow Snow's distinction, about exploitation rather than exploration. These agencies work within boundaries that they have defined for themselves. Projects have specific challenges or visions that drive them and are managed with specific metrics and 'go or no-go' decision points (not 'stage gates'). Crystal clear communication of concrete results – both imagined and actual – is how success can be gauged and, hence, the foundation upon which proposals can be deemed credible, project approaches can be chosen (and changed), and portfolios of many diverse, short-lived projects can be managed. Visions and metrics of success do not have to be attainable (in fact, they ought to be risky), but they do have to be tangible and specific.

BRIDGE / KEEP CLEAR / **SPECIFY VISIONS**

There are *specific visions*, or as one participant put it 'big hairy challenges,' that define success at both the program-level and the project-level. These can be visions of *specific technologies* or *specific applications* of these technologies. As noted in DRIVE CHANGE, above, managers at the model agencies tend to be *visionaries and dreamers*. At DARPA and ARPA-E, program-level visions are developed by the program manager, drawing upon a great deal of input from external partners in industry and academia. At all agencies, visions at the project level are suggested by investigators in their proposals, and then negotiated with the manager.

Visions are outcome-oriented, focusing on problems, not solutions. ARPA-E 'funds technology-focused applied research to create real-world solutions to important problems in energy creation,

distribution and use and, as such, will not support basic research'. ATP projects required 'concrete technical goals [that are] very clearly spelled out'. DARPA requires 'a clear statement of the programs' objective and discussion of who will benefit and why it matters'. Examples include 'an unmanned aircraft that can loiter for months or years' and 'a fully functional prosthetic limb directly controlled by the brain'.

BRIDGE / KEEP CLEAR / **SPECIFY METRICS**

All three agencies use *specific measures* to inform management. 'Otherwise,' in the words of one manager, 'how do you know when you're done'? Metrics should also be malleable (see TRY AND ERR, below). One ARPA-E manager contrasted ARPA-E with a Department of Energy service line where 'it's impossible to change milestones, so you write them to be meaningless'. For a DARPA manager, 'Gut instinct isn't good enough; I needed hard facts that pointed to success'. And, pointing out the utility of measures for defining problems, while leaving open solution, an ATP manager said, 'They don't care whether it's made out of magnetic materials or milk… as long as it does what they advertise with the reliability that they need'.

Many of these are, as one might suspect, *technical measures*. ATP and ARPA-E also used *commercialization measures*, such as number of licenses, license revenue, and company growth. Another commonly cited measure at these two agencies was *leveraged funds*, used to indicate the confidence of the external community and the degree to which the agency 'catalyzes new interest and investment'. It is worth noting that measures not mentioned (except in a very few instances) were those more typically used at research institutions: number of peer-reviewed publications and patents.

Programs do not use standardized stage gates, such as Technical Readiness Levels used at NASA, to track progress. A few of the managers suggested that this type of performance management is

antithetical to the nature of the research they oversee. They do, however, develop (with the researchers) a few key milestones that are tailored to individual projects. The milestones are often quantified using technical measures, and are used to define *go or no-go decision points* during the life of a project. Not reaching a milestone could prompt a manager to redirect or terminate a project.

BRIDGE / **LISTEN AND LEARN**

These agencies are in a constant dialog with expertise and experience. They listen to the community for direction on framing problems and solving them. Programs are set up to learn from failure. This heuristic is intimately connected with KEEP CLEAR, above. Program managers actively engage with allies, rivals, and others in the innovation ecosystem. Projects have specific challenges or visions that are co-developed with stakeholders. The clarity provided by visions and metrics is the basis upon which effective communication can occur. Frequent and precise communication, in turn, is the mechanism by which projects are evaluated and improved over time.

BRIDGE / LISTEN AND LEARN / **SEEK INPUT**

The model agencies have no permanent research staff, infrastructure, and, at the beginning of programs, at least, they do not yet have fully formed ideas. These agencies rely on the imaginations of potential partners. One of the first steps within programs is to seek *concepts from the community*. Programs will call for concept papers or hold workshops to identify challenges that could define the work of projects. The process described by one ATP manager was typical: 'We solicited papers from industry on what they thought were important problems, and technical problems out there needing to be solved'.

Of course, the following steps also centered on external input: the *announcement to the community* to submit proposals, and the *receipt of proposals*. At DARPA these are called 'Broad Area Announcements' (BAAs). At ARPA-E they are 'Funding Opportunity Announcements' (FOAs). ATP solicited proposals for both 'focused programs' with defined technology areas, as well as 'open competitions,' in which no specific technology areas were defined. As noted above, programs would often seek external help in reviewing the proposals received. In addition to the other criteria of program choice already discussed, managers sought proposals that *made a credible case*: they told a story effectively, and showed that the funding was enough to make a difference. They could not seem 'incoherent' or 'stapled together'.

BRIDGE / LISTEN AND LEARN / **TRY AND ERR**

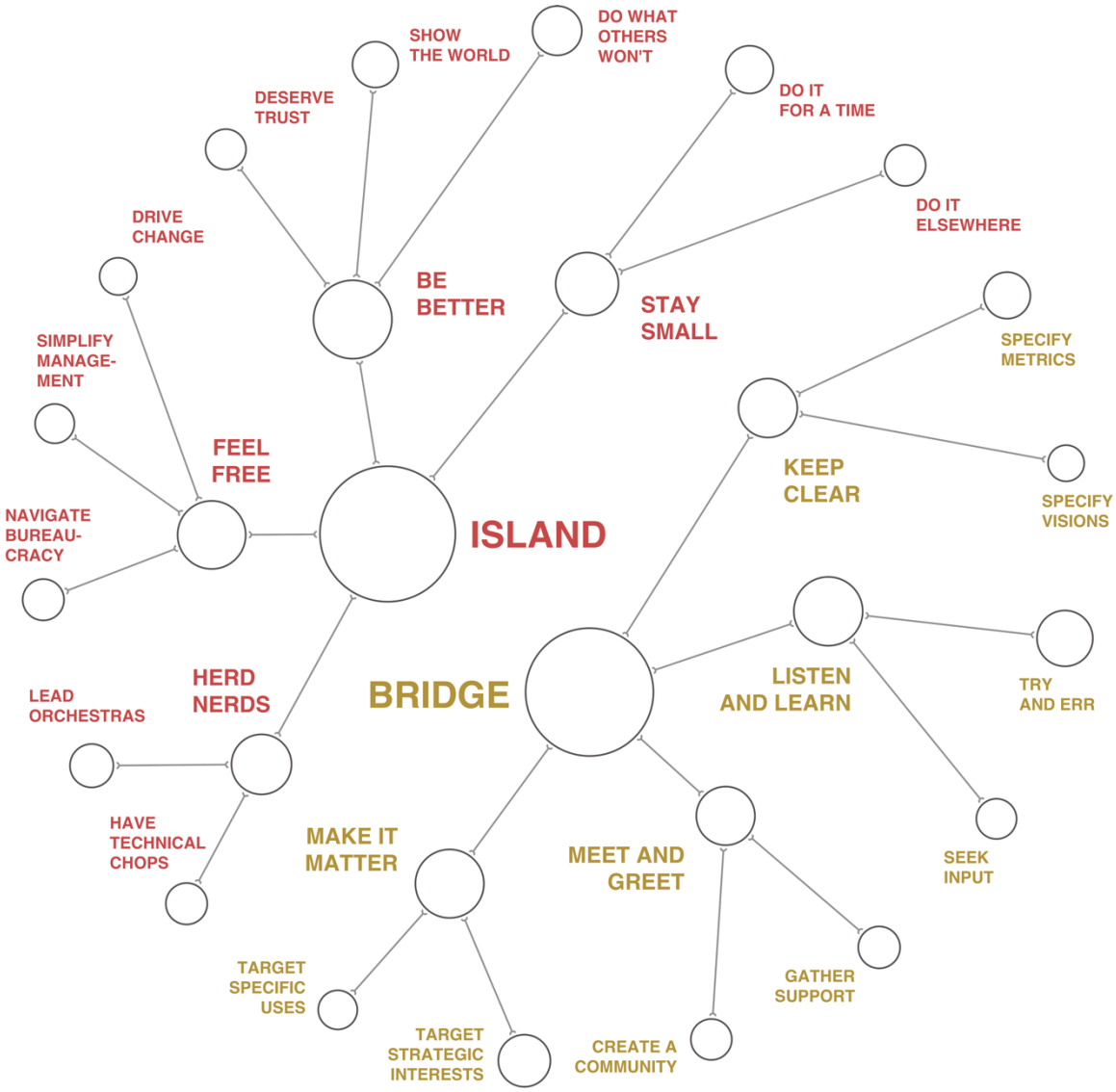
At these agencies failure is expected. It is a necessary consequence of choosing to do things that are high risk and that no one else will do. However, what is most important at these agencies is to *learn from failure*. Not knowing which avenues of research will pan out, the manager will often choose projects that take multiple, *diverse approaches* to solving a problem, in order to spread risk across the entire program portfolio. At the beginning of each project, the visioning process is itself iterative. In defining a challenge that a project will address, *the program manager scopes it, but the performer proposes it*. Manager and researcher work together to frame the problem. One manager would ask, 'What is a reasonable goal? What is unreasonable? Let's find something in between'.

The learning process of programs depends on *active management*, that is, the manager has frequent, face-to-face interaction with researchers to discuss project progress. ATP, for example, would conduct oral defenses of proposals prior to award. Through *negotiation with the performer*, measures and milestones are set up front, and often reset over the life of the project as

results come in. Negotiation (and renegotiation) of the formal agreement was particularly important at ARPA-E. As one manager expressed, 'There's nothing like a conversation'.

Active management also entails *regular reviews with performers* as projects progress. For these agencies, this means requiring reports, conducting on-site interviews, and 'lots of travel'. At ATP, 'for a three-year project, you might visit three times. Four or five times if there are problems'. One very active DARPA manager would 'meet with them a couple times a month'. At ARPA-E, 'Not only do our teams have quarterly milestones, but also quarterly reviews. We try to stay as close to technical work as possible. We hear time and again that that is really appreciated by technical community'.

Figure 5: Heuristic taxonomy for **ISLAND** and **BRIDGE** as a treemap. Circle area proportional to number of excerpts. Low-level heuristics sum the values of associated factors. High-level heuristics sum the values of those below. Values included for all important factors in I+B cases.



Discussion

In the sections below, I develop an understanding of how heuristics appear to interrelate by focusing on two cross-model constellations of heuristics that emerged as interesting and relatively unexpected. I then address the contribution of the island+bridge model to the literature by comparing it to another general model for organizing transformative innovation, recently proposed by former DARPA leadership. I close out by discussing how the model might be applied, as well as suggesting avenues for future research.

Constellations

The first notable constellation of heuristics has a center of mass ON THE ISLAND, and concerns the ability to be **deliberately but delicately disruptive**: transforming systems, while still working within a system of controls. Its core is the FEEL FREE heuristic, which combines the motivation to DRIVE CHANGE with SIMPLIFY MANAGEMENT and NAVIGATE BUREAUCRACY. The ability to appreciate and drive systematic change with creative researchers links DRIVE CHANGE to HERD NERDS / LEAD ORCHESTRAS. Factors such as empowered managers, brutal and fast program environments, flat hierarchies, and no micromanagement link SIMPLIFY MANAGEMENT to BE BETTER / DESERVE TRUST. Similarly, the SIMPLIFY MANAGEMENT is connected to STAY SMALL / DO IT FOR A TIME, via the extensive use (at DARPA and ARPA-E, anyway) of SETA contract staff.

Note also that there is no heuristic in the model entitled ‘be organizationally independent,’ or something similar. FEEL FREE emphasizes the feeling of freedom to act within constraints that come in many different forms. It is composed of SIMPLIFY MANAGEMENT and NAVIGATE BUREAUCRACY, rather than ‘avert management’ or ‘eliminate bureaucracy’. It is important for managers to have administrative talents, to take on the role of a problem-solver and facilitator,

and to emphasize team-work within the bureaucracy. This was notably the case at ATP, where the bureaucracy was not particularly flexible. In this respect, and with respect to the tenure of program managers, DARPA and ARPA-E appear to be less bound to the norms of the federal programs than ATP. Nevertheless, what seems to be present at all three agencies is an ability to carefully push certain limits while simultaneously working within others. Thus, if a separate chain of command is not feasible for the aspiring transformative agency, there may be other ways to exert freedom.

The second constellation has its center of mass ON THE BRIDGE, and concerns the importance of **frank and frequent face time**: regular, detailed conversations between the managing team and performing team as projects progress, focusing on direction and performance, and resulting in the co-creation of knowledge about technology evolution. The lynchpin of this constellation is an unexpected, emergent factor, namely, the negotiation and renegotiation of project visions and metrics. TRY AND ERR ties this factor to the general approach of active project management, and regular reviews with performers. There are obvious connections to the heuristics to SPECIFY VISIONS, SPECIFY METRICS, and generally KEEP CLEAR in programmatic communication. Specific measures and milestones are also intimately connected to STAY SMALL / DO IT FOR A TIME, by defining when projects end. The capacity to MEET AND GREET / CREATE A COMMUNITY also appears to combine the clear communication of intent and approach with the inherently social aspect of active management.

At the heart of every project, at all three agencies is in-person, back and forth discussions across organizational boundaries to articulate problems, consider alternative solutions, agree upon the meaning of success, and to share the understanding of whether or not success was reached. These agencies are able to blend the reductionist perspective and quantitative style of technical talent with the holistic perspective and qualitative style of managerial talent. Routine, in-person

conversations about strategy, execution, and evaluation seems to allow diverse specialists to share semantics and situational awareness. (For more on how we might improve this communication process, see below the discussion on future research.)

Contributions

As noted above, the island+bridge model specified here is largely a confirmation that the factors identified by Bonvillian and Van Atta (Bonvillian & Van Atta 2011) are present at DARPA, ARPA-E, and ATP. This study builds, therefore, upon a lineage of prior, separate studies of the DARPA-model by Ree, Van Atta, & Deitchman (1990), Fong (2001), Van Atta, Lippitz, Lupo, Mahoney, & Nunn (2003), Bonvillian (2006; 2009), and Bonvillian & Van Atta (2011). It contributes also to the self-reflective studies that all three model agencies have conducted of themselves, as well as the individual accounts made in presentations by program managers (Advanced Technology Program, Economic Assessment Office 2007; Kerwin & Campbell 2006; Ruegg & Feller 2003; Boysen & Gur 2012; Haynes 2013). Further, it offers an administrative perspective on how any public R&D agency might operate in Pasteur's Quadrant (Stokes 1997).

As a model of heuristics, island+bridge also contributes to the growing literature on the Science of Science Policy (SoSP), intended to improve the management of the national science and technology enterprise (Marburger 2011). It begins to respond to the question posed by Koizumi, namely, how does one measure the 'transformativeness' of a research proposal or portfolio? (Koizumi 2011, p. 298). It more directly responds to Sarewitz's suggestion that SoSP investigations be directed to 'the identification, classification, and interrelations among values that motivate research within a complex institutional ecology, as a foundation for assessing a science institution's potential to achieve desired social outcomes' (Sarewitz 2011, p. 346).

The island+bridge model also encompasses all of elements of the 'Special Forces' model of innovation, recently advanced by former DARPA Director Regina Dugan and former Deputy Director Kaigham Gabriel. They believe that the DARPA's organizational model can be successfully applied by agencies in the public sector and by companies in the private sector. Dugan and Gabriel suggest, as this study assumes, that past attempts have failed 'because the critical and mutually reinforcing elements of the DARPA model were not understood' (Dugan & Gabriel 2013). To create transformative innovation, they recommend that firms emphasize the core of the island+bridge model, organizational independence and ambitious, use-inspired goals:

> The work in Pasteur's Quadrant doesn't exist on road maps. It results in discoveries that upset the current trajectory and can destroy an existing business. Expecting the research organization executing against the road map to simultaneously deliver breakthrough innovations that challenge the road map is unrealistic. Instead, companies should create a small, dedicated independent organization to work in Pasteur's Quadrant (Dugan & Gabriel 2013).

The heuristics derived in this study confirm the three core components of the Special Forces model: independence, ambitious goals, and temporary projects. From the perspective of the island+bridge model, independence is the essence of being ON THE ISLAND, and is realized through the heuristics to FEEL FREE and SIMPLIFY MANAGEMENT. Dugan and Gabriel's full description of the ambitious goals component of their model incorporates aspects of several other heuristic clusters: MAKE IT MATTER, KEEP CLEAR, TRY AND ERR, and DO WHAT OTHERS WON'T. The third component of the Special Forces model is also confirmed in this research: temporary projects that 'get great people to tackle great problems with other great people'. This aligns with heuristics under STAY SMALL and HERD NERDS, but also incorporates the DESERVE TRUST, as well as DRIVE CHANGE.

Dugan and Gabriel do not address, however, some of the more inter-personal, inter-organizational elements that this study found ON THE BRIDGE. They do not explicitly focus on the capacity to

GATHER SUPPORT or CREATE A COMMUNITY (both under MEET AND GREET), nor on the capacity to SEEK INPUT from external experts and incorporate it into programs. While Dugan and Gabriel note that clear goals and measures are critical, and draw the connection to iterative planning and evaluation, they do not underscore that setting and achieving ambitious goals is a social phenomenon that requires (as discussed above) frank, frequent face time. The authors may feel that the human touch is too obvious to mention, particularly to a business management audience, but to a federal-scientific audience, bridge-based activities warrant explicit mention. Any model of transformative innovation should start to address the mechanisms of organizational learning and group cognition, as well as the sharing of knowledge and understanding among scientists, engineers, and stakeholders.

Applications

Congress has set policy for science agencies to create strategies for transformative research in the 2007 America COMPETES Act (US Congress 2007), as well as in the 2010 reauthorization (US Congress 2011). By repeating the methods used here, we could use the island+bridge model as a qualitative baseline against which to conduct a diagnostic assessment of how well positioned other agencies are to meet the intent of the COMPETES Act. Furthermore, by detailing the heuristics at other agencies, and comparing them to those of the three agencies studied here, we may set up the evaluation of such policy efforts by specifying inputs that could be statistically correlated with outputs and outcomes through causal analysis.

It is important to note that the model presented here is still in an early stage of development and has limitations. It is a *description* of the three agencies studied; it may enable *prediction* of outcomes through more precise hypotheses; but it is by no means a *prescription* for design of an agency. Many of the heuristics identified in this research may not be appropriate in other organizational contexts and, perhaps more importantly, it may not even be possible to deliberately

create an institutional culture that incorporates any or all of them. As one ARPA-E manager said of his agency, 'it was like catching lightning in a bottle'.

The model is not, therefore, intended to be a recipe for the 'secret sauce' of the ARPA-model. It is, however – to extend that culinary metaphor – intended to express the palate of flavors for the three sauces sampled. This palate offers a more nuanced framework for comparing the organizational factors at play at R&D agencies aiming for transformative innovation. It is the hope of this author that by gaining an appreciation for what these flavors are, learning about their subtleties and how they combine, we might rely less on recipes that require ingredients we do not have, and instead learn to cook better with whatever we have in the fridge.

Not every agency, to be frank, can enjoy the political advantages of DARPA, nor would every agency necessarily face the level of political resistance faced by ATP. Sapolsky and Taylor (2011) have shown that presidential advocacy can help clear the way for federal innovation efforts that face systematic resistance to change. Bonvillian asserts that agencies created for innovation must 'ensure in program design that there will be a noteworthy political interest constituency that will support the new agency before the executive and legislative branches on a continuing basis' (2011, p. 312). This includes linking the efforts of the agency to a politically recognized mission area (such as national defense in the case of DARPA, or climate and energy security in the case of ARPA-E) and leveraging the launch of the agency as a high-visibility event. It also includes carefully aligning with the gaps and comparative advantages of potential intra-agency rivals, who will likely have established interests in early stage research, and of potential rivals in industry who will likely have established interests in late-stage development.

Agencies that seek to organize for more transformative results should, therefore, carefully survey their managerial, organizational, and political landscapes, develop options in all of these arenas,

and then proceed in the way that best suits their situation. If there is one thing we can learn from the three agencies studies here, it is that there are no recipes for the things that are worth doing; you have to make up the rules as you go along and adjust to circumstances. This may apply as much to the agencies themselves as to the projects that they run.

Future Research

Very often in federal research, partnerships that span multiple federal agencies are required to meet complex national challenges. A notable example of this is the National Nanotechnology Initiative (NNI), in which twenty-five departments and agencies are partnering to create revolutionary technology by controlling matter at the nano-scale (National Science and Technology Council 2011). More recently, the Obama administration's Advanced Manufacturing Partnership (AMP) will bring together federal as well as academic and industry partners to improve technologies such as robotics, nano-manufacturing, and advanced materials design (National Institute of Standards and Technology 2011). Thus, in addition to intra-organizational factors of innovation, it may also be fruitful to look at cross-organizational ones, as well as how the innovation heuristics at different organizations might reinforce or counteract each other.

The island+bridge model elaborated here, while attending equally to the isolation of technology development and to the connection to application and social value, is still anchored on the supply side of innovation. Use is the inspiration, and the destination, but the core activity is still advanced science and engineering. Might there be a complementary 'mainland-bridge' model with core activities in product design and operations research, for instance, to bring discipline to service-oriented agencies as they try to adopt technologies demonstrated by the likes of DARPA, ARPA-E, and ATP? For example, the National Oceanic and Atmospheric Administration (NOAA) has been working for decades to incorporate aerial drones (originally developed by DARPA) into its fleet of environmental observation platforms (Hooke 2013). How might this be

done in a more coordinated fashion? Is it possible to design an 'anti-ARPA' organization, separated from basic research, technology development, and regular operations, yet centered more on technology pull, than on technology push?

With respect to the centrality of **frank, frequent, face time** to the island+bridge model, we may also learn a great deal from studies of group cognition. There is great economic potential to realize the latent synergy that exists across technical and other specializations (Hill 2007). Page (2007) has shown how groups with diverse expertise can outperform groups of specialists, assuming the task is sufficiently complex and the group members can communicate well enough to overcome any hindrances from their diversity. Hollingsworth and Hollingsworth (2011) have found that the major discoveries made in biomedical research over the twentieth century were facilitated by factors that included moderately high (but not extreme) scientific diversity, social integration among scientists from different fields through frequent and intense interaction, as well as managerial capacity to lead the integration of diverse science and recruit scientists who can, themselves, internalize scientific diversity.

Future research might, therefore, be directed toward enabling and improving strategic communication among cognitively diverse groups of scientists, engineers, program managers, and other stakeholders. In this area of research, there are a number of exciting questions to explore. What modes of communication work well for this purpose? Are successful results attributable to shared vocabularies? Or complementary personality traits? Could new technologies and social media be applied to facilitate collective sense-making? If, as it is suggested here, transformative innovation is a result of connections among people, then we can expect innovations in communication and in media to have second-order impacts on the productivity of innovation systems.

Conclusion

The set of organizational factors at play in the three model agencies seems, at first, riddled with contradictions. Agencies are separate, and they are connected. They are scientific, and they are social. They defy conventions, and work within the rules. They are clear about where they want to go, and rely on others to get there. They focus intently on performance metrics, and actively encourage failure. They aim for broad benefit, and work on specific applications. They maintain small programs, and grow large communities. They make little bets, and expect big payoffs. The model presented here is intended to harmonize these apparent contradictions. Assembled as a nested typology of heuristics, it offers a general account of factors concerning organizational structure of these agencies at the institutional level, as well as the management of programs and projects at a personal level. Individually, the heuristics are simple – concepts that probably underlie the decisions at many organizations. Taken together, however, they reveal a gestalt of organization for innovation for transformation.

Each of the three model agencies examined here represent unique instances of the island+bridge model. Organization at DARPA emphasizes a streamlined bureaucracy and a flat hierarchy, managerial talent for systems thinking and definition of portfolios systems of technologies, as well as a habit of learning from failure and transition of technologies though people in transition. ARPA-E distinguishes itself from the other two in that managers were very hands on with their administrative staff, SETAs, and contractors, they focus intently on negotiation with performer and on using 'go or no-go' decision points. Managers at both DARPA and ARPA-E are visionaries and dreamers that are driven to change, and empowered to do so. They both emphasize managing portfolios of complementary thrusts, taking diverse approaches to show the world what is possible, and monitoring progress with technical metrics.

While not categorically different from DARPA and ARPA-E in terms of most of the factors in this study, ATP stood out in several ways: its bureaucracy was not particularly flexible, it was managed to encourage joint ventures among research performers, Congress was a vocal critic, and it evaluated economic impacts of its projects as a matter of regular practice. Both ARPA-E and ATP share a focus on economic outcomes that was different from DARPA. Both made project choices based particularly on credibility of the case made in proposals and the plausibility of the path to broad public benefit. Both closely managed the business side as well as the technological side of projects, which had objectives to demonstrate cost efficacy and metrics to gauge commercialization.

The conclusion of this study is, as such, not that organizations that seek to organize for transformative innovation must do so in one particular way; unique missions and circumstances will dictate what options are possible, and which are preferable. Studying other island+bridge style organizations may, in fact, reveal new factors and different heuristics that could improve the model as it was presented here. The conclusion is also not that all federal research agencies should organize for transformative innovation. Programs that focus on basic scientific research and incremental technology development should have organizational models appropriate to their own desired outcomes. This study offers no judgment as to which of these types missions are inherently more valuable.

If, however, we have decided to aim for radical change in society and economy, then the island+bridge model of nested heuristics may be of use. Compared to other frameworks for analysis, such as Dugan and Gabriel's 'Special Forces' model, the island+bridge model provides a more holistic account of practices of transformative innovation. Mission agencies and inter-agency programs in the federal research enterprise can use it for more systematic evaluation and planning of the, as called for in Rising above the Gathering Storm (National Academy of

Sciences 2007). It may also find application beyond the US federal system, to better characterize the organization of innovation directed toward transformation in private industry, in the government research efforts of other nations, or in large-scale international R&D efforts, such as the International Space Station (ISS), the Intergovernmental Panel on Climate Change (IPCC), and the International Thermonuclear Experimental Reactor (ITER).

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4

NOAA Research:

Is It Organized for

Transformative Innovation?

Summary

Transformative innovation is not transformative research. The former is actually transformative; the latter is still only potentially transformative. If we are truly concerned with social and economic outcomes, transformations must occur in the realm of applications – beyond the realm of research. A few government agencies have been designed from this perspective. They have an organizational culture that turns visions into reality by applying a pattern I call island+bridge. I use this model (developed in Paper 2) to frame a study the central research division of the National Oceanic and Atmospheric Administration (NOAA) in order to learn how a science-based mission agency might better translate research into transformative outcomes. Assuming that NOAA would like to adopt the island+bridge model, it should consider the comparative advantages of: program offices, versus laboratories and cooperative institutes; programs in a special unit, versus programs in a science division; negotiated cooperative agreements, versus open-ended grants; excited temporary managers, versus expert permanent scientists; and tackling risks though specificity, versus assuming risk though ambiguity.

Introduction

In the interest of US competiveness, Congress has directed NOAA (The National Oceanic and Atmospheric Administration) to create a 'strategy for enhancing transformational research in its research and development portfolio' (US Congress 2011). The federal government's role in transformative research is a topic that has received much attention has received much attention of late (American Academy of Arts and Sciences 2008; Bement 2007; Dietz & Rogers 2012; Frodeman & Holbrook 2012; National Academy of Sciences 2007; National Science Board 2007; National Science Foundation 2007; US Council on Competitiveness 2005). Until recently, the focus of transformative research has been on basic research at the National Science Foundation, rather than mission agencies such as NOAA.

With basic research as the focus, the framing of the issue has naturally been Kuhnian: transformation is the result of revolutionary work that intentionally tests paradigms rather than solving puzzles. 'By concentrating scientific attention upon a narrow area of trouble and by preparing the scientific mind to recognize experimental anomalies for what they are, crisis often proliferates new discoveries' (Kuhn 1969/1996, p. 90). Research that is transformative could be experimental work that aimed at provoking crisis, or it could be theoretical work to develop the alternative paradigms that will be available for adoption once a crisis emerges. It might be the result of early career scientists, or interdisciplinary teams. In general, though, the attention on transformation typically emerges from concern for fundamental knowledge rather than ultimate application, to borrow a distinction made by Stokes (Stokes 1997).

Over the past few years, recommendations of NOAA's Science Advisory Board, a federal advisory committee, have made clear that the agency should foster transition of advanced research capabilities into operations – usually corresponding to traversing an organizational

divide between NOAA's research division to its service lines. A 2004 report of the SAB emphasized that 'it is imperative that the products of NOAA's research, be they operational advancements or expanded information services, reach the user' (Moore et al. 2004, p. 8). A more recent SAB analysis of the NOAA research and development (R&D) portfolio stated…

> NOAA is a mission organization. The three pillars of the organization are science, service, and stewardship. Its work begins with science, but unless that science is transitioned into operations – whether in services to the Nation or stewardship of the Nation's resources – NOAA will fail in its mission. NOAA must make certain that the intended end use of the scientific information is understood from the start by its researchers working on scientific questions and, ensure that internal as well as external end-user needs are incorporated explicitly into the problem formulation (NOAA Science Advisory Board 2013, p. 13).

The concern for 'transition' at NOAA parallels the concern for 'translation' and 'transfer' at other science-based mission agencies. The National Institutes of Health, for example, has established the National Center for Advancing Translational Sciences (NCATS) to remove the barriers between medical discoveries and treatments for patients (National Institutes of Health 2011). The Advanced Research Projects Agency – Energy (ARPA-E) clearly sets aside five percent of resources for technology transfer and outreach activities (Advanced Research Projects Agency - Energy 2012).

Transformation is a priority. Transition is a priority. Are transformation and transition at odds? This paper assumes that they are not. Indeed, if US competitiveness (or other ultimate social and economic outcomes) is our true concern, transformations must occur in the realm of applications – beyond the realm of research – and therefore require transitions to become real. Per prior work (Sen 2013 and Chapter 2, above), I assume transformative innovation implies that efforts are 'organized to realize concrete visions of change beyond the research community', that is, an R&D program embodies transformative innovation if: it is intended to be radical in its benefits to society and economy, not merely in its challenge to prevailing scientific paradigms; it is driven by

a clear and tangible objective, not discovery of 'unknown unknowns'; and it combines all the capabilities necessary to achieve that objective.

There are a few government agencies have been designed to achieve transformative innovation. Those that do apply an institutional pattern that I refer to as island+bridge (I+B). They craft clear visions of tangible outcomes that are radical for range of stakeholders, and they have an organizational culture that turns visions into reality. In the pages to follow, I compare NOAA Research (formally: NOAA's Office of Oceanic and Atmospheric Research) to this model to see how similar or different they are, and what options there are for such a mission agency to better translate research into transformative outcomes. What I find is that, while NOAA Research displays some qualities of R&D on the 'island' and the 'bridge', it is strongly rooted on the 'land'. I delve into what this means in the Interpretation section, but I first provide background on NOAA Research and the I+B model, describe the research question and methods, and detail findings. I conclude with a discussion and considerations for NOAA.

NOAA Research

NOAA is comprised of the National Weather Service, National Marine Fisheries Service, National Ocean Service, National Environmental Satellite Data and Information Service, and the Office of Oceanic and Atmospheric Research (OAR, also known as 'NOAA Research'). OAR serves alongside NOAA's other operational lines as the agency's primary research arm. While R&D is funded and conducted by NOAA's other lines, the lion's share of NOAA R&D activities are housed under OAR, which is also responsible for coordinating cross-line research efforts. OAR focuses on topics of climatology, meteorology, and atmospheric sciences (the so-called 'dry side' of NOAA), as well as ocean exploration, ocean chemistry, and coastal environmental quality (the 'wet side' of NOAA).

At OAR, R&D is funded or conducted by three varieties of organizational units: laboratories that conduct R&D, program offices that fund R&D (often at OAR laboratories); and NOAA-commissioned cooperative institutes (CIs) that conduct R&D at universities (funded though OAR laboratories or program offices). Table 6 provides a list of OAR's operating units.

Table 7: OAR Laboratories, Program Offices, and Cooperative Institutes[9]. Note that the relatively large Earth Systems Research Laboratory (ESRL) and the Climate Program Office (CPO) are broken up into their major divisions.

Laboratories
Air Resources Laboratory (ARL)
Atlantic Oceanographic & Meteorological Laboratory (AOML)
Earth Systems Research Laboratory - Director's Office (ESRL-DO)
Earth Systems Research Laboratory - Global Monitoring Division (ESRL-GMD)
Earth Systems Research Laboratory - Physical Sciences Division (ESRL-PSD)
Earth Systems Research Laboratory - Chemical Sciences Division (ESRL-CSD)
Earth Systems Research Laboratory - Global Systems Division (ESRL-GSD)
Geophysical Fluid Dynamics Laboratory (GFDL)
Great Lakes Environmental Research Laboratory (GLERL)
National Severe Storms Laboratory (NSSL)
Pacific Marine Environmental Laboratory (PMEL)
Program Offices
Climate Program Office – Climate and Societal Interactions (CPO-CSI)
Climate Program Office – Research and Predictions Division (CPO-RPD)
Climate Program Office – Climate Observations Division (CPO-COD)
National Sea Grant College Program (NSGCP)
Office of Ocean Exploration & Research (OER)
Office of Weather and Air Quality (OWAQ)
Office of Ocean Acidification (OA)

[9] Note that the larger units, the Earth Systems Research Laboratory and the Climate Program Office, have been broken up into their respective subdivisions. Note also that there are other cooperative institutes not included on this list that are funded by other NOAA line offices.

Cooperative Institutes
Cooperative Institute for Climate Science (CICS)
Cooperative Institute for Alaska Research (CIAR)
Cooperative Institute for Limnology and Ecosystem Research (CILER)
Cooperative Institute for Marine and Atmospheric Studies (CIMAS)
Cooperative Institute for Marine Ecosystems and Climate (CIMEC)
Cooperative Institute for Mesoscale Meteorological Studies (CIMMS)
Cooperative Institute for Marine Resources Studies (CIMRS)
Cooperative Institute for the North Atlantic Region (CINAR)
Cooperative Institute for Ocean Exploration, Research and Technology (CIOERT)
Cooperative Institute for Research in the Atmosphere (CIRA)
Cooperative Institute for Research in Environmental Sciences (CIRES)
Joint Institute for the Study of the Atmosphere and Ocean (JISAO)
Northern Gulf Institute (NGI)

OAR – and NOAA as a whole – represents an amalgamation of existing federal authorities with extensive pedigrees and distinct cultures. OAR's origins date back to 1807 when the Jefferson Administration created the Survey of the Coast to spur development of the western frontier though better data on water levels and currents for navigation. In 1967, the Johnson Administration merged the Coast and Geodetic Survey, United States Weather Bureau, and the Central Radio Propagation Laboratory of the National Bureau of Standards into the Environmental Science Services Administration (ESSA) in 1967. ESSA and the Commercial Fisheries Bureau were folded into NOAA in 1970 under the Nixon Administration. In 1983, The Reagan Administration combined what had been separate offices (NOAA's Office of Oceanic and Atmospheric Services and its Office of Research and Development) to form what is known to this day as the Office of Oceanic and Atmospheric Research. The NOAA Business Operations Manual provides a primer on some of the finer details of NOAA's current research portfolio:

> NOAA spent $630 million on R&D in FY 2011, with 71% going to research, 7% to development, and 22% to R&D infrastructure. Over half of NOAA's R&D budget is spent by OAR… Over the past few years, non-federal partners have conducted between a quarter and a half of the R&D that NOAA funds. R&D at NOAA is conducted by about 2700 bench scientists (workers at NOAA facilities who are expected or encouraged to publish), about 1700 of which are federal employees, with the remainder from academic, private, or not-for-profit entities. Most specialize in either natural resources management and biological sciences (about 1300) or the physical sciences (about 1000) (NOAA Office of Program Planning and Integration 2013, section 3.12)

In the first term of the Obama administration, under the leadership of Dr. Jane Lubchenco, NOAA attempted a reorganization that would create a Climate Service (akin to the National Weather Service), to provide climate information in a more regular, customer-focused fashion. The Climate Service would be formed out of laboratories and offices that were in OAR, leaving a much smaller, non-climate portion of OAR at NOAA's research hub. This reorganization encountered political resistance from Congress, and whether or not there will ever be a separate business unit for climate services at NOAA remains to be seen.[10] However, the attempt to create a Climate Service as a separate business unit has triggered the search for the proper role of OAR within NOAA.

Climate science – once a young, transdisciplinary field connecting atmospheric chemistry with physical oceanography – has become the hub of research at OAR. Climate science at NOAA now provides national assessments, seasonal and even regional predictions with regularity and reliability. Those at NOAA ponder: Should OAR continue to pursue climatology – or any other discipline, for that matter – even as it matures into a field of regular, scientific operations? Or should it define itself more by the type of work it does – namely: research and development, rather than operations – and concern itself less with the sustenance of maturing scientific disciplines? OAR has already been changing slowly from the former role to the latter. Where its

[10] Indeed, the entire issue may be overshadowed by a reorganization of the Commerce Department, which could place NOAA in the Interior Department.

identity was once defined by discipline, external pressure is forcing it to define its identity by function.

The decision to carve out from OAR a Climate Service is one example of the evolving narrative of OAR's raison d'etre. In the face of Congressional pressure in the 1990s to distribute OAR labs among NOAA's operational units, NOAA's Science Advisory Board stated that the purpose of OAR is to protect long-term science from the immediate concerns of operational business lines (Moore et al. 2004). OAR's recently revised mission is 'research, development and transition' toward the strategic goals of NOAA as a whole. OAR has also revised its budget structure for FY 2014 to reflect functions such as observations, predictions, and outreach, rather than disciplinary themes of climate, weather, and oceans.[11] Still, it is unclear whether the 'R' in 'OAR' is supposed to advance the science of OAR and its university partners, or to advance service innovation in NOAA's operational line offices, or both.

Most recently, Congress has also provided direction on the course of NOAA research – stimulated, no doubt, by the Climate Service debate, and the potential for a smaller central research unit at NOAA (O'Konski & Piltz 2011). The America Creating Opportunities to Meaningfully Promote Excellence in Technology, Education, and Science (COMPETES) Reauthorization Act of 2010 directs NOAA to craft a report describing its 'strategy for enhancing transformational research in its research and development portfolio to increase United States competitiveness in oceanic and atmospheric science and technology' (US Congress 2011). Should the portfolio of NOAA's R&D be, as some in NOAA and Congress suggest, more 'transformative'? What would this mean? Could 'transformative' be the mantra that will provide OAR with renewed purpose?

[11] One could interpret this change, of course, as a political tactic to obscure the budget for 'climate science', in response to long-standing opposition from Congressional conservatives.

Island+Bridge

As Bennis and Biederman write of the many groups responsible for transformative innovation, every one 'is an island, but an island with a bridge to the mainland' (Bennis & Biederman 1997, p. 206). The island is the refuge for experimentation and failure, and the bridge is the conduit for the transfer of knowledge and technology. Innovative organizations – contrary to popular mythology – are not an island alone, but constitute both an island and a bridge. New technological capabilities make their way out, requirements and other sorts of influence must make their way in. Research is neither entirely shut off from real-world interests, as with a traditional laboratory setting, nor is it beholden to the interests of operational incumbents. The island+bridge model applies a 'connected science' approach to research (Bonvillian 2009), combining and integrating the forces of technology push and pull, balancing the need for isolation and connectivity, and providing just the right types of structure for processes that are necessarily chaotic.

Google's 'hybrid' model of research (Spector, Norvig, & Petrov 2012) is an example of the island+bridge model, as is the 'connect and develop' model at Procter & Gamble (Huston & Sakkab 2006). It also describes a number of federal R&D agencies that have a track record for delivering transformative results. DARPA, the Defense Advanced Research Projects Agency, 'was formed to address the problem of transformative innovation' (Bonvillian & Van Atta 2011, p. 470) during the Cold War, when the centralized R&D units of US Army and Navy had become too focused on fundamental principles and produced innovation that was too incremental. A semi-detached organizational model was also the basis for a number of innovative federal agencies that were modeled on DARPA, including the Intelligence Advanced Research Projects Activity (IARPA), and In-Q-Tel, a non-federal partner of the Central Intelligence Agency (Cotell 2006; Yanuzzi 2000), as well as the Advanced Technology Program (ATP) active in the

Department of Commerce during the 1990s (Advanced Technology Program, Economic Assessment Office 2007), and more recently at ARPA-E (Bonvillian 2006).

The island+bridge approach is similar to what former DARPA Director Regina Dugan and former Deputy Director Kaigham Gabriel call the 'Special Forces' model of innovation. They believe that DARPA's success at transformative innovation can be reproduced by other agencies (and companies in the private sector) through research activities that 1) bring diverse talent together only temporarily, 2) focus on ambitious, use-inspired goals, and 3) are organizationally independent from the parent institution (Dugan & Gabriel 2013). The Special Forces model, however, appears to include only the 'island' and not the 'bridge' of the island+bridge model. They do not highlight the need for personal engagement and frank, frequent face time (the keystone of the 'bridge'), which is closely related to setting and achieving ambitious goals.

To investigate the island+bridge approach, I have examined the organization and management of three federal agencies mentioned above: DARPA, ARPA-E, and ATP (see paper 2 of this dissertation). I took as given the transformative capacity of all three agencies (Advanced Technology Program, Economic Assessment Office 2007; Bonvillian 2006; Bonvillian & Van Atta 2011; Boysen & Gur 2012; Fong 2001; Haynes 2013; Kerwin & Campbell 2006; Ree, Van Atta, & Deitchman 1990; Ruegg & Feller 2003; Van Atta, Lippitz, Lupo, Mahoney, & Nunn 2003). I focused instead on the practices of program and project management. In examining these model agencies, I cataloged a host of organizational and managerial factors at work. The fully specified island+bridge model that resulted, however, not only describes the factors that are present at these agencies, but also the logic that underlies those factors. In this study, it serves as a baseline for describing and comparing the transformative nature of another federal R&D effort, namely NOAA Research.

The model itself takes the form of a taxonomy of institutional beliefs – heuristics – about how transformative innovation programs are organized and managed. Heuristics are simple, internal narratives or cognitive shortcuts that staff in organizations learn when facing decisions with limited information, time, and analytical capability (Bingham & Kahl 2013; Bingham & Eisenhardt 2011; Davis, Eisenhardt, & Bingham 2009). By looking at a small number of causal heuristics – as derived from a much larger number of individual factors – we are better positioned to generalize across cases. Moreover, a heuristic model shifts discussion from organizational behavior (i.e. 'DARPA does X') to organizational cognition (i.e. 'DARPA values X'), thereby expanding the possibilities for how an organization might embody transformative innovation differently than how it has previously.

Methods

The primary research addressed here is: To what extent is NOAA's central research division managed like federal R&D agencies that routinely create transformative innovation? To answer it, I compared factors of management and organization at OAR's three mechanisms for getting research done – federal laboratories, program offices, and cooperative institutes (hereafter OAR/L, OAR/P, and OAR/C, respectively) – with three agencies designed for transformative innovation – DARPA, ARPA-E, and ATP (hereafter called the island+bridge agencies, or I+B agencies). I take a data-driven approach, augmenting the qualitative methodology known as grounded theory with some quantitative analysis. I conducted interviews, took detailed notes, and coded all the times participants mentioned distinct factors of management and organization. I assumed that the more often a factor was mentioned, the more certain I could be that it is important. Focusing on the most important factors, I analyzed OAR with respect to I+B in two ways: by type of factor (see below: Findings), then by institutional heuristic (see below: Interpretation).

Data and Coding

Interviews provided the majority of data for this study. Interviews had already been conducted with seventeen I+B managers[12] and, to those, this study added interviews with twenty-nine OAR managers: one interview from every lab director, a program office manager, or cooperative institute director – or one of their respective deputies – per Table 6 above (with the exception of two CIs). All interviews lasted from 40 to 60 minutes. Data took the form of typed notes (plus a few transcribed audio recordings), all made available to participants for editing. Data were binned into three cases for the I+B group (DARPA, ARPA-E, and ATP) and three cases for the OAR group (OAR/L, OAR/P, and OAR/C) for a total of six cases across two groups (see Table 7). These raw data were coded using HyperResearach, a computer assisted qualitative data analysis software package. Quantitative information on coded excerpts was analyzed in Microsoft Excel. Data visualization and heuristic modeling were performed in OmniGraffle Pro, a diagramming application.

Coding of OAR data was performed in four phases. Initial coding aligned responses with those codes already defined for, and applied to the model agencies in the creation of the I+B model (see paper 2). In the second phase new codes were generated to identify factors that emerged at OAR but not at the model agencies. The third phase was horizontal coding, where excerpted material was reviewed within codes (and across sources) to add, remove, or refine codes further. The fourth phase was quality control; coded excerpts were revisited to correct errors in coding. The result of coding for I+B was 1468 textual excerpts tagged by one or more of 163 codes. At OAR, the result was for 2243 excerpts for 269 codes. Because more people were interviewed at OAR than I+B, there were more excerpts (and more codes). To make it possible to compare factors

[12] Interviews at the I+B agencies were supplemented by strategic documents made public by the agency and written presentation material delivered by program managers that addressed interview questions directly.

qualitatively across cases, and therefore OAR versus I+B, I then converted excerpt values to percentage points. The values used in analysis were, for each factor in each case, the proportion of excerpts coded as that factor relative to all excerpts in that case.

Table 8: Number of coded excerpts by case and type of source material[13]

	I+B cases			OAR cases		
Source Material	DARPA	ATP	ARPA-E	OAR/L	OAR/P	OAR/C
Interview Notes or Transcripts	6	6	5	11	7	11
Published Strategy or Process Documents	2	1	2	0	0	0
Published Presentation Materials	1	0	2	0	0	0
Coded Excerpts	**474**	**507**	**487**	**881**	**551**	**811**

In this study, a code represents a factor that at least one interviewee felt worthy of attention in the course of discussion, and that I believed to be relevant to addressing the research question. A coded excerpt is a single instance of a factor being raised in discussion. If a factor was discussed several, independent times in an interview, it was coded as many times as it was raised.[14] Each code indicates the presence of a unique factor, some of which were mentioned many times in all

[13] Note that interviews at the model agencies were supplemented with additional data sources, i.e. the published materials. A comparatively larger number of interviews at OAR obviated the need to code supplementary materials. While not coded, the content of published documents available for OAR confirmed the responses to interview questions.

[14] Coding was performed in preparation for this analysis. When a topic was raised, the code for that topic was applied to that piece of text only once. Thus, if an interviewee discussed the use of prizes to engage external research partners, and raised the topic of prizes three times within the course of a few minutes (or a few sentences of notes), the code 'award prizes' would be applied only once. A code may be applied multiple times per document if it were raised again in answer to a different question, or different section of text (as in the case of a published document), or if it were raising a significantly different aspect of that topic from prior mentions. This method did demand the discretion of the researcher in some cases.

cases, others only once or twice in each case. The following step was to reduce the number of factors down to a more manageable number by isolating those that had the highest frequencies.

Analysis and Synthesis

To refine the list of factors, I took a weight-of-evidence approach. I assumed that I could be more certain that a particular factor was an important part of organizing transformative innovation if, in guided discussion, it were raised more often than other factors. Thus, the excerpt percentage points value (per code, per case) was my basis for determining which factors I could be most sure were important. I could then order the codes from 'most certainly important' to 'least certainly important,' and focus on those with higher frequencies. I did not assume that there is one best way to sort and isolate them; instead, I identified the top tier of factors in a few different ways, and used the combined result as the list of factors most likely to be important.

I ran four quantitative tests of importance, each sorting the list of all codes by a different statistical value for the two groups of cases: I+B and OAR. I started with those codes with high total numbers; the first test was simply a sort of all codes by total number of excerpts in all cases in a group (i.e. where $N_{I+B} = N_{DARPA} + N_{ARPA\text{-}E} + N_{ATP}$ and $N_{OAR} = N_{OAR/L} + N_{OAR/P} + N_{OAR/C}$). I also wanted those codes where numbers were not only high, but also highly consistent across the three cases in each group, that is, codes that had a large number of total mentions and a small difference among the number of mentions across DARPA, ARPA-E, and ATP, and the same for OAR labs, program offices, and CIs. The second test sorted codes by the total in all cases divided by the standard deviation across cases in a group. Next, I wanted to identify those codes that had relatively high values even in the cases with the lowest number of excerpts, thus, the third test sorted codes by the highest minimum number. Finally, to identify those codes for which there was a particularly high value in at least one of the cases, the fourth test sorted codes by highest maximum number to reveal factors that may be salient, even if they are not universal.

Only those codes with values in the top third of list, thusly sorted, would pass each test.[15] Passing any test identified a factor as 'important,' relative to factors that did not pass a test. Passing more tests indicated that the factor is more 'robust,' and therefore more likely to be important relative to those factors that passed fewer tests. For I+B, this process yielded 96 factors as important out of the original 163, for OAR it yielded 128 out of 269. (Test results are provided in Table 8. A full list of codes, with excerpt quantities by case, associated statistics, and test results, is provided in Appendix 1.) Analysis of the OAR responses yielded a host of emergent codes for factors that were not present (or did not emerge as important) as part of the I+B model.

Table 9: Number of codes that passed each test of factor importance

Tests of Factor Importance	**I+B**	**OAR**
Test 1: 'Total is high'. (Sort by Total)	55	86
Test 2: 'High and highly consistent'. (Sort by Total / StdDev)	55	81
Test 3: 'At least *X* in all cases'. (Sort by Minimum)	68	86
Test 4: 'At least *X* in one case'. (Sort by Maximum)	61	90
4 out of 4 tests passed (most robust)	30	41
3 out of 4 tests passed (more robust)	47	73
2 out of 4 tests passed (less robust)	66	101
1 out of 4 tests passed (least robust)	96	128

[15] In choosing how to define the 'top tier', I did not assume too much quantitative precision. Thus, I opted for a scale of high, medium, or low and chose those codes that were high. I initially looked at the distributions across codes for natural breaks, but the distributions were too smooth. I chose to determine importance though these multiple tests in order to create more defined cut-offs. Codes that were not found to be important on any test are clearly lower total group numbers, lower case numbers, less consistently high numbers across cases, and do not have at least one excerpt in each of the three cases within a group. (See Appendix for complete list of codes with excerpt values and associated statistics.)

In developing the I+B model, and in using it to assess OAR, the most important factors at I+B and OAR, respectively, were recombined by clustering similar or related factors together organically. Clusters represented culturally pervasive heuristics that seemed to underlie constellations of factors. They were derived as the commonalities of clustered factors, or of clusters of other heuristics.[16] Model development was an iterative process; heuristics were defined and redefined as factors were combined and recombined. Relations among factors and heuristics were established visually by concept mapping (Bryson 2004; Novak & Cañas 2008).

The I+B model was the starting point – a qualitative baseline – against which I compared organizational heuristics at OAR. As such, most heuristic clusters were created for I+B, whereas others were created to account for important factors that emerged at OAR. Most of the important factors at OAR were already defined and associated with heuristics derived from the model agencies. About half of the excerpts underlying OAR-important factors applied to existing heuristics, while the other half applied to factors yet to be organized. The important factors unique to OAR were, therefore, recombined into OAR-specific heuristics. At the end of this synthesis, all heuristics for all important factors in all cases were incorporated into a combined model of island, bridge, and land. (See Appendix 2 for a tree map of the final model, which scales heuristics and contributing factors in each case by excerpt values as percentage points.)

Findings

Before delving directly into the similarities and differences among heuristics at I+B and OAR, it might be helpful to understand and compare their respective important factors by type. Factors naturally fall into a few subject categories: the life cycle of projects, the role of the manager, how

[16] In some cases, individual factors underlie more than one heuristic, but, for the purpose of reporting and visualization, each was associated with a single, primary heuristic.

programs/projects are decided upon, how they are planned for, how they are executed, the political relationships involved, and the process of transitioning research results into valuable applications. These categories reflect (roughly) the way that interviews with participants were structured.

The Life Cycle

Before going into any further detail, a comparison of program and project life cycle may provide useful context. In the I+B model, a program starts when leadership defines an application area, and a manager is hired. The new manager studies the area, perhaps doing a 'deep dive' with those in the relevant scientific and engineering communities, then pitches an idea to the agency director for the challenge that will guide the program. The manager defines a portfolio based on feedback and solicits more specific concepts from the community. An announcement of opportunity is published, then proposals are received, reviewed, and selected. Metrics, milestones, and schedules are negotiated with selectees before the award is granted, and regular reviews are conducted with performers over the 3-4 year life of the project. Successful projects result in the adoption of the technology created.

The labs, program offices, and CIs at OAR are more enduring and evolve over time, so it is not as easy to say when they begin. At some point, of course, an area of work was defined and the manager hired. Managers at OAR also do 'deep dives' with the broader community in which they work. Another foundational activity, particularly at the labs and CIs, is scouting and hiring new scientific talent to recruit the appropriate researchers. OAR managers do not pitch their units' defining idea to the NOAA Administrator or the Assistant Administrator that leads OAR. They do not have much contact with agency leaders, in general. OAR managers determine their own direction and pick portfolios of projects often with others in their unit.

OAR program offices operate much like the I+B agencies: issuing announcements, receiving and selecting proposals, awarding funds, and facilitating project execution. They have no research staff of their own. OAR labs and CIs execute projects with resident staff and 'base' funds, but PIs are also encouraged to submit proposals to bring additional funds into the unit. Proposals are reviewed by OAR manager, often with unit-wide committees (to confirm mission relevance), before being approved for submission to the external funding agency. By and large, OAR managers do not negotiate metrics, milestones, and schedules with investigators prior to project execution. Regular reviews with performers do happen, but less frequently and with fewer or less specific metrics.

Overall, OAR managers discuss project termination much less than I+B managers. Whereas managers in both groups address ending projects when there is no demonstrated progress and no 'plan B,' those at the I+B agencies also note other important ways that a project might end: when time runs out, when a challenge is met successfully (see the Planning section), or when a capability has been demonstrated to the point where others can refine it (see the Decisions section). While transfer of research results to adopters was important at both OAR and the I+B agencies, managers at OAR discussed transfer of results more often. In some case it was highlighted as an area in which OAR needs help.

The Manager

In contrast to the temporary tenure of program managers at the I+B agencies, there are no time limits for managers at OAR labs, programs and cooperative institutes. While about half of the OAR managers have been in their positions for 2-4 years, the other half have been there 5 or more years. The average tenure at OAR was about six years; for those who have been in their positions for 5 or more years, that average is over 10 years.

A greater proportion of the managers at the I+B agencies completed advanced degrees or certifications in management disciplines (either business administration, engineering administration, or accounting) than their OAR counterparts, none of whom studied management. A slightly higher proportion of those at OAR had advanced degrees in science or engineering and had experience working in academia than in I+B. More of those at the I+B agencies had experience in private industry, while more of those at OAR had experience in government.

Both I+B and OAR managers have deep technical or scientific expertise, the ability to motivate researchers, and expressed skills in program administration and bureaucracy. However, this is where their similarities end. At the I+B agencies, managers clearly possess talents for systems thinking and for providing technical direction to investigators (as distinct from their own expertise). They have an internal drive to create change and know how to commercialize new technology. These factors of managerial talent represent the biggest differences from OAR managers, who emphasized these talents much less. The ability to communicate well is an aspect of managerial talent that was present in both groups, but showed up as a relatively important factor only at OAR.

The role of a program manager at an I+B agency is – predominantly – an entrepreneur, a visionary, or a dreamer (delivering on dreams, not just having them). They often function like movie producers or orchestra leaders (by coordinating diverse people to create something none of them could alone), and serve as general problem solvers and facilitators for investigators. Managers at OAR share these latter two perceptions of role, but to a lesser degree. The primary role at OAR is to be a lead scientist, or the 'scientist who happens to be in charge'.

Very few OAR managers see their role as manager in the 'orthodox' sense; some find management antithetical to science. They do, however, take very seriously their role as coach or

mentor to those earlier in their careers (a factor that was present, but not as strong at the I+B agencies). Many OAR managers see themselves in the role of someone who connects people to opportunities, i.e. a liaison or ambassador. They also see their role as what was alternately described as a sailor, or a chicken herder, or an orthodontist: someone who exercises slow, steady, often indirect control over scientists to guide them toward the mission of the agency, and over the mission toward 'the science'.

At both I+B agencies and at OAR, managers spend most of their time (i.e. day-to-day, week-to-week, month-to-month) interacting with principal investigators, as well as their own administrative staffs. OAR managers also interact a great deal with others in the OAR system, that is, lab directors, program managers, and CI directors often talk to each other; lab directors, in particular, spend a good deal of time with their division chiefs. OAR managers also spend more time interacting with those who adopt the results of their scientific work (e.g. managers at the National Weather Service, or representatives of state and local governments), as compared to I+B program managers.

The Decisions

The biggest differences between the I+B cases and the OAR cases are in how projects are chosen. The program managers at I+B agencies are the ones who choose which projects are executed (with input, of course, from proposal review panels). OAR decisions, on the other hand, involve a cast of characters (including sub-unit managers), many of whom control resources from outside of the operating unit (at other OAR labs, programs, CIs, as well as partnering universities). At OAR – and at OAR labs and CIs in particular – the principal investigator (PI) was identified most frequently as the person choosing projects.

I+B agencies choose projects specifically because there is a high risk of failure (both technical and institutional), because no one else will do them (i.e. they are 'crazy ideas'), and because managers can foresee broad benefits to society and economy at large. Programs are defined by program managers (with approval from the agency leaders) to address specific problems; projects take diverse approaches to solving them. Proposals must demonstrate technical merit, make a credible case for meeting pre-defined objectives, and show a plausible pathway to a specific application. Projects must build on (but not be) basic research.

At OAR, project proposals should also build on basic research and show a plausible path to application, though these were less important criteria than in the I+B cases. Project choice at OAR is determined by other criteria: if the project fits within one of the broad science themes that define the work of each unit; if the results might be relevant to the operational needs of NOAA; and if the results might satisfy the demands of the scientific community beyond the unit itself. The biggest difference between the I+B agencies and OAR in the entire study was the role played by perceived risk as a criterion. Few OAR managers cite 'high-risk' as a factor of project choice; some, in fact, feel that the organization encourages projects that are low-risk.

Project portfolios at I+B agencies are defined by technology in a few different ways: technological application (or industry), systems integration versus single components, or by 'thrusts' of technologies that are envisioned to complement each other (possibly through future systems integration). OAR managers also understand their portfolios in terms of technology applications and complementary thrusts, however, just as important is understanding portfolios in terms of scientific themes (which are multi disciplinary, but science-focused, and often defined by a research question). Also important at OAR is managing projects based on where the finances come from, particularly at labs and CIs. Funds appropriated by Congress are considered

'base resources,' and are often supplemented by funds from other agencies (such as NSF), which are solicited competitively by individual PIs.

OAR managers define their portfolios in several other ways that differ from their counterparts at I+B agencies, though these are not necessarily categories they use for apportioning investments. First, they make a clear distinction between activities of science and those of technology, and often note that the latter are conducted in the interest of the former. Also, as noted above, there are two types of projects at OAR: finite and ongoing. These two types seem to correspond roughly to OAR managers' portfolio distinction between sustained observations and all other studies and experiments. Sometimes, projects are thought of as being either high or low risk, and as either mature activities or 'seeds' to be planted. Managers of OAR program offices, in particular, understand projects as either the creation of science and technology or their diffusion to downstream users.

For both I+B and OAR, allocation of resources across portfolio categories is done – for the most part – at an organizational level above the manager, but, where he or she does have control, managers make allocations based on professional judgment, rather than by a strict formula. The one exception to this is at OAR, where some participants described managing budget cuts as keeping historical ratios across portfolio categories (i.e. 'haircuts'). At OAR, allocation decisions can involve partners who contribute resources (largely other labs/programs/CIs), and sometimes the principle investigators performing the research.

The Planning

At the I+B agencies, the focal point for every program and every project is a challenge or vision statement that defines a specific technology or technology application. This is not the case at OAR. While challenges (broadly defined) do seem to drive projects at OAR, they tend to be

challenges of scientific understanding, and less specific than those of the I+B group. At OAR labs and CIs, if project-driving challenges are more specific and more technical, this tends to be the result of requirements defined by an external funder, rather than the result of routine practice of strategic planning.

Performance metrics and milestones are specific at I+B agencies, in terms of technical progress and often commercialization. Based on specific metrics and milestones, managers at these agencies set up several 'go or no-go' decision points at the beginning of a project, which structure regular check-ins with research performers over the life of the project. Standardized stage gates, such as technical readiness levels, are not used. OAR managers, on the other hand, do use technical readiness levels in some units, but do not set up (or return to) 'go or no-go' decision points. OAR managers discussed specific metrics and milestones much less than I+B managers; in fact, this was another notably large difference between the two groups. Where they were specific, OAR managers talked about performance in terms of technical capability, but also those that were scientific: papers published, as well as data use or citations. OAR managers understand performance as the successful application of technologies (usually models or algorithms) at NOAA's operational service lines.

It is worth noting that a few managers from OAR program offices were emphatic about assessing their performance in terms of the outcomes for ultimate stakeholders, specifically community engagement and learning. One manager rejected the notion that they should strive for monetary return on investment because that stifles innovation. Instead he said they should try to answer questions like, 'Are people saying this information will help them plan and coordinate any better? Did these observations help stave off this risk? Can we map how this works to inform decisions'?

The Execution

Another clear difference between the I+B agencies and OAR is where projects are executed. Under the I+B model, all projects are competed externally. At OAR labs, they are conducted in-house by bench scientists (either federal or university employees), and at OAR CIs they are conducted at universities by university employees. OAR programs offices fund projects that are conducted at universities, OAR labs, and the labs of other agencies. OAR participants gave no mention at all of companies executing OAR-funded research, yet an important factor for the I+B agencies was private industry execution. (ATP, in particular, actively encouraged joint ventures by bringing companies together on single projects.)

While I+B managers are careful to refrain from micromanaging the projects they oversee, they are extremely 'active' in their approach to management, that is, they often meet with and receive progress reports from the groups performing the research (at least annually, often quarterly). In high-risk projects, failure is accepted and learning from failure is expected – and learning depends upon regular discussion of progress and approach. I+B managers also work with performers to plan for the business- or application-side of the technologies they develop. OAR managers similarly try not to micromanage and to plan for applications (albeit to a lesser degree than the I+B agencies). However, active project management and systematic learning from project failure did not appear to be important in OAR.

In several interviews, the first response to queries on project management was along the lines of 'it depends on what you mean by a project'. At OAR (particularly at labs and CIs, but at program offices, too) there are two types of projects. There are projects that are finite, that last fewer than five years (mostly 2-3 years), and that concern scientific studies or experiments. Then there are projects that are ongoing, that can last many decades, and that concern the long-term collection of

environmental data or the long-term improvement of predictive models. The ongoing projects were described as 'evolutionary' and 'essentially operational'.

Project management at OAR appears to be a less disciplined affair than at the I+B agencies. Indeed, practices of execution at OAR are better defined at the unit-level than at the project-level. There is a very common operating philosophy: stay abreast of ideas that emerge from academia; recruit and develop new scientific talent; then 'pass through' to them as much 'soft money' as possible. As one interviewee put it, 'the best thing you can do is hire the best people, give them the resources, and get the hell out of the way'.

Cooperative agreements were commonly mentioned in both groups, but its use as a tool of execution was markedly different. In the I+B model, they are the sole vehicles used to fund projects because they are, like a research grant, competitive and not confined to a single solution, but, like a contract, they are directed to the resolution of a specific problem. They allow the research performers to work for up to five years to achieve results, without penalty if the results are not achieved. In their use at OAR, however, cooperative agreements are used to reduce competition at the project-level by *pre-competing* broad research themes to universities in 5-year increments. The agreements serve as charters for CIs that function like labs, but with greater flexibility, different expertise, and a more academic environment than labs.

The Politics

Within the bureaucracy, managers at I+B agencies are empowered; they have the control and accountability to 'drive the entire process'. At OAR, the sense of empowerment tends to be either high or low. Many participants described themselves as empowered (particularly program managers), but others said they were not empowered or even disempowered (particularly CI directors). Participants' responses indicated, in fact, that it is the principal investigator who is

particularly empowered at OAR. Neither I+B nor OAR managers feel that hiring/firing is particularly easy within the federal system, but those at OAR do feel that cooperative institutes are an efficient vehicle for getting (or getting rid of) researchers at OAR labs. Some CI directors objected to the use of their institutions as 'job shops' or 'body shops' for the laboratories.

Both I+B and OAR managers find the bureaucracy in which they operate is, though not particularly flexible, relatively streamlined with a relatively flat hierarchy. However, at OAR, these factors represented a lower proportion of coded responses, and these perceptions applied mostly at the unit level. At the agency level, those at OAR reported that there was too much bureaucracy at headquarters, and that there have been longstanding troubles with OAR leadership, including frequently shifting priorities, no big picture thinking, and a generally 'weak-headed organization'. Insufficiency of resources was another commonly expressed sentiment at OAR that was not present at the I+B agencies.

At I+B agencies, the program operating environment is 'brutal' and fast-paced (with projects competing to prove performance, or else be shut down), but also – perhaps counter-intuitively – very reliant on teamwork among managers and between managers and their staffs. These factors seem barely present at OAR. While managers did not report teamwork in the collaborative sense was as present at OAR as it was at the I+B agencies, OAR managers did note their ability to get around administrative obstacles through networks of personal relationships.

The science divisions of I+B agencies' home departments were, in separate instances, both partners in research and competitors for resources. OAR has a similarly complicated relationship with the service lines of NOAA (particularly the National Weather Service), which are at sometimes customers who take the results of OAR research, and, other times, incumbents who are unwilling or unable to adopt those results. For the I+B agencies, Congress is identified as

both an advocate and (at ATP, especially) a political critic. Managers at OAR do not view Congress as either a strong ally or rival. I+B managers see their agency's top leadership as a strong advocate (particularly when dealing with Congress or the Cabinet), whereas OAR managers do not; a small number reported that they view OAR leadership as a poor advocate.

Both universities and private companies are important partners in research for I+B agencies – indeed, they are the ones performing the work. At OAR, universities are major partners, as are a host of research institutions at other agencies, and in other countries. OAR does partner with some small companies, but not often with large, established firms. As alluded to above, OAR labs, program offices, and CIs also share many partnerships with each other. Yet partners can easily shift from friend to foe. One interviewee described colleagues at OAR as 'frenemies', while another described OAR as 'a feudal system of lieges and lords' that are 'happily at war'.

When engaging with allies and rivals, OAR managers employ several tactics that, while present at the I+B agencies, did not rise to the level of importance that they did at OAR. Those at OAR emphasize being fair and thereby gaining the trust of others institutions, operating in domains where they have a comparative advantage, and seeking external advice on research direction. Both those at OAR and the I+B agencies find it important to build communities of practice (though at OAR these were primarily scientific communities), and leverage their unit's or their agency's reputations when seeking partners (the 'halo effect'). Important in the I+B group but not at OAR are tactics to evaluate and report social and economic impacts (to gain political support), as well as to 'throw rocks' at incumbents in industry and departmental science divisions (to stimulate competition).

The Transition

In the I+B model, the two general approaches to getting research results applied is to show the world what is possible or show the world what is cost effective, thereby motivating the subsequent work of others. This is also the general approach at OAR, however, there is much less emphasis on showing cost effectiveness and even greater emphasis on a third approach: showing the world what has been learned scientifically by publishing papers, posting data sets on websites, and promoting and interpreting research findings.

I+B managers proactively seek adopters of the technologies that they develop, both in the commercial and government sectors, while those at OAR focus on government (and NOAA's operational service lines in particular). In many OAR units, potential adopters approach the manager, rather than the other way around. At some OAR units, the manager does nothing intentional to get results adopted. One noted, 'My job is not to worry about technology transfer' and another said that his was not 'an end-to-end program. I don't expect PIs to shake hands with farmers or things like that'.

In contrast to those at OAR, those at the I+B agencies emphasized that transition depends on people. For example, ARPA-E has a team of people at their headquarters office to assist awardees develop and implement plans for commercialization. At ATP, each project was jointly led by a technical manager and a business manager, the latter being responsible for commercial feasibility. DARPA interviewees often noted that people are, themselves, the medium for technology transfer, for instance, when a researcher brings his or her knowledge to a new job.

Like their I+B counterparts, OAR managers engage potential adopters in the private sector by attending industry consortia and by funding small, creative companies. The researchers in their units patent and license the technologies that they create, and managers may forge Cooperative

Research and Development Agreements (CRADAs) with private firms. OAR is much more likely than I+B agencies to develop agreements for adoption with partners in the government (particularly with NOAA service lines) and connect mature research to the stage gate processes used by these partners (particularly that of the National Weather Service).

One interview question asked participants to comment on the frequency of research leading to application. A small number of those at both the I+B agencies and OAR could not comment, noted indirect or untraceable routes to application, or said that the transition to application happens less often then not. The most common report on application frequency at the I+B agencies was that, while applications do not always happen, it is still worth trying. A common report at OAR was that the transition to application happens more often then not; however, 'application' was understood more broadly at OAR to include anything useful, including a published paper.

Another strong sentiment at OAR that was absent at the I+B agencies is that the science performed by the unit (and by partner institutions) is, itself, the application of much of the research and technology development. As noted above, much of the engineering is performed in the interest of improving systems to collect data or to run experimental models, which are themselves inputs for scientific studies and assessments. 'For our climate researchers,' said one lab director, 'there isn't really an operational end point, so [a pathway to] operations isn't really applicable'.

Interpretation

To interpret the findings above in terms of the island+bridge model, I reframed the important factors in terms of the organizational heuristics that may underlie them – simple, intuitive, and

commonly held rationales that might explain the presence of these factors. As discussed earlier, understanding the patterns of collective cognition can help us understand how similarly (or differently) OAR behaves relative to R&D organizations with a demonstrated capacity for transformative innovation and, furthermore, what changes might be possible for OAR to make to increase its own capacity in this regard.

This section provides an assessment of OAR in terms of heuristics defined by examining DAPRA, ARPA-E, and ATP (see Paper 2 for a detailed description of the original model) as well as heuristics that surfaced at OAR, but not at the model agencies. As noted above, analysis of the OAR responses yielded a host of emergent codes for factors that were not present (or did not emerge as important) as part of the I+B model. Consequently, while the original model consisted of two components, the island and the bridge, analysis at OAR prompted the development of a third component, the land, which was, heretofore, implied but not explicitly addressed. Detailed findings are discussed below in terms of heuristics nested under 'island' or 'bridge' or 'land', as shown in Figure 6. Heuristics are designated as two- or three-word phrases and described in terms of the important factors that were clustered to form them, as summarized in Table 10. The most immediate difference between OAR and the model agencies is the presence of a number of heuristics that, with one exception, are associated with the metaphor of land, not island or bridge.

The section is organized per the relationship among heuristics in Table 10. There are three primary 'domains' (at the first level): the ISLAND, the BRIDGE, and the LAND. Under each of these domains are a few general heuristics (at the second level), and under each of these are a few, more specific heuristics (at the third level). Heuristics are described in terms of a cluster of important factors (at the fourth level). Aside from the taxonomic structure, heuristics are addressed in no particular order. The same taxonomy is displayed in Figures 7, 8, and 9 as tree maps for island, bridge, and land, respectively, where the area of the circle for each heuristic is

proportional to the number of coded excerpts of the underlying factors. Figure 10, at the end of this section, displays a combined tree map of island, bridge, and land.

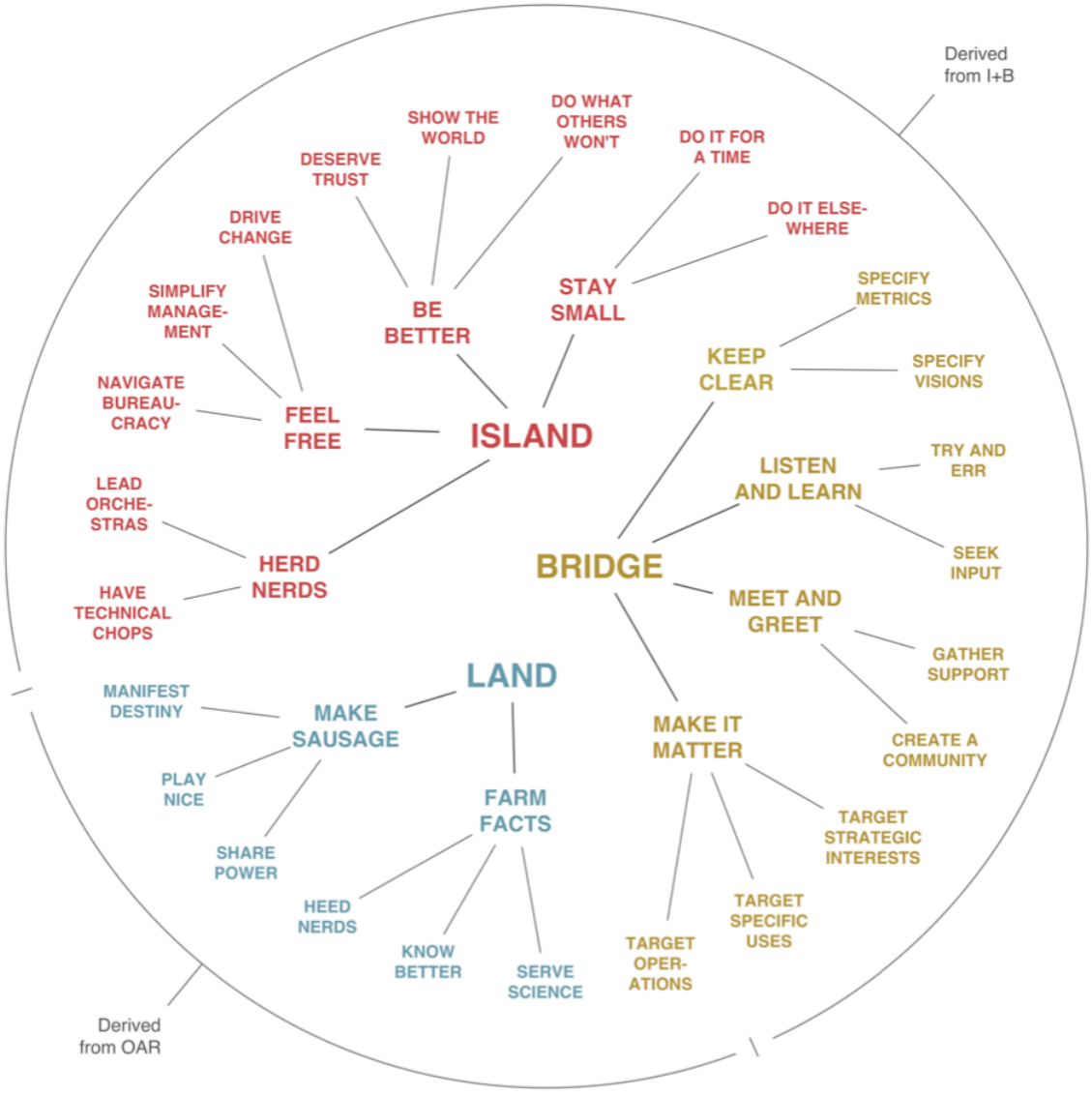
To facilitate the reader's comprehension and navigation in this section, I use specific formatting and notation. Heuristics take the form of short phrases and are identified by text in ALL CAPS. Each is described by a paragraph that references associated factors. Title headings for each paragraph use forward slashes to denote the relative position of each heuristic within the overall taxonomy, as is done with Internet addresses (e.g. '**ISLAND / STAY SMALL / DO IT ELSEWHERE**'). In each description, *italicized text* indicates a factor that emerged as important to OAR. Any quotes are taken directly from source material.

Table 10: Heuristics nested under **ISLAND**, **BRIDGE**, and **LAND**, with summary descriptions. Asterisks indicate Heuristics developed from factors important to NOAA.

ISLAND: Isolate activities from traditional contexts (both operations and basic research)
STAY SMALL: Limit the size and duration of projects and programs
DO IT ELSEWHERE: Fund activities externally, don't conduct them internally
DO IT FOR A TIME: Fund more activities temporarily, not fewer activities indefinitely
FEEL FREE: Avoid or cope with the bureaucracy of typical research organizations
DRIVE CHANGE: Critique the status quo, dream big, take action
SIMPLIFY MANAGEMENT: Flatten hierarchies, streamline processes, learn from business
NAVIGATE BUREAUCRACY: Accept necessary constraints, work smoothly within them
HERD NERDS: Focus the creative talent of scientists and engineers
HAVE TECHNICAL CHOPS: Have just enough knowledge of the science and engineering
LEAD ORCHESTRAS: Organize people and projects toward the big picture
BE BETTER: Embrace difference, attract excellence, stand out from the crowd
DESERVE TRUST: Empower managers who display creativity, fairness, and sound judgment
DO WHAT OTHERS WON'T: Take on challenges that most find too risky or even 'dumb'
SHOW THE WORLD: Demonstrate what is technically possible or cost-effective
BRIDGE: Connect activities to non-traditional contexts (both public and commercial applications)
MAKE IT MATTER: Direct activities to realize outcomes for society, not for science
TARGET STRATEGIC INTERESTS: Develop technologies/systems aimed at broad benefit
TARGET SPECIFIC USES: Tailor technologies/systems to public, commercial applications
TARGET OPERATIONS: Develop technologies/systems for agency's operational services*
MEET AND GREET: Socialize with stakeholders, engage in matters of business and policy
CREATE A COMMUNITY: Grow social networks to foster collaboration and generate ideas
GATHER SUPPORT: Forge relationships with research conductors and potential adversaries
KEEP CLEAR: Codify concrete results to negotiate terms and gauge success
SPECIFY VISIONS: Create tangible vision or challenge statements to direct work
SPECIFY METRICS: Define measurable results to determine when work is complete
LISTEN AND LEARN: Stay in a constant dialog with expertise and experience
SEEK INPUT: Rely on the knowledge and imaginations of potential partners
TRY AND ERR: Take diverse approaches, expect to fail, learn from failure
LAND: Maintain activities within traditional contexts (both operations and basic research)*
FARM FACTS: Cultivate scientific knowledge, understanding, and scientists themselves*
SERVE SCIENCE: Deliver data and information as a regular, reliable service*

KNOW BETTER:	Improve understanding of the world, assume that innovation follows*
HEED NERDS:	Follow the lead of scientists and investigate the interests of academia*
MAKE SAUSAGE:	Abide by the typical, bureaucratic machinations of the organization*
MANIFEST DESTINY:	Strive to grow or sustain capabilities, staff, and budget indefinitely*
SHARE POWER:	Distribute programmatic control and decision-making authority*
PLAY NICE:	Maintain agreeable relationships with 'frenemies', strive for consensus*

Figure 6: Heuristics on the **ISLAND**, the **BRIDGE**, and the **LAND** are organized as a taxonomy. Low-level heuristics (on the right) are more particular; each is defined by 3-12 underlying factors. High-level heuristics (in the middle) are more general; each is defined by 2-3 low-level heuristics. ISLAND, BRIDGE, and LAND are most abstract: metaphors defined by 2-4 high-level heuristics.



ISLAND

The ISLAND is a metaphor for aspects of research endeavors that isolate them from broader contexts. R&D is separated from operational concerns. The ISLAND is also a separation of R&D from other, more fundamental research concerns, as well as quasi-operational scientific services, both of which occur on LAND, (see below: SERVE SCIENCE and FARM FACTS). The ISLAND is more than a division in organizational structure; it signifies differentness and specialness in technical ability, personal attitude, and program administration. The ISLAND is not the open sea – there are limitations. Thus, a hallmark of the island is the ability of the manager, and the program as a whole, to be deliberately but delicately disruptive: transforming systems, while still working within a system of controls.

ISLAND / STAY SMALL

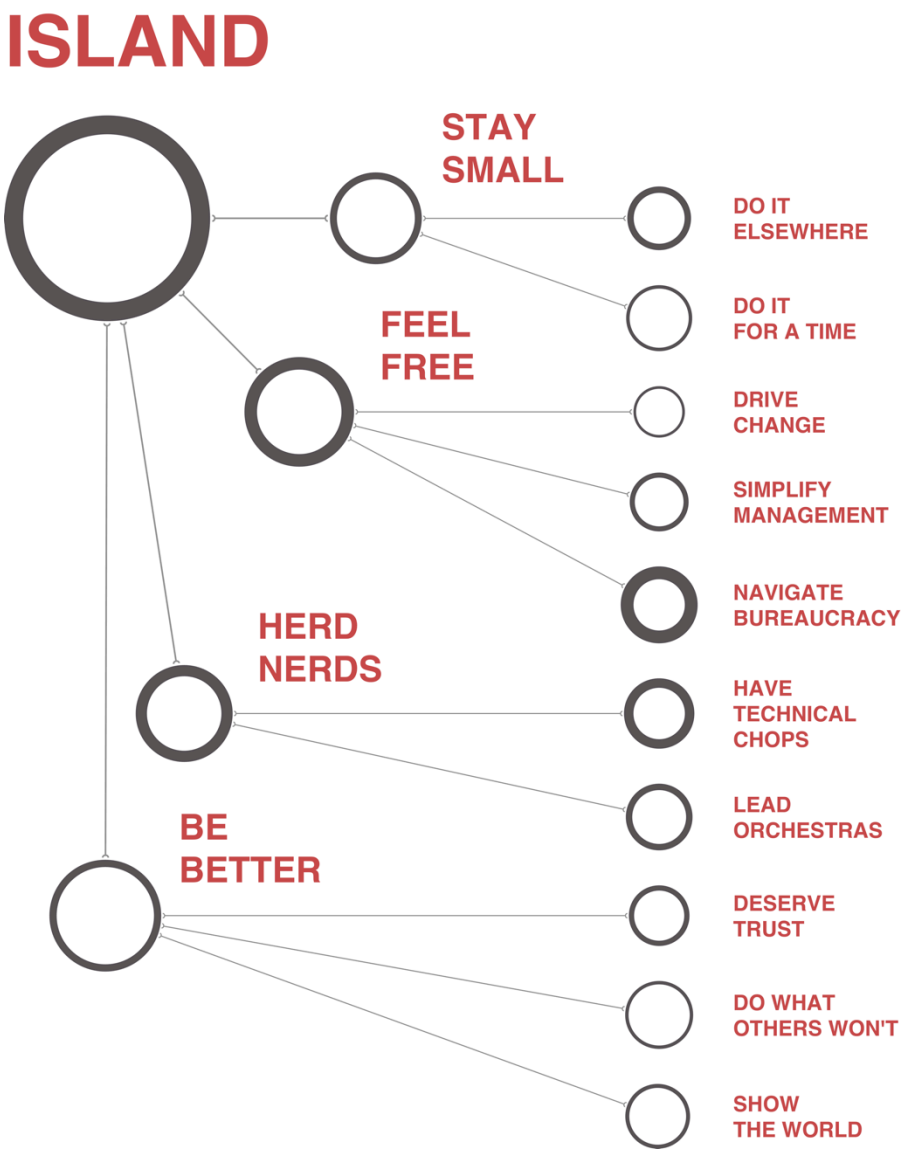
I+B agencies are organized to be lean and agile. Rather than running a few, big projects over long periods, they run hundreds of small projects for short periods. In contrast, OAR runs laboratories, employs bench scientists and managers as federal employees, and sustains projects for many years and often decades at a time. On the ISLAND, agencies avoid establishing fiefdoms that specialize in maintaining scientific capabilities and growing empires with 'base' resources. This is a much different organizational heuristic from that which guides OAR (see MANIFEST DESTINY below).

ISLAND / STAY SMALL / DO IT ELSEWHERE

Projects at I+B agencies are executed extramurally, either by universities or by private industry, including small, creative companies and joint ventures of two or more companies. Managers at these agencies value this as quite important. In the words of one, 'It doesn't tie you down. If you

see an opportunity to go on a tangent you can take it'. OAR does make extensive use of *cooperative agreements* to conduct research at universities and, therefore work is often conducted by *CI employees at universities*. Program offices also issue grants for research that is *executed at universities*. However, OAR's maintenance of a large in-house research capacity, in the form of labs, is an obvious difference from the I+B agencies.

Figure 7: Heuristic taxonomy for **ISLAND** as a treemap. White inner circle represents I+B, black outer ring represents OAR. Areas for both are proportional to excerpt values (percent per case). Low-level heuristics sum the values of associated factors. High-level heuristics sum values of those below. Values included for important factors at both I+B and OAR.



ISLAND / STAY SMALL / **DO IT FOR A TIME**

An important factor for I+B agencies is the temporary nature of projects, programs, and managers. DARPA and ARPA-E (though not ATP) hire temporary program managers through the Intergovernmental Personnel Act (IPA) and make use of Systems Engineering and Technical Assistance (SETA) contractors to staff managers. Regular turnover is not an important factor at OAR, where lab directors and program managers (and their staff) are federal employees. Many research projects at OAR are temporary, lasting *5 years or fewer*. *Project termination* is an important step in project life cycle at OAR, though not as important as at the I+B agencies. Managers at OAR may end a project if it is making *no progress and there is no 'Plan B'.* Managers at I+B agencies also noted they may stop a project for this reason, but also in other circumstances: when a challenge is met successfully or when time runs out.

ISLAND / **FEEL FREE**

I+B managers feel free to be different. They are organizationally separate from departmental science or service divisions. They are smaller, flatter, and less complex than other federal institutions and, where processes cannot be simplified further, managers work effectively to navigate within the government bureaucracy. There is little satisfaction with the status quo, either of technological capability or of existing institutions and managers are intent on using their programs to affect transformation. There is less a feeling of being free, however, at OAR. While similarly adept at using the machinery of the federal government to keep programs moving forward, managers at OAR do not thrive on change, and the practice of management is less important, and less simple.

ISLAND / FEEL FREE / **DRIVE CHANGE**

Program managers at I+B agencies (DARPA and ARPA-E, in particular) tend to be driven to change the status quo and are described as visionaries or dreamers. They often have 'fire in the belly' and a 'burning desire' to make an impact. The manager is an entrepreneur, finding the funding to get things done, but also realizing that it is 'not just about the technology' (see MAKE IT MATTER, below). This heuristic was notably absent at OAR. At DARPA and ARPA-E (more so than ATP), the environment of getting projects funded and executed was also characterized as brutal and fast-paced. There is a great deal of competition for funding – among programs, as well as among projects with different approaches within programs – combined with an acceptance that tough decisions must be made quickly. While there is also competition for finite resources at OAR, it is not as explicit (see PLAY NICE, below).

ISLAND / FEEL FREE / **SIMPLIFY MANAGEMENT**

From the perspective of those at the model agencies, sound and efficient management can be an enabler of breakthrough results, not an obstacle to it. An element of management efficiency that was important for both I+B and OAR is the use of *milestones and metrics* to track the progress of projects (see SPECIFIC METRICS, below). For both I+B and OAR, decisions about the allocation of resources (at the program level) happen *above the program manager*. A robust factor at I+B agencies is that bureaucratic processes are *streamlined*, but this was important only for OAR CIs. Managers at both I+B agencies and OAR reported that they worked within a *flat hierarchy*. At OAR this is specific to the unit level, not the OAR or NOAA level. OAR managers noted too much bureaucracy and troubles with leadership at headquarters (see SHARE POWER, below), while I+B managers described very regular and very positive interactions with the directors of their agencies. Many I+B managers studied management as a discipline, but few if any managers at OAR did so.

ISLAND / FEEL FREE / **NAVIGATE BUREACRACY**

It was clearly important for managers at both I+B and OAR to successfully navigate the bureaucracy: deftly working through the federal rules governing the administration of their programs. Managers at OAR recognize (as did their counterparts at ATP, in particular) that the bureaucracy within which they find themselves is *not particularly flexible*. (Those at OAR, however did note that processes of *contracting* and *hiring and firing of CI employees* were streamlined.) I+B and OAR managers have hands-on relationships with skilled *administrative staff*. Managers in both I+B agencies and OAR often serve the role of *problem solver or facilitator*. They have talent for *administration and bureaucracy* and draw upon their experience with *working in government*. In contrast to OAR, I+B managers tend to have experience in industry as well as in government. They also place importance on teamwork as a way to navigate the bureaucracy, whereas OAR managers apparently did not.

ISLAND / **HERD NERDS**

As research institutions, it is not surprising that both I+B agencies and OAR have a strong scientific culture. Managers in both groups have a passion for science and engineering, have extensive experience in conducting research, and can draw upon the curiosity and experimentalism of those in scientific fields. In both groups managers have the technical skills and the systems perspective to understand and guide the work of researchers. They see the big picture. Those at OAR tend to focus on the science, and maintaining a lineup of high quality scientific talent over the long term. Managers at the I+B agencies tend to take a more transdisciplinary approach that finds problems and attacks them on multiple fronts over the short- to mid-term.

ISLAND / HERD NERDS / **HAVE TECHNICAL CHOPS**

Baseline knowledge of the science or technology at hand is necessary (though not sufficient) for running a program for both I+B and OAR. Managers in both have typically *studied a STEM field* (science, technology, engineering, and mathematics), possess a graduate degree, and have also *worked in academia*. They have a *hands-on relationship with researchers*, and a universally important factor of managerial talent is *technical direction* of researchers. While managers at I+B agencies have the chops, they are 'still up 30000 feet' and 'don't get into the nitty-gritty'. At OAR, however, technical expertise is an important managerial talent (see HEED NERDS). OAR managers also noted a *hands-on relationship with university faculty* (who are not necessarily the ones performing the research). Interestingly, while I+B managers indicated that an important factor of their choice of projects is the *technical merit* of proposals, OAR managers did not.

ISLAND / HERD NERDS / **LEAD ORCHESTRAS**

An important factor of managerial talent for both I+B and OAR is *systems thinking*, and the ability to see 'how it all fits together'. A common notion among interviewees for the role of the manager is *orchestra leader or a movie producer,* combining the abilities of talented people. Managers in both cases often assume the role of *coach or mentor* and express the ability to *motivate creatives*, transferring their own sense of purpose to researchers. While both I+B and OAR managers discussed how they managed portfolios with *complementary technological thrusts*, only I+B managers (primarily those at DARPA) understood portfolios in terms of *systems versus single technologies*. Also, managers in I+B agencies reported their role as a *translator* between the fields of science and application, whereas OAR managers did not. I+B managers have a breadth of expertise, including, but not limited to, scientific expertise. Their orchestration abilities are often concern 'how to get from point A to point B'.

ISLAND / **BE BETTER**

While OAR strives for excellence (in an absolute sense), the I+B agencies strive to be better (in a relative sense). I+B agencies create contrast, embrace difference, and are not for the modest or meek. They get ambitious people to do unique things in order to tackle significant problems. Managers at OAR are trusted and empowered to do great things as they are at I+B agencies, but not to the same degree. OAR also exists to show the rest of the world what can be made possible though research, but it does not intentionally focus on what others deem to be insurmountable problems. I+B agencies are characteristically bold and risky; OAR – while it is responsible for great scientific work – is neither.

ISLAND / BE BETTER / **DESERVE TRUST**

Managers at I+B agencies are given a great deal of 'control and accountability' and 'drive the entire process' of a program. For DARPA and ARPA-E, a manager's first task is to make a pitch to the agency director for what the program will accomplish and what approaches will be taken. As with these agencies, at OAR program offices (in particular) the *manager is empowered*, the *manager chooses projects* (though often along with others, see SHARE POWER, below) and the manager is trusted to *use judgment to allocate resources*. Another important factor at for I+B and all OAR units was that there should be *no micromanagement*. While the analysis revealed that all of the factors described here are definitely present at OAR, each is there is to a noticeably lesser degree than the I+B model (relative to total for factors coded in each case).

ISLAND / BE BETTER / **DO WHAT OTHERS WON'T**

OAR units conduct research activities for which they have established track records and competitive advantages. OAR managers noted that they take advantage of the *halo effect*, that is, they use their reputations to form partnerships and leverage resources. However, OAR does not

have, as do I+B agencies, a compulsion to take on challenges that others will not. Like their counterparts at I+B agencies, managers at OAR program offices (in particular) frequently discussed *selection with reviewers* as a step in the project life cycle. Yet selection criteria are different for I+B than at OAR. At I+B agencies, the key criterion for choosing projects is that no one else would do it, and that others would think it was 'a dumb idea'. ATP and ARPA-E apply both technical criteria and business application criteria to choose which proposals to fund. A common narrative among I+B managers is that choosing projects in this way may not always result in application, but it's worth it when it does. This type of narrative did not appear to be significant at OAR.

ISLAND / BE BETTER / **SHOW THE WORLD**

I+B agencies and OAR share a conception that their research will *show the world applications are possible* and also *show the world applications are cost effective*, not just possible. The signal for showing cost-efficacy was notably stronger in the I+B agencies than at OAR. Where OAR really differs from the I+B model in terms of showing the world, is in terms of risk. For I+B, choosing programs that are high risk is the factor with the largest total number of coded excerpts, but this factor was absent at OAR. I+B agencies exist to demonstrate, not refine technologies, but this is not the case at OAR. Moreover, whereas those at DARPA and ARPA-E (in particular) see their agencies role as 'throwing rocks' at their home departments from outside, those at OAR did not take such an antagonistic approach.

BRIDGE

The BRIDGE is a metaphor for aspects of research endeavors that connect them to broader contexts. R&D is integrated with real-world concerns. Applications – albeit fundamentally new and different applications – are important, as with the concepts of 'use-inspired research' (Stokes

1997) and 'disruptive innovation' (Christensen 2003). The BRIDGE is by its very nature a social space, inhabited by a diversity of specialists. Opportunities for communication are created and knowledge is exchanged between human beings through dialog. The hallmark of the BRIDGE is frank and frequent face time: regular, detailed conversations focusing on direction and performance and, specifically, the iterative negotiation of project visions and metrics.

BRIDGE / **MAKE IT MATTER**

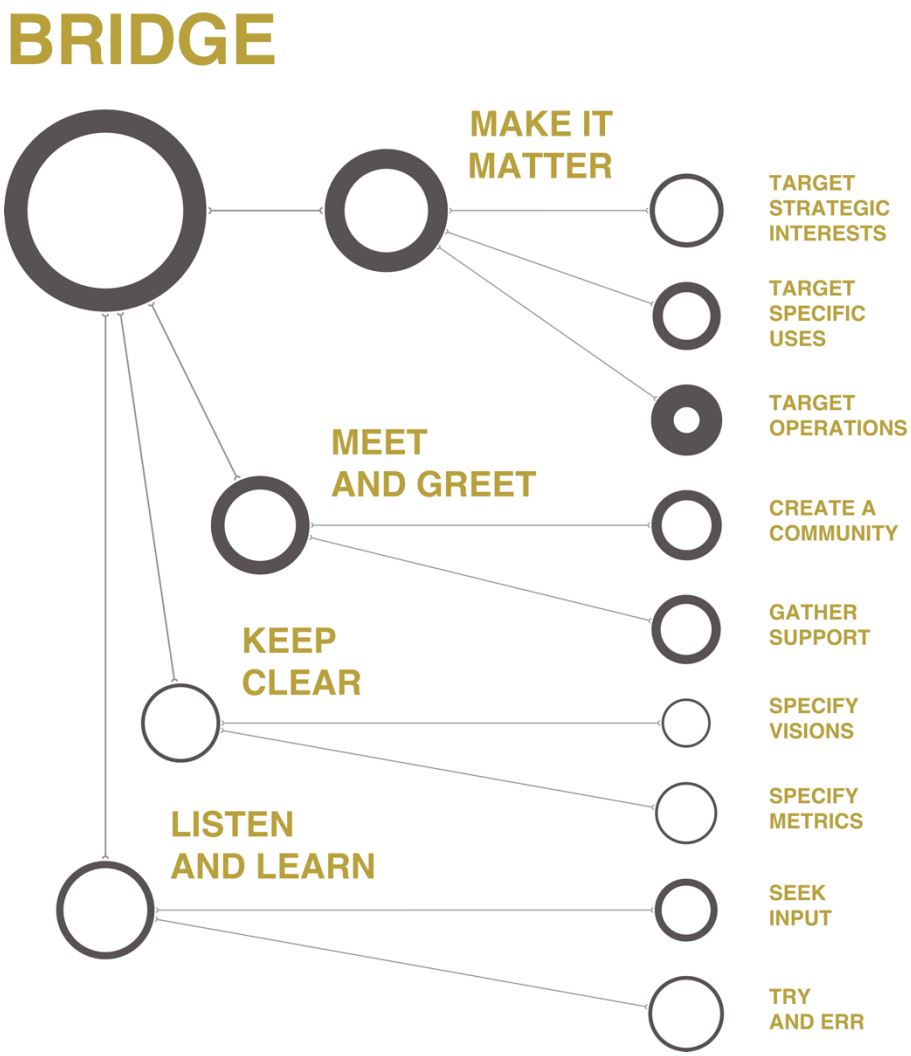
OAR certainly shares with I+B agencies a concern with outcomes for society. While not explicitly driven by broad social and economic benefits or commercial applications, OAR is driven by the more immediate needs of NOAA as a parent organization, that is, new or improved operational services at the National Weather Service, the National Marine Fisheries Service, the National Ocean Service, and the National Environmental Satellite Data and Information Service. Relationships with these sister line offices is complex; they are in many ways OAR's customer, but in other ways its competitor. Technology transfer is a large concern for OAR, but it does compete for attention with the more traditional roles of a science agency (see below: LAND / FARM FACTS).

BRIDGE / MAKE IT MATTER / **TARGET STRATEGIC INTERESTS**

As with I+B agencies, at OAR the important factors indicate that it drives toward strategic interests. Portfolios are defined with particular *technological applications or industries* in mind. In choosing projects, managers look for applications that show that the proposed research has a *plausible path* to implementation. Both of these factors, however, were somewhat less important at OAR than in I+B agencies, i.e. each accounted for a smaller proportion of coded excerpts at OAR than in the I+B agencies. While much of OAR's research is aimed at improving 'the science' (see below: LAND / FARM FACTS / KNOW BETTER), some does go beyond

fundamental understanding and *builds on basic research* to realize the potential of discoveries already made. During project selection, managers at I+B agencies (ATP and ARPA-E in particular) look for applicants to make a credible case that the research will have broad benefits**,** beyond those to the investigators and their institutions. Broad social and economic benefits did not show up as important criteria for managers choosing projects at OAR.

Figure 8: Heuristic taxonomy for **BRIDGE** as a treemap. White inner circle represents I+B, black outer ring represents OAR. Areas for both are proportional to excerpt values (percent per case). Low-level heuristics sum the values of associated factors. High-level heuristics sum values of those below. Values included for important factors at both I+B and OAR.



BRIDGE / MAKE IT MATTER / **TARGET SPECIFIC USES**

The final step of the project life cycle is to *transfer technology to adopters*. This phase was actually discussed much more at OAR than at the I+B agencies (as a proportion of coded excerpts). The way DARPA gets technologies adopted is though agreements with service divisions within the Department of Defense – some formal, others informal. Those at OAR also noted that making *agreements with operations* is the means of technology transfer. This factor, too, was more prevalent at OAR than I+B. OAR managers do not have a talent for commercialization of new technologies, nor do they have a dedicated commercialization team, as do their ATP and ARPA-E counterparts. OAR managers (particularly those at program offices) indicate that they *manage the business side* of projects like managers in I+B agencies. However, business outreach in OAR is less formal than at ARPA-E, which requires both a 'tech-to-market plan' and an 'intellectual property management plan,' or at ATP, where programs are led simultaneously by a technical manager and a business manager.

BRIDGE / MAKE IT MATTER / **TARGET OPERATIONS**

Targeting operations is unique to OAR. While similar to TARGET SPECIFIC USES, this heuristic focuses on operational services of NOAA, either current or prospective. Project choice at OAR is strongly based on *the needs of NOAA*. OAR managers often reported that they view performance in terms of *technologies applied*, usually in NOAA operations, and engage operational entities (particularly the National Weather Service) by *connecting to their stage gate* processes. OAR managers indicated that the *services of their home agency were a customer* (an ally in innovation), and also that the *services of their home agency were an incumbent* (a rival in innovation), which is a contradiction that is not unusual. Managers have *hands-on relationships with adopters*, and some are *talented at technology transfer*. Labs, specifically, engage the

private sector through *patents and licenses*, as well as *CRADAs*. Program offices, specifically, understand portfolios in terms of *technology creation versus diffusion*, and understand performance in a semi-operational way as *community engagement and learning*.

BRIDGE / **MEET AND GREET**

An essential part of I+B managers' job is to socialize. They need to create communities in order to realize their visions. OAR managers also connect frequently with peers and other experts, however, these peers tend to be partners in research domains, not so much in industry or in operational services at NOAA (or elsewhere in government). The focus of meeting and greeting is not, generally, for the purpose of creating commercial or public sector applications. As with I+B managers, OAR managers also engage stakeholders to gather support, though here too, support comes from the research community. In OAR, support is gathered much less, if at all, from industry, from Congress, or from NOAA leadership.

BRIDGE / MEET AND GREET / **CREATE COMMUNITY**

As with I+B agencies, OAR managers endeavor to *build communities* externally. They bring people together through meetings, seminars, and workshops, and thereby grow informal social networks to foster collaboration. Managers often see their role as a *liaison, ambassador, or promoter* and recognize the ability to *communicate well* as a necessary managerial talent. For I+B agencies, external engagement is well balanced between academia, private companies, and government agencies. At OAR, community building is centered largely on academia, though *finding adopters in government* (largely at NOAA service lines) and *attending industry consortia* did show up as important factors.

BRIDGE / MEET AND GREET / **GATHER SUPPORT**

I+B agencies have no permanent staff or infrastructure, so they depend on the capabilities of partners. While not entirely dependent on others, OAR does also emphasize gathering support from stakeholders. Managers at OAR (particularly Lab directors) indicated that their work involves *partners in other agencies*, *partners in universities*, and partners that are *international equivalents*, i.e. science centers in other countries. OAR managers did not, as I+B managers did, indicate partnerships with industry or with other science components in their home agency (e.g. research centers in the National Marine Fisheries Service); in fact, there is a strong signal that other *science components at the agency are competitors* for funding. OAR also differs from I+B in that managers did not mention agency leadership or Congress as an advocate. Neither does OAR evaluate economic impact as a means of assuaging political critics (as did ATP).

BRIDGE / **KEEP CLEAR**

This is an area of significant difference between OAR and the I+B model. While OAR managers use some metrics to understand performance, it is not to the same degree as in I+B agencies. Metrics are not decided upon up-front, to guide projects, nor are they used extensively over the course of projects to gauge their progress toward ultimate success. Similarly, visions of what ultimate success means are not tangible and specific. Whereas crystal clear communication of concrete results – both imagined and actual – is essential to project and portfolio development in the I+B agencies, it does not appear to be essential at OAR. This makes sense, however, if we understand OAR as the provider of fundamental research, which has, by definition, less direction.

BRIDGE / KEEP CLEAR / **SPECIFY VISIONS**

Specifying visions is not an active heuristic at OAR. Managers in I+B agencies, on the other hand, define success up front with specific visions, or, as one manager called them, ‘big hairy

challenges'. Visions focus on problems, not solutions. They might depict specific technologies or specific applications of technologies. Examples from DARPA include 'an unmanned aircraft that can loiter for months or years' and 'a fully functional prosthetic limb directly controlled by the brain'. Program level visions are negotiated with agency leadership (at DARPA and APRA-E), and project level visions are negotiated with principle investigators (at all I+B agencies). While there were a few coded excerpts from OAR managers that fall within this cluster, none of the factors discussed here emerged as important.

BRIDGE / KEEP CLEAR / **SPECIFY METRICS**

As with visions, OAR is also less rigorous about establishing specific metrics and milestones. OAR lab directors (in particular) indicated that they use *technical metrics*, and program office managers (in particular) indicated that they use *commercialization metrics*, but for both of these factors (as well as for other types of metrics) OAR managers are much less concerned with performance monitoring than managers in I+B agencies. I+B agencies use metrics to define key *go or no-go decision points*. 'Otherwise,' said one I+B manager, 'how do you know when you're done'? Specific metrics need not only apply to outcomes; ARPA-E managers were also keen to use leveraged funds, i.e. cost sharing with awardees, as a measure of support and confidence.

BRIDGE / **LISTEN AND LEARN**

Conversation and evaluation are critical for ensuring that ideas do not get marooned on the ISLAND. This heuristic is closely related to BRIDGE / KEEP CLEAR, above. Clarity of vision and performance are the foundations for effective dialog with experts and experience. Whereas I+B agencies have a well-established culture of learning from failure, even encouraging it, OAR does not. Listening to the input of stakeholders – to frame specific problems and generate potential solutions – is common at OAR program offices, but not at labs or CIs.

BRIDGE / LISTEN AND LEARN / **SEEK INPUT**

Managers in I+B agencies rely on the imaginations of potential partners and, similarly, this is the case with OAR, though almost exclusively with program offices. While managers at labs and program offices in OAR both clearly *seek external advice* to guide their research efforts, managers of program offices will often conduct *deep dives with the community by* holding workshops that could define projects, or solicit *concepts from the community* by calling for papers. They make *announcements to the community* to submit proposals, and then *receive proposals*. A notable contrast from OAR – even OAR program offices – is that managers at I+B agencies choose proposals on the criterion that they make a coherent, credible case that the funding will make a difference.

BRIDGE / LISTEN AND LEARN / **TRY AND ERR**

OAR does not put the same emphasis on trial and error that we find in the I+B model. Failure is expected at I+B agencies, as is learning from failure. It is part and parcel of choosing to do things that no one else will. I+B managers choose projects that take many different *approaches* to solving a single problem, thereby hedging risk. The learning process at I+B depends on '*active management*,' in which the manager has frequent, face-to-face interaction with researchers to discuss project progress. I+B managers negotiate measures and milestones with researchers at the beginning of a project, and often renegotiate them over the life of the project as results come in. While this heuristic is not strong at OAR, some supporting factors are present – particularly at CIs. There are *regular reviews with performers* as projects progress, however these reviews are less frequent than at I+B agencies. For example, APRA-E has quarterly milestones and check-ins). Reviews at OAR do not rely as heavily on specific performance metrics as those in I+B agencies (see above: BRIDGE / KEEP CLEAR / SPECIFY METRICS). To the extent that

project-defining challenges are stated, the OAR manager and researcher work together to frame the problem: *the manager scopes it, but the performer proposes it*.

LAND

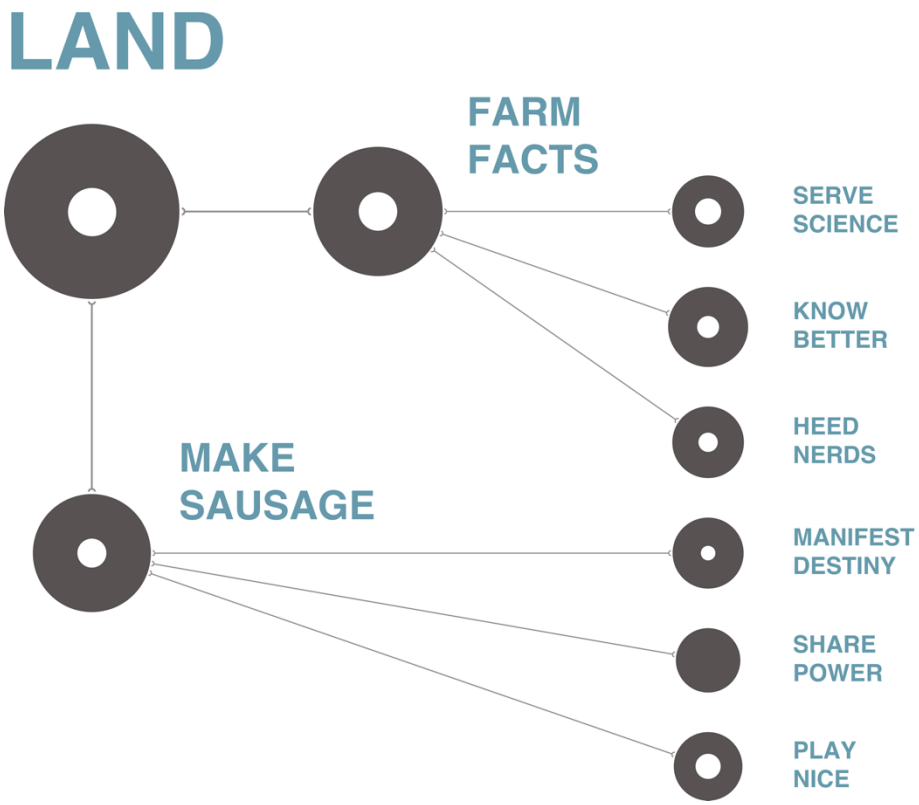
All heuristics on LAND were unique to OAR. Extending the island and bridge metaphor, the LAND is a place of normality – of business as usual. Of course, this means something different in the business of science than in other, non-scientific occupations. Nevertheless, managers are operating on the land when they are concerned with everyday affairs such as satisfying the demands of known customers, growing (or maintaining) permanent capabilities and staff, and vying for resources to do these things within a network of other LAND-dwellers. The LAND represents the more typical concerns and constraints of the bureaucracy that the I+B agencies were designed to get away from, including well-established interests of federal laboratories and government-sponsored science more broadly. While not antithetical to innovation, the LAND is not a place of transformation. It is a place where people strive for stability, rather than change.

LAND / FARM FACTS

OAR is a responsible caretaker of the land of science. OAR has adopted the traditional role of a science agency laid out in *Science: The Endless Frontier,* i.e. it 'creates the fund from which the practical applications of knowledge must be drawn' (Bush 1945, p. 13). Per Dietz and Rogers (2012) metaphors of transformative research, the two operative metaphors for OAR managers appear to be 'evolution' and 'the frontier'. The evolution metaphor takes the perspective of the steward who tries to guarantee the diversity of the science and technology ecosystem by increasing variation – the Johnny Appleseed of innovation. The frontier metaphor evokes the view of the explorer, who creates new possibilities by expanding the scope of the known – the Daniel Boone of innovation. The FARM FACTS heuristic centers on the (implicit) value

proposition is that stakeholders require facts about the environment that only OAR can provide; it does so by cultivating scientific data, information, and knowledge, as well as scientists themselves. Research at OAR is the 'seed corn' from which future applications may sprout.

Figure 9: Heuristic taxonomy for **LAND** as a treemap. White inner circle represents I+B, black outer ring represents OAR. Areas for both are proportional to excerpt values (percent per case). Low-level heuristics sum the values of associated factors. High-level heuristics sum values of those below. Values included for important factors at both I+B and OAR.



LAND / FARM FACTS / **SERVE SCIENCE**

OAR's activities focus heavily on providing scientific information as a regular, quasi-operational service. Labs, program offices, and even CIs often *sustain data services*, including environmental observations (such as greenhouse gas concentrations in the atmosphere) as well as the output of models in the form of predictions or projections (such as air quality predictions). Portfolios are often defined in terms of observations or other research, *projects definition is fuzzy*

because many projects are information services that are actually like small programs: *ongoing and multi-decadal*. Science services have *customers in other agencies* and *non-governmental organizations* and, hence, the activities *result in application more often than not*. A few other factors reinforce this science-as-service approach: managers often stay on in their positions for *more than five years*, they sometimes use *stage gates to track performance*, and many consider their portfolios to be *low rather than high risk*.

LAND / FARM FACTS / **KNOW BETTER**

Many other activities at OAR focus on improving understanding of the natural world. This is not, as many managers at OAR are clear to point out, science for the sake of science; the understanding created (for instance, how hurricane intensity is attributable to climate change) is central to NOAA's mission. However, research to KNOW BETTER aims to generate new knowledge rather than capitalize on it. The primary approach to value creation is to *show the world what they have learned*. Portfolios are defined by *science themes* as well as by exploratory *'seed' projects versus established research* efforts. Projects are chosen such that they fit *within science themes* and meet the *needs of the scientific community*. Projects are executed in house by *OAR Lab researchers* and success is measured by *papers published* as well as *citations or use of data*. Many managers noted that adopters of their research results *approach them* (not vice versa), or that they do *nothing intentional* to aid adoption. For some, *science is application*.

LAND / FARM FACTS / **HEED NERDS**

On the ISLAND, the organizational heuristic is to HERD NERDS; on the Land it is to HEED NERDS. Here, the research scientist leads the way, rather than following the lead of managers. At OAR, it is *the principal investigator who is empowered*; he or she often *chooses projects* and sometimes engages with the manager to *direct the portfolio*, particularly in the process of

submitting proposals externally for competitive (non-appropriated) funds. Managers often understand their portfolios as *science versus technology*, frequently with the latter in the service of the former (e.g. engineering buoys as platforms for data collection). Managers predominantly see their role as that of *lead scientist* (sometimes in explicit contrast to 'manager,' per se) and see their talents in terms of *technical expertise* (as distinct from *technical direction* at the I+B agencies.) A common strategy across OAR labs, program offices, and CIs is to *get ideas from academia*, which appears to underlie the belief that innovation comes from the minds of creative individuals at universities.

LAND / **MAKE SAUSAGE**

The most typical part of the Land is not the stewardship of science, but the more routine, competitive, and – perhaps, from the point of view of the scientist – less exciting work of politics, policy and administration. No matter how good the idea, nor how just the cause, projects cannot be executed without managers attending to the internal machinations of the organization. While the I+B agencies were created to avoid burdensome bureaucracy wherever possible, the OAR labs, program offices, and even CIs were not. The bureaucracy is a part of them, and they are securely a part of it. This is not inherently a negative thing; however, it does point to organizational values of growth, consensus, and temperance.

LAND / MAKE SAUSAGE / **MANIFEST DESTINY**

Compared to those at the I+B agencies, OAR managers are very concerned about securing resources, both financial and human, in order to grow (or prevent the shrinkage of) their unit and its capabilities. The modus operandi of Labs and CIs, specifically, is to *recruit and develop research talent* (including *CI employees to work at OAR Labs*), then *get the money to the talent*. Correspondingly, the major steps in the execution of projects are *scouting and hiring talent*, then

picking projects and portfolios (i.e. distribution of resources). CIs and program offices often work to *reduce competition* to keep favored research efforts stable over multiple years. Those at OAR frequently commented on how they were *under-resourced*, and very commonly understood their portfolios in terms of *where funding comes from* (Congress, competitive grants, redirects from other OAR labs and program offices, etc.) Managers at I+B agencies did not raise these factors at all.

LAND / MAKE SAUSAGE / **SHARE POWER**

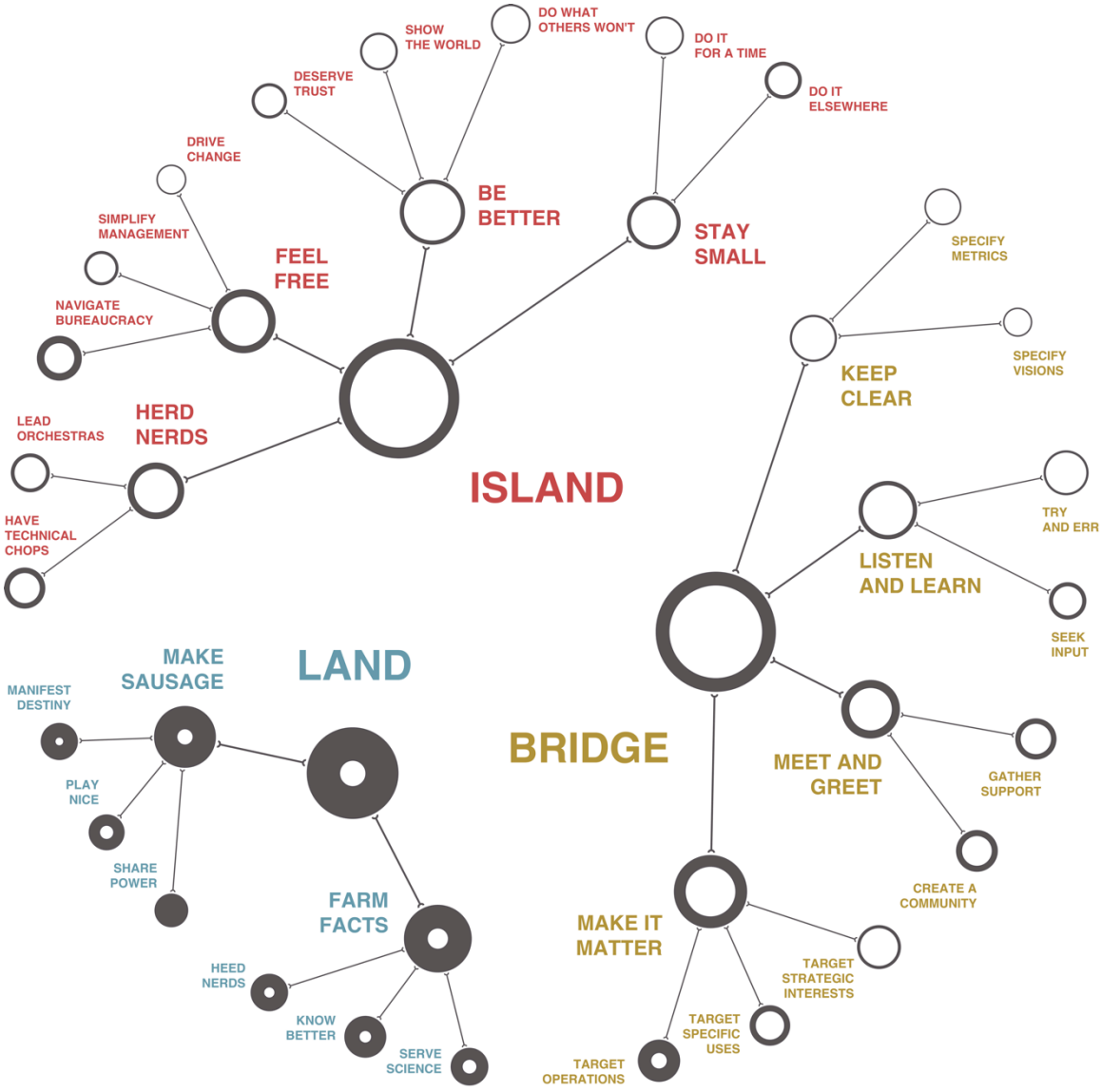
Programmatic control and decision-making authority in the OAR system appears quite distributed. This is notably the case at labs and CIs, less so at program offices. The directors of labs and especially the CIs expressed that they were *not empowered as managers*. *Project choices are made with other units,* and *portfolio allocation is done with other units* – these factors are particularly present at CIs, but also at some labs and program offices. Many managers express that there is *too much bureaucracy at the OAR and NOAA levels*, above the level of the manager, and *troubles with the leadership* at these levels, centering on incoherent management. Below the level of the manager, project choices are often *made with sub-unit managers*. The *projects that are conducted are those that others will fund* (beyond what was appropriated to the unit), and *the challenges that drive these projects are those determined by the sponsor*, not the manager.

LAND / MAKE SAUSAGE / **PLAY NICE**

Because power is distributed, managers at OAR work to maintain agreeable relationships with a cadre of counterparts who may or may not share their goals. Non-specificity of goals may be a tactic for arriving at consensus. Many at OAR remarked that they rely on a *network of personal relationships* within OAR in order to get things done. Not surprisingly, lab directors (but also CI

directors, to a lesser degree) have *hands-on relationships with lab division chiefs*. OAR research projects frequently involve labs and CIs in active *partnership with program offices*, and program offices and CIs in active *partnership with labs*. It is important that, when engaging in these and other partnerships, managers *be fair and gain trust*, as well as *add not subtract* from the relationship, that is, by acknowledging comparative advantages in particular research functions. Noting the 'feudal' nature of the OAR system, a general rule of engagement is to *keep friends close, but 'frenemies' closer*. Lab directors, specifically, used metaphors such as *sailor, shepherd, or orthodontist* to describe their role as applying slow, light pressure to direct staff and partners to take on certain types of work.

Figure 10: Heuristic taxonomy for **ISLAND**, **BRIDGE**, and **LAND** as a treemap. White inner circle represents I+B, black outer ring represents OAR. Areas for both are proportional to excerpt values (percent per case). Low-level heuristics sum the values of associated factors. High-level heuristics sum values of those below. Values included for important factors at both I+B and OAR.



Discussion

NOAA Research, as a whole, is not organized like DARPA and its analogs. Within OAR, heuristics on the LAND are more active than those on the ISLAND or on the BRIDGE. The organizational commitment to scientific products, in particular, creates the potential for benefits, rather than realize the potential of benefits. As such, we should not expect transformative innovation from NOAA Research. There may be other models of how to organize for innovation that is transformative, and NOAA Research may be close to those models. However, with respect to the island+bridge model, the framework used for this analysis, NOAA Research does not – as a whole – display an organization for transformative innovation similar to I+B agencies, although some of OAR's units get closer to the model, as described below.

OAR certainly has the desire and the capacity for taking ideas and inventions over the finish line, that is, for innovation. It is strongly evident from the drive to TARGET OPERATIONS – a BRIDGE-based heuristic unique to OAR – but also present to a modest degree in all of the other heuristics on the ISLAND and BRIDGE. OAR managers often understand success as the successful application of technologies at NOAA's operational service lines. OAR units develop agreements for adoption with NOAA service lines and connect mature research to the stage gate processes used by these partners. They patent and license the technologies that they create, and forge Cooperative Research and Development Agreements (CRADAs) with private firms. OAR is willing and able to CREATE A COMMUNITY and GATHER SUPPORT, though these efforts focus on the academic community. Managers at OAR certainly have HAVE TECHNICAL CHOPS and are quite able to NAVIGATE THE BUREACRACY.

Thus, while OAR is not an I+B agency, it is not entirely unlike them either. It does possess a solid foundation for building toward the island+bridge model of transformative innovation.

OAR's faculty for transformative innovation rests, to some extent, on the balance of effort across the functions of science and engineering, creation and diffusion. Future studies could examine the distribution of resources across these elements of the OAR portfolio. In other ways, however, the cultural and historical differences between OAR and I+B agencies that cut deeper into the core of these organizations than resource distribution.

Clearly, OAR differs from I+B is in the relative permanence of its management, staff, infrastructure, and functions. As evidenced by research at labs and CIs, in particular, there is much less tendency at OAR to DO IT ESLEWHERE or DO IT FOR A TIME, compared with I+B agencies. There are no time limits for managers, programs, or projects. In fact, the opposite is true: successful programs are those that MANIFEST DESTINY by surviving and growing over time. The life cycle of a project is slower-paced – possibly never-ending – and less 'brutal'. Program managers do not play the role of, as one ATP manager put it, 'the Terminator'. Performance metrics, because they are often loosely defined, cannot be used as the basis for killing some projects in the interest of starting new, potentially more successful ones.

There is also no established tradition to engage in activities that are high-risk-high-reward (Hughes, Balakrishnan, Shipp, & Lal 2011). OAR's research enterprise does not encourage those within it to take chances – to DO WHAT OTHERS WON'T and SHOW THE WORLD that it can be done. Few at OAR cited 'high-risk' as a factor of project choice and some feel that OAR discourages risky projects. Those at OAR are not motivated to 'throw rocks' at incumbents, but rather align with the interests of others as they PLAY NICE and SHARE POWER. In directing programs, OAR managers apply slow pressure and exercise indirect control with researchers, colleagues, leaders at headquarters, and Congressional appropriators. It is not surprising, then, that there is less explicit and less risky portfolio management; unit budgets often keep historical ratios across portfolio categories, rather than more radical changes in allocation.

Intimately related to taking on risk and realizing rewards is the capacity to SPECIFY VISIONS, to SPECIFY METRICS. The organization and management of NOAA Research do not tend to emulate these heuristics. Clarity of vision and monitoring progress are what make I+B projects risky, rather than just speculative. They focus on a challenge of a specific technology or application; accordingly, it is clear to see if those challenges are met or not. Challenges at OAR tend to be less specific challenges of scientific understanding, which are more difficult to judge as successes or failures. (Even if hypotheses are not supported, scientific research always results in understanding.) Taking risks and tracking progress underlies the capacity to TRY AND ERR and, therefore, to learn from failure.

Many OAR managers are intimately involved with the science that they direct, but they do not display the same sort of 'active management'. There is much less frank, frequent face time between managers and investigators on project progress. They do not negotiate metrics, milestones, and schedules with investigators prior to project execution. Reviews with investigators are less regular reviews and involve fewer or less specific metrics. At least part of the reason for this less active management seems to be that the primary mechanism for giving (or getting) research funding is the grant, and the only expectation that comes with a grant is showing what has been learned scientifically by publishing papers, posting data sets on websites, as well as promoting and interpreting research findings. I+B agencies, however, structure management around the cooperative agreement, a funding mechanism that codifies specific results that the awardee agrees to aim for, but does not promise to achieve. As such, it is more directed than a grant, and more flexible than a contract.

Management at OAR seems less disciplined than at the I+B agencies. OAR doesn't HERD NERDS; it HEEDS NERDS. Managers tend to be the 'scientist who happens to be in charge' and

commonly believe that 'the best thing you can do is hire the best people, give them the resources, and get the hell out of the way'. OAR managers are not, as I+B managers tend to be, educated in and passionate about management disciplines, nor do they have as much experience in private industry. It makes sense, then, that they are less likely to drive change themselves, in their capacity as manager, and less likely to commercialize new technology. We can also understand why they might be better prepared to NAVIGATE BUREACRACY than to SIMPLIFY MANAGEMENT when it becomes too complex. Prioritizing scientific skills over management skills doe not only happen at the level of the manager, given the troubles some interviewees expressed about OAR's senior management, including weak leadership, shifting priorities, no big picture thinking, and poor advocacy.

Overall, the heuristics that are active at OAR are consistent with what others have identified as 'myths' of science policy and innovation. The first myth, identified by the Science Policy Assessment and Research on Climate (SPARC), is that more knowledge is always better. However, as the SPARC handbook on *Usable Science* reminds us, 'sometimes we have adequate knowledge to address a problem, and additional research may not be the best approach.' (SPARC 2010, p. 3). OAR's primarily intent seems to be to KNOW BETTER and SERVE SCIENCE, with TARGET OPERATIONS as a secondary, or, at least, a somewhat disconnected intent. For much of OAR's work, it doe not seem that any amount of knowledge will be adequate. It explains why there is much such emphasis on scientific research compared to technological development and, within scientific research, such emphasis on collecting data and building predictive models compared to determining the needs and behaviors of potential adopters.

Then there is the 'Moses myth' (Liedtka, King, & Bennet 2013) that that innovation takes a special gift that most managers do not have. 'Innovation is the miracle that results when a special person raises his or her hands to the heavens and the Red Sea parts, or the iPod is born' (Liedtka,

2013). This is evident from the managers' emphasis (most often at labs and CIs) on getting research ideas by engaging with academics, recruiting the early career scientific talent who (ostensibly) generate the good ideas, and occupying the role of the 'lead scientist' who is dispassionate – even skeptical – about the disciplines of management and administration. That OAR is more concerned with cultivating the seeds of innovation than on developing applications reveals a strong cultural norm: innovation is a miracle, and the faithful shall be vindicated when serendipity strikes.

It should be clear, however, that OAR is not a monolith; there are differences among the three avenues that it uses to conduct R&D. In general, program offices at OAR operate more similarly to the island+bridge model. Program office managers, in general, feel more empowered than labs or CIs. Compared to labs and CIs, program offices tend to STAY SMALL. They fund, rather than conduct research, which means that they can take a portfolio approach and diversify approaches. They maintain relatively little staff and infrastructure, so they can be agile. With fewer people and less in-house expertise, they must SEEK INPUT from stakeholders. Moreover, program offices are particularly concerned with transition-related factors, especially those under TARGET OPERATIONS. Managers of OAR program offices often regarded the creation of science and technology and their diffusion to downstream users as important activities. The most important way to evaluate success, for a few program office managers, is in terms of outcomes for ultimate stakeholders, specifically community engagement and learning.

Two programs in particular, strongly emphasize the connection between researchers and local communities in a shared experience of learning and practice. The first is the Climate Program Office's division of Climate and Societal Interactions, which maintains the Regional Integrated Service and Assessment (RISA) initiative. RISA takes an interdisciplinary, multi-stakeholder approach to developing climate impacts assessments and decision support for regional and local

interests (Pulwarty, Simpson, & Nierenberg 2009). The RISA manager considered the program itself to be an ongoing experiment in the interaction of science and policy institutions, thus emphasizing the TRY AND ERR heuristic. In this respect, RISA represents a ripe area for further study of how federal programs organize and manage innovation.

The second is the National Sea Grant College Program, which funds a large network of universities in the interest of innovation for the sustainable development of ocean- and coast-based economies. Sea Grant recognizes extension as a critical component of research activities. Freitag has provides an account of the knowledge co-production process at work at a Sea Grant program that focuses on fisheries management in North Carolina (2013). The Sea Grant network encompassing over 300 separate institutions and could easily justify a separate assessment. Gathering data the organization and management of individual institutions was not feasible for this investigation, but could be the basis for future work.

This study has focused on OAR as NOAA's central research division, yet there are also R&D efforts funded and conducted by NOAA's service lines, some of which also look like island+bridge. Matso has recently examined science for decision-making at the National Estuarine Research Reserve System, part of NOAA's National Ocean Service (2012). There are a host of other research efforts run by NOAA operational lines, spanning topics of weather, fisheries, coastal ecosystems, and satellite-based remote sensing. Are any of these closer to the island+bridge model than OAR? While these efforts are certainly under the authority of divisions focused on operations, not research and development, future studies could reveal that they share similarities with I+B agencies, and operate more like the island+bridge model than OAR.

Implications

This assessment of NOAA Research rests on several key assumptions. It is assumes that transformation means innovation, and so requires both research and transition to be complete. It is assumes that organization and management are significant determinants of innovation. It assumes that the overall organizational and managerial style of DARPA and its analogs is a time-tested approach for generating transformations through research. Finally, it is assumes that NOAA may wish to engender more transformation though its central research division. It does not assume, however, that large-scale cultural change is a prescription that any organization can realistically fill. Accordingly, while I offer a number specific considerations for policy and practice, non-specific calls for 'culture change' are not among them.

In the language of the heuristics presented here, a general consideration is to farm fewer facts and make more matter. OAR as a whole, and its constituent units, could shift the balance from activities of exploration to those of exploitation. They could manage with less faith in serendipity and more strategy for the here-and-now. They could aim to reap the rewards of scientific capital already created by integrating it into larger networks of professional expertise. Within its portfolio, NOAA Research could direct efforts to development and dissemination of new technologies and new technological systems that improve how NOAA conducts its business, particularly though its service lines. In order to truly transform, the agency might prioritize R&D that targets operational improvements rather than serving scientific information or striving to know better about the Earth System. More specific policy considerations are offered below. Each is phrased as an distinction that NOAA's leadership should address if they attempt to emulate the I+B model at their agency.

1. Program offices, versus laboratories and cooperative institutes.

NOAA should consider the relative advantages of the different organizational models being used at OAR. Labs and CIs have a comparative advantage in science, and have relatively large institutional momentum. OAR program offices, on the other hand, are organized and managed most similarly to the I+B agencies; they have no dedicated research staff or facilities, but fund the temporary research projects conducted by others. They can stay nimble and focus more on enabling new applications, rather than maintaining current ones. Of the OAR managers interviewed, those at program offices felt most empowered and were most concerned with societal outcomes and the diffusion of knowledge and technology. NOAA should consider focusing on program offices if it wishes to move closer to the island+bridge model. Accordingly, NOAA might use programs to generate innovation, not generate science – a function better suited for Labs and CIs. Rather than maintain single projects over decades, programs could take advantage of their comparative advantage to conduct and finish multiple projects over the course of a few years, and take different approaches to solving problems.

2. Programs in a special unit, versus programs in a science division.

Building on the first option, NOAA could consider placing program offices more directly under the control of the NOAA Administrator, either through the creation of new programs, or by pulling some (or all) program offices out of OAR. In the same way that the DARPA director reports to the Secretary of Defense, and the ARPA-E director reports to the Secretary of Energy, the NOAA Administrator could appoint a director of an 'Advanced Research Project Agency for Ocean and Atmosphere', or 'OA-ARPA'.) This would clarify the role of OAR at NOAA as one of science (primarily concerned with understanding), and the role of 'OA-ARPA' as one of innovation (primarily concerned with application). It would afford program offices greater liberty to 'throw rocks' at both the service lines and the laboratories, and position those

incumbents more clearly as rivals than as nebulous 'frenemies'. It may, however, encounter resistance from those who believe OAR is stronger (read: more successful in the budget process) with its current components together. Many inside and outside of OAR did not agree with the attempt to reorganize OAR to create a NOAA climate service for just this reason.

3. Negotiated cooperative agreements, versus open-ended grants.

Program offices largely issue awards as grants, which are not conditioned by demonstrated progress. In addition, OAR (and NOAA more broadly) makes regular use of the same tool that I+B agencies employ to encourage risk: the cooperative agreement. However, OAR uses these agreements to create semi-permanent institutions to fund science at universities, rather than as multi-annual experiments to link technologies to applications. Cooperative institutes, while providing a useful supplement to labs in the farming of facts, are not directed to produce specific innovations. Instead, program offices could use cooperative agreements as the touchstone of more active management: a multi-year agreement with explicit challenges, negotiated measures and milestones, frequent check-ins on both technical progress and potential applications. Managers at ATP noted that they created incentives for the NIST laboratories to participate in competitions. Similarly, competitions at NOAA could be arranged in phases, with the first phase set aside exclusively to NOAA labs and CIs including, but not limited to OAR); competition could then be opened up broadly for those challenges that could not be addressed internally to the line office.

4. Excited temporary managers, versus expert permanent scientists.

Managers as I+B agencies have 'fire in their belly' and are driven to change the world. They defy the 'Moses myth' of innovation by being creative as managers – not as scientists – in the creation, communication, and realization of visions. They are passionate about particular

problems, not particular solutions, talented as specialists, but more talented as generalists. While they have backgrounds in science, they frequently have backgrounds in managerial disciplines. Their skills in science are superseded by their skills in coordinating the work of scientists. They create social networks that span government, academia, and industry. NOAA Research should consider the benefits of seeking creative, excited, and skilled managerial talent. Maintaining quality management can be a challenge for federal agencies (Clark 2013). Several at ARPA-E noted, the best managers are not usually motivated to peruse long, stable careers in government. A DARPA manager noted that most at his agency are either winding up a career, or winding one down, rarely in the middle. As such, NOAA should explore the use of flexible hiring mechanism, e.g. intergovernmental personnel assignments (IPAs), to bring managers on more quickly, and off more regularly. IPAs offer a streamlined way to bring on quality talent, opportunities to refresh direction, and a means of transferring the knowledge that managers gain out into the broader community as they move on to their next position.

5. Tackling risk though specificity, versus assuming risk though ambiguity.

Before, during, and often after a project is executed, I+B agencies seek to uncover the sources and degrees of risk and reward. They are 'high-risk-high-reward' not because the risks and rewards are assumed to be high by virtue of their uncertainty, but because they are known to be high by virtue of careful attention. NOAA Research could focus more explicitly on the risks and rewards; rather than writing them off as unknowable, they could scrutinize them further. Risks and rewards are, of course, unknown, but not unknowable. The codification of specific metrics and visions is, what makes research risky, but also, ironically, what brings the reward closer to fruition. An important aspect of the managerial talent called for above is the analytical skill to develop discrete challenges statements to focus work, along with performance indicators that can be tracked, and changed as necessary, in order learn from experience.

Conclusion

We should not undervalue OAR's essential public service of producing scientific information and scientific understanding, but we must distinguish it from an equally important role of government to affect change more directly. Similarly, we should acknowledge that the quests for fundamental understanding and ultimate application can overlap, but we must recognize them as distinct. There is a difference – and an inherent tension – between staying small and manifesting destiny; between herding nerds and heeding them; between showing the world and playing nice; between knowing better and driving change.

NOAA Research has a long history of providing the nation with the oceanic and atmospheric science that it needs, but would not otherwise have been produced. The Global Monitoring Division of the Earth System Research Laboratory, for example, has collected data on atmospheric composition around the world for decades and monitors carbon dioxide for the US Global Change Research Program. It is indispensible for understanding climate change. In a different way, a program like RISA is also critical for facing the same global threat by increasing society's capacity to respond to it. State and local governments need not only data about problems, but also the wherewithal to take action.

These examples epitomize two different contributions made by NOAA's central research division. This study does not suggest which of these two contributions is, ultimately, more important to national interests. However, if the goal is transformative innovation, efforts of the latter type are closer to the island+bridge model and, hence, more likely to deliver actual social, economic, and environmental impact. If more transformative innovation is the goal of agency leaders, then the considerations detailed here can serve as a starting point for discussion.

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Conclusion

Advocates of transformative research seem to use the term from the perspective of the linear model of innovation. The desire for 'ground-breaking' or 'paradigm-shifting' research stems from a continued focus on what is usually called fundamental or basic research. Research such as this occupies an important place in society, but such statements of importance are not new, and do not directly address the oft-stated concern for sustainable economic growth. While transformative research may yield the potential for transformation, it does not necessarily yield actual transformation. While we cannot know, a priori, the scale or scope of outcomes that will ultimately result from a given R&D effort, we can identify markers that signal a higher propensity for actual transformation.

Transformative innovation, in contrast to transformative research, is relatively easy to identify. However, what distinguishes it has less to do with science and more to do with management. Transformative innovation is *organized to realize concrete visions of change beyond the research community*. It aims for radical change in broad social and economic welfare, not fundamental scientific understanding. It is driven toward a clear vision of a discrete outcome, not discovery of the unknown and unexpected. It understands potential as the opportunity for value that can only

be realized by organizing diverse talent, not as a warning that value may or may not be realized by someone else.

Adopting such a stance inherently means being more strategically minded and more politically engaged than is typically the case for federal R&D programs. It means choosing programs based on which problems they aim to solve, how well they define those problems, and how well they are organized to solve them, not based on which discipline or science theme is emerging or popular. It also means improving the way programs are managed, not (necessarily) increasing their budgets. The 'why' and the 'how' are more important criteria for innovation policy choices than the 'where' or the 'how much'. Transformative federal R&D cannot occur without organizations that embody transformation. To paraphrase Gandhi, transformative innovation programs must be the change they want to see in the world.

This dissertation next explored the organizational factors at play in transformative innovation, as realized by three agencies designed to create it. It took a qualitative approach to grounding an understanding of transformative innovation in the experience of managers at DARPA, ARPA-E, and ATP. The result is a descriptive and illustrative account of organizational heuristics – managerial rules of thumb – at play at these agencies. The island+bridge model is a nested typology of heuristics. It offers a general account of factors concerning organizational structure of these agencies at the institutional level, as well as the management of programs and projects at a personal level. Individually, the heuristics are simple – concepts that probably underlie the decisions at many organizations. Taken together, however, they reveal a gestalt of organization for innovation for transformation.

I have used the island+bridge model as a framework for assessing the transformative capacity of NOAA Research. NOAA Research has a long history of successfully providing the nation with

the oceanic and atmospheric science that it needs, and that would not otherwise have been produced. However, with respect to the island+bridge model, NOAA Research does not – as a whole – display the organization for transformative innovation. In general, program offices– rather than labs and CIs – are closer to the island+bridge. Assuming that NOAA would like to adopt this model to engender more transformation, it should consider the comparative advantages of: program offices, versus laboratories and cooperative institutes; programs in a special unit, versus programs in a science division; negotiated cooperative agreements, versus open-ended grants; excited temporary managers, versus expert permanent scientists; and tackling risks though specificity, versus assuming risk though ambiguity.

The island+bridge model could be applied and improved by using it as a framework to study other organizations. I would speculate to say that, like NOAA, most science-based mission agencies probably focus more on science than on innovation, that is, they operate more according to LAND-based heuristics than ones on ISLAND or BRDIGE. More broadly, the island+bridge model could be used to investigate the organization of R&D efforts at agencies in other countries (the governments of Japan and Korea seem particularly interested) and at firms in the private sector (I noted in Paper 3 that Google and Proctor & Gamble use an approach that seems similar). The similarities and differences between the model I+B agencies studied here and those of international and private organizations would be interesting to see, and might reveal idiosyncrasies that stem from national and corporate cultures. The model is not intended, however, for assessment of institutions whose missions are limited to basic research, e.g. the National Science Foundation. It is intended, to reiterate a distinction made earlier, to gauge the organizational propensity for transformative innovation, not transformative research.

The arguments made and suggestions offered in the dissertation rely heavily on a distinction between science versus innovation, or, as Stokes (1997) put it, between concern for fundamental

understanding and concern for ultimate application. I take it as given that at a mission agency, even the R&D component of a mission agency, the latter concern takes precedence. Moreover, I take is as given that transformation implies an ultimate application and that, to develop new applications, fundamental understanding is not sufficient and often not necessary. Some may retort that science and innovation can overlap, which is true, but just because they can doesn't mean that they do. Consistent with Stokes's *Pasteur's Quadrant*, fundamental understanding may certainly be part of the quest for transformation, either by design or by chance, but it need not be. Some innovation is not scientific, some science is not innovative, and it is important to know the difference.

Though I have focused on innovation rather than science, I have been careful in these three papers not to make categorical statements about the inherent relative values of fundamental understanding and ultimate application. A large challenge to more rational policy choice is that the value of science is poorly understood. Federal research efforts are rarely evaluated in terms of the public values used to justify them (Bozeman & Sarewitz 2011). We ought to recognize that science provides us with more than improved utility; there is surely tremendous non-monetary value in, for instance (using NOAA-relevant examples), understanding why weather systems evolve the way they do, knowing that certain whale species are not extinct, and being inspired by the natural complexity of the global climate system. These things may not be 'totally radical', but they are quite literally awesome. The understanding and inspiration that the sciences provide are inherently valuable. It is disingenuous, however, to use innovation as a rhetorical device to advocate for science that may, in fact, be aimed at creating value in a different, less measurable way. That helps neither the cause of science nor innovation.

At NOAA, as with other science-based mission agencies, science and innovation do overlap when the desired outcome of science is – as it often is – improved personal and political decisions.

However, it is simply unwise to maintain historical investments into improving the accuracy and precision of scientific knowledge if what will affect decisions more is improving how that knowledge is communicated, interpreted, and applied. This appears to be the case with NOAA efforts in weather and climate, where the challenge is not quality of information but quality of decisions (Maricle 2011; Meyer 2011). If our goal is innovation in the interest of more informative science, then we also need better knowledge of the marginal value of the factors of scientific production.

To continue with the weather and climate science example, we could ask which link in the value chain has a higher marginal benefit: improvements in observations to gather data, in modeling environmental processes, in combining data and models to make predictions, in publishing the results of those predictions, or in supporting the decisions of those who access those predictions? What are the marginal costs of improvements in each of these areas? Evaluations of this sort would provide insight into where to direct efforts to innovate, and how to define success in terms of cost efficacy. I strongly suspect that compared to observing, modeling, and predicting, the next taxpayer dollar is better spent on innovation in communication and decision support, and that this will require managers to HERD NERDS from the social sciences and humanities as much as the natural sciences. It may also (as I discuss below) require managers to have the 'technical chops' in these disciplines themselves.

If NOAA were to set an explicit strategy for transformative innovation, there are a number of particularly vexing challenges facing the agency that could be the focus of work. One opportunity is the use of social sciences and design disciplines to improve the delivery of environmental information to the public and improve how individuals interpret and apply that information to their decisions. Predictive accuracy of weather and climate models has improved markedly over the years, but the ability of end users to apply it has lagged (the often-criticized

response of the Federal Emergency Management Agency's to forecasts of Hurricane Katrina is a notable example).

Other big challenges facing NOAA concern the cost and availability of environmental observations. NOAA faces the increasing cost of satellite-based observing systems for environmental data collection. A transformative innovation effort at NOAA could seek to reduce the cost-to-performance ratio though the development of non-traditional observation systems. Further, it could seek to better manage and utilize the vast and diverse data sets that NOAA already collects, or exploit other, non-traditional sources of 'big data' collected by others (e.g., time- and geo-stamped photos of clouds publically posted on Facebook.) Another direction is in developing technology for in-situ ocean observations and monitoring. The deep ocean offers resources of food, energy, minerals, and genetic material, but much of the ocean remains unexplored. New sensors and submersible platforms could help us understand what resources are available, and how to manage them sustainably.

With the benefits of such a transformative innovation effort at NOAA in mind, such an effort would not be without its consequences – particularly if it were a separate 'OA-ARPA' unit, per the second consideration listed above. There would likely be resistance to the unit from within OAR, just as DARPA, ATP, and ARPA-E faced resistance from their own departments' science divisions, and as there was within OAR when NOAA Administrator Lubchenco attempted to create a operational climate service by repurposing large components of OAR, including the Climate Program Office. The remainder of OAR would have been smaller, in budgetary terms, and some perceived the (ultimately unsuccessful) reorganization as a signal of weakness or regression of science. Therefore, NOAA might want to favor the creation of new programs and the reassignment of qualified managers for an OA-ARPA, rather than the wholesale re-organization of existing program offices. It would also want to learn from the experience of the

model agencies: design into the operating model a role for the science agencies (such as proposal review), and actively encourage and facilitate laboratory involvement on projects, possibly as partners with academic or private sector organizations.

In addition to internal resistance, a NOAA effort at transformative innovation could also face external resistance, given the political controversies that surround environmental policy. Bonvillian notes that agencies created for innovation must 'ensure in program design that there will be a noteworthy political interest constituency that will support the new agency before the executive and legislative branches on a continuing basis' (2011, p. 312). Efforts should clearly link to a politically recognized mission area (such as national defense in the case of DARPA) and aligning with the gaps, comparative advantages, and established interests of potential rivals in industry, in Congress, and in other agencies in the federal system. NOAA could, therefore, survey the political and economic landscapes, develop options, and proceed in the way that best suits its own unique situation.

Given its recent failure to get Congressional approval to establish a climate service, NOAA would do well not to brand a transformative innovation effort as a response to the challenge of climate change. Instead, NOAA might consider ensuring that the portfolio of projects spans all of its four strategic goals for climate, weather, oceans and coasts. The hypothetical OA-ARPA could, in fact, serve to better link the mission of NOAA to its parent organization, the Department of Commerce, by focusing on economic benefits of R&D to solve (rather than further characterize) known environmental problems. The example challenges noted above have measurable, economically-relevant outcomes in terms of the growth of new, high-tech industries as well as the reduction of escalating operational costs in government.

This dissertation has developed and applied a model that is undeniably abstract. However, in the course of this study, what became most fascinating were those things that were most concrete and most visceral. In particular, I was intrigued by the personality traits of all of those I had the pleasure to interview. While I did query on managerial talent and role, and while I did get some fantastic responses (ranging from 'orthodontist' to 'the Terminator'), I could not code for things like attitude, confidence level, or sense of humor. Nor could I capture, exactly, the atmosphere at DARPA and ARPA-E headquarters, where several interviews took place, and how they are different from the atmosphere at NOAA headquarters. People dressed better and used more foul language. They worked in open office layouts, not cubicle farms. I believe that the personalities of the people and the place must play a part in the process of innovation. To continue the work started with this dissertation, I would attend to the more particular traits and skills of managers (and their administrative staff) as a way to address the mechanisms of organizational learning and group cognition.

Managers at island+bridge agencies seem to be what Martin (2007) and Brown (2009) call 'integrative thinkers', with a knack for transcending the specializations of others and resolving the conflict between exploration and exploitation. Cognitive psychology shows that humans have multiple types of intelligence, correlating with different modes of sensory perception and sections of the brain. It could be useful to characterize their personalities in terms of, for instance, the Myers-Briggs type indicator (Myers 1962), or Gardner's multiple intelligences (Gardner 1993). Do they 'judge' more than they 'perceive'? Is their intelligence more verbal or visual? Similarly, it would be useful to know what sorts of tools do they use to codify and transfer knowledge as they HERD NERDS and KEEP CLEAR. Carleton (2010) emphasized the value of visions to radical technology development at DARPA, but how, exactly, are visions effectively created and shared? Clarity of communication implies a mastery of media. Are managers of innovation good writers? Are they good speakers? Are they skillful at white-boarding during meetings? Answers

to questions like these could help organizations to seek and support the type of transdisciplinary abilities in their managerial and administrative staff that will serve as the foundation for transformation.

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Appendix:

Excerpt Data, per Coded Factor

(Sorted by Heuristic Cluster)

heuristic cluster			code for factor black: important at I+B and OAR brown: important only at I+B teal: important only at OAR grey: not important italics: only at OAR	excerpts as percent points, per case						totals		percent point difference from I+B				tests for OAR					tests for I+B model					
				DARPA	ARPA-E	ATP	OAR/L	OAR/P	OAR/C	I+B	OAR	OAR/L	OAR/P	OAR/C	OAR	Test 1: "Total is high."	Test 2: "High & highly consist-ent."	Test 3: "At least X in all cases."	Test 4: "At least X in one case."	OAR Tests Passed	Test 1: "Total is high."	Test 2: "High & highly consist-ent."	Test 3: "At least X in all cases."	Test 4: "At least X in one case."	I+B Tests Passed
Level 1	Level 2	Level 3		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	300.0%	0.48%	0.42%	0.47%		86	81	86	90	128	55	55	68	61	96
BRIDGE	KEEP CLEAR	SPECIFY METRICS	J.Performance-MetricsMilestones-Specific-Commercialization	0.0%	1.6%	1.0%	0.1%	0.4%	0.2%	2.6%	0.7%	0.8%	0.3%	0.5%	1.5%		1			1			1		2
BRIDGE	KEEP CLEAR	SPECIFY METRICS	J.Performance-MetricsMilestones-Specific-LeveragedFunds	0.0%	1.2%	0.6%	0.0%	0.2%	0.2%	1.8%	0.4%	0.6%	0.4%	0.2%	1.4%					0			1		1
BRIDGE	KEEP CLEAR	SPECIFY METRICS	J.Performance-MetricsMilestones-Specific-Technical	0.8%	1.6%	1.0%	0.9%	0.4%	0.5%	3.5%	1.8%	1.3%	0.8%	0.7%	1.7%	1	1	1	1	4	1	1	1	1	4
BRIDGE	KEEP CLEAR	SPECIFY METRICS	J.Performance-MetricsMilestones-Specific-X	1.7%	2.9%	1.0%	0.1%	0.2%	0.0%	5.5%	0.3%	1.7%	1.7%	1.8%	5.3%					0	1	1	1	1	4
BRIDGE	KEEP CLEAR	SPECIFY METRICS	J.Performance-StageGate-NotStandard-GoNoGo	0.4%	1.4%	0.6%	0.0%	0.0%	0.0%	2.5%	0.0%	0.8%	0.8%	0.8%	2.5%					0	1		1	1	3
BRIDGE	KEEP CLEAR	SPECIFY VISIONS	H.ChallengeVision-Specific-Application	0.8%	0.2%	1.0%	0.7%	0.2%	0.1%	2.0%	1.0%	0.5%	0.1%	0.1%	0.7%					0	1	1	1	1	4
BRIDGE	KEEP CLEAR	SPECIFY VISIONS	H.ChallengeVision-Specific-Technology	1.9%	0.6%	1.6%	0.2%	0.0%	0.0%	4.1%	0.2%	1.1%	1.4%	1.4%	3.9%					0	1	1	1	1	4
BRIDGE	KEEP CLEAR	SPECIFY VISIONS	H.ChallengeVision-Specific-X	1.5%	1.4%	0.6%	0.1%	0.4%	0.6%	3.5%	1.1%	1.1%	0.6%	0.7%	2.4%					0	1	1	1	1	4
BRIDGE	LISTEN AND LEARN	SEEK INPUT	F.ProjectChoice-CredibleCase	0.6%	1.4%	1.2%	0.0%	0.0%	0.0%	3.3%	0.0%	1.1%	1.1%	1.1%	3.3%					0	1	1	1	1	4
BRIDGE	LISTEN AND LEARN	SEEK INPUT	P.RulesOfEngagement-SeekExternalAdvice	0.6%	0.0%	0.4%	0.8%	1.6%	0.5%	1.0%	2.9%	0.5%	1.3%	0.1%	1.9%	1		1	1	3					0
BRIDGE	LISTEN AND LEARN	SEEK INPUT	U.Steps-04.Program-ConceptsFromCommunity	0.4%	1.0%	0.4%	0.0%	0.9%	0.2%	1.8%	1.2%	0.6%	0.3%	0.2%	0.7%			1		1		1	1	1	3
BRIDGE	LISTEN AND LEARN	SEEK INPUT	U.Steps-05.Project-AnnouncementToCommunity	0.8%	0.8%	0.4%	0.0%	1.5%	0.0%	2.1%	1.5%	0.7%	0.8%	0.7%	0.5%	1		1		2	1	1		1	3
BRIDGE	LISTEN AND LEARN	SEEK INPUT	U.Steps-06.Project-ReceiveProposalsFromCommunity	0.6%	0.6%	0.8%	0.0%	0.9%	0.0%	2.0%	0.9%	0.7%	0.2%	0.7%	1.1%			1		1	1	1		1	3
BRIDGE	LISTEN AND LEARN	SEEK INPUT	U.Steps-01.Program-DeepDiveWithCommunity	0.8%	0.8%	0.0%	0.0%	1.1%	0.1%	1.7%	1.2%	0.6%	0.5%	0.2%	0.5%			1		1					0
BRIDGE	LISTEN AND LEARN	TRY AND ERR	F.ProjectChoice-DiverseApproaches	1.9%	1.4%	0.6%	0.1%	0.0%	0.4%	3.9%	0.5%	1.2%	1.3%	0.5%	3.4%					0	1	1	1	1	4
BRIDGE	LISTEN AND LEARN	TRY AND ERR	H.ChallengeVisionWho-ManagerScopesPerformerProposes	1.3%	0.6%	0.8%	0.2%	0.2%	0.4%	2.7%	0.8%	0.7%	0.7%	0.5%	1.9%		1		1	2	1	1	1	1	4
BRIDGE	LISTEN AND LEARN	TRY AND ERR	I.ExecutionHow-ActiveManagement	1.1%	1.2%	1.2%	0.0%	0.4%	0.2%	3.5%	0.6%	1.2%	0.3%	0.9%	2.9%					0	1	1	1	1	4
BRIDGE	LISTEN AND LEARN	TRY AND ERR	I.ExecutionHow-LearnFromFailure	1.7%	0.6%	0.8%	0.0%	0.5%	0.0%	3.1%	0.5%	1.0%	0.5%	1.0%	2.5%					0	1	1	1	1	4
BRIDGE	LISTEN AND LEARN	TRY AND ERR	U.Steps-08.Project-NegotiationWithPerformer	0.4%	3.5%	1.4%	0.1%	0.5%	0.2%	5.3%	0.9%	1.7%	1.2%	1.5%	4.4%					0	1		1	1	3
BRIDGE	LISTEN AND LEARN	TRY AND ERR	U.Steps-09.Project-RegularReviewsWithPerformers	0.6%	1.6%	2.2%	0.5%	0.4%	1.4%	4.4%	2.2%	1.0%	1.1%		2.3%	1		1	1	3	1	1	1	1	4
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*C.ManagerTalent-TechnologyTransfer*	0.0%	0.0%	0.0%	0.3%	0.4%	0.1%	0.0%	0.8%	0.3%	0.4%		0.8%		1			1					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	D.HandsOnWith-Adopters	0.4%	0.0%	0.0%	0.6%	0.7%	0.4%	0.4%	1.7%	0.4%	0.6%	0.2%	1.2%	1	1		1	3					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*F.ProjectChoice-NeedsOfNOAA*	0.0%	0.0%	0.0%	1.4%	1.5%	1.2%	0.0%	4.0%	1.4%	1.5%	1.2%	4.0%	1	1	1	1	4					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*J.Performance-CommunityEngagmentLearning*	0.0%	0.0%	0.0%	0.0%	1.1%	0.1%	0.0%	1.2%		1.1%		1.2%				1	1					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*J.Performance-MetricsMilestones-Specific-TechnologyApplied*	0.0%	0.0%	0.0%	1.1%	0.0%	0.9%	0.0%	2.0%	1.1%		0.9%	2.0%	1			1	2					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	L.PortfolioDefinition-ByApproach-CreationOrDiffusion	0.0%	0.4%	0.0%	0.0%	1.8%	0.1%	0.4%	1.9%	0.4%	1.7%		1.5%	1			1	2					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	N.Allies-Customer-HomeAgency-Service	0.4%	0.4%	0.0%	0.8%	1.3%	0.9%	0.8%	2.9%	0.5%	1.0%	0.6%	2.1%	1	1	1	1	4					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	O.Rivals-Incumbant-HomeAgency-Service	1.1%	0.0%	0.0%	0.8%	0.5%	0.4%	1.1%	1.7%	0.4%	0.1%		0.5%	1	1	1	1	4					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*S.EngagePrivateSector-CRADAs*	0.0%	0.0%	0.0%	0.9%	0.0%	0.0%	0.0%	0.9%	0.9%			0.9%				1	1					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*S.EngagePrivateSector-PatentLicenseTechnologies*	0.0%	0.0%	0.0%	0.9%	0.2%	0.7%	0.0%	1.8%	0.9%		0.7%	1.8%	1			1	3					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	T.EngagePublicSector-ConnectToStageGate	0.4%	0.2%	0.0%	0.7%	0.7%	0.6%	0.6%	2.0%	0.5%	0.5%	0.4%	1.4%	1	1		1	3					0
BRIDGE	MAKE IT MATTER	TARGET OPERATIONS	*J.Performance-StageGate*	0.0%	0.0%	0.0%	0.2%	0.2%	0.2%	0.0%	0.7%	0.2%			0.2%		1		1	2					0
BRIDGE	MAKE IT MATTER	TARGET SPECIFIC USES	C.ManagerTalent-Commercialization	0.0%	0.2%	1.2%	0.0%	0.0%	0.1%	1.4%	0.1%	0.5%	0.5%	0.3%	1.3%					0			1		1
BRIDGE	MAKE IT MATTER	TARGET SPECIFIC USES	I.ExecutionHow-BusinessSideManaged	0.0%	1.4%	2.2%	0.3%	0.7%	0.4%	3.6%	1.4%	0.9%	0.5%	0.8%	2.2%		1		1	2	1		1		2
BRIDGE	MAKE IT MATTER	TARGET SPECIFIC USES	S.EngagePrivateSector-CommercializationTeam	0.2%	1.6%	0.0%	0.0%	0.0%	0.0%	1.9%	0.0%	0.6%	0.6%	0.6%	1.9%					0			1		1
BRIDGE	MAKE IT MATTER	TARGET SPECIFIC USES	T.EngagePublicSector-AgreementsWithOperations	1.1%	0.4%	0.2%	1.4%	0.9%	0.9%	1.7%	3.1%	0.8%	0.4%	0.3%	1.5%	1	1	1	1	4			1		1
BRIDGE	MAKE IT MATTER	TARGET SPECIFIC USES	U.Steps-11.Project-TransferToAdopters	1.1%	1.2%	0.4%	3.0%	2.2%	2.0%	2.7%	7.1%	2.1%	1.3%	1.1%	4.4%	1	1	1	1	4	1	1	1	1	4
BRIDGE	MAKE IT MATTER	TARGET STRATEGIC INTERESTS	F.ProjectChoice-BroadBenefit	1.1%	1.8%	2.6%	0.1%	0.5%	0.1%	5.5%	0.8%	1.7%	1.3%	1.7%	4.7%					0	1	1	1	1	4
BRIDGE	MAKE IT MATTER	TARGET STRATEGIC INTERESTS	F.ProjectChoice-BuildOnBasic	1.3%	0.8%	0.2%	0.3%	0.4%	0.2%	2.3%	1.0%	0.4%	0.4%	0.5%	1.3%		1		1	2	1		1		2
BRIDGE	MAKE IT MATTER	TARGET STRATEGIC INTERESTS	F.ProjectChoice-PlausiblePath	1.1%	2.7%	2.4%	0.9%	0.7%	0.6%	6.1%	2.3%	1.1%	1.3%	1.4%	3.8%	1	1	1	1	4	1	1	1	1	4
BRIDGE	MAKE IT MATTER	TARGET STRATEGIC INTERESTS	L.PortfolioDefinition-ByTechnology-IndustryApplication	2.3%	1.6%	2.2%	0.8%	1.1%	1.2%	6.1%	3.1%	1.2%	1.0%	0.8%	3.0%	1	1	1	1	4	1	1	1	1	4
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	P.RulesOfEngagement-BuildCommunities	1.7%	1.8%	1.6%	0.6%	1.3%	1.5%	5.1%	3.3%	1.1%	1.1%		1.6%	1	1	1	1	4	1	1	1	1	4
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	R.ApplicationHow-FindAdopters-Commercial	0.4%	1.2%	0.2%	0.2%	0.0%	0.6%	1.9%	0.8%	0.6%	0.6%		1.6%					0			1		1
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	R.ApplicationHow-FindAdopters-Government	0.2%	0.2%	0.4%	0.2%	0.4%	0.7%	0.8%	1.3%			0.4%	0.4%				1	1		1			1
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	R.ApplicationHow-FindAdopters-X	0.6%	0.2%	0.2%	0.2%	0.5%	0.0%	1.0%	0.8%	0.2%	0.2%		0.5%					0				1	1
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	R.ApplicationHow-TransitionThroughPeople	1.3%	0.0%	0.0%	0.3%	0.0%	0.1%	1.3%	0.5%	0.4%	0.1%		0.5%					0			1		1
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	S.EngagePrivateSector-IndustryConsortia	0.4%	0.4%	0.6%	0.6%	0.7%	0.2%	1.4%	1.5%	0.5%			0.5%	1	1		1	3		1		1	2
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	C.ManagerTalent-CommunicateWell	0.6%	0.0%	0.2%	0.6%	0.9%	0.0%	0.8%	1.5%	0.3%	0.6%	0.2%	0.5%	1		1		2					0
BRIDGE	MEET AND GREET	CREATE A COMMUNITY	*E.ManagerRole-LiaisonAmbassadorPromoter*	0.0%	0.0%	0.0%	0.6%	0.9%	0.9%	0.0%	2.3%	0.6%	0.9%	0.5%	2.3%	1	1	1	1	4					0
BRIDGE	MEET AND GREET	GATHER SUPPORT	N.Allies-Advocate-Congress	0.2%	0.6%	1.2%	0.1%	0.2%	0.0%	2.0%	0.3%	0.6%	0.5%	0.7%	1.7%					0	1		1	1	3
BRIDGE	MEET AND GREET	GATHER SUPPORT	N.Allies-Advocate-HomeAgency-Leadership	0.6%	0.8%	0.4%	0.1%	0.7%	0.2%	1.8%	1.1%	0.5%		0.1%	0.6%					0		1		1	2
BRIDGE	MEET AND GREET	GATHER SUPPORT	N.Allies-Partner-HomeAgency-Science	0.4%	0.4%	0.2%	0.0%	0.0%	0.2%	1.0%	0.2%	0.3%	0.2%		0.3%					0		1			1
BRIDGE	MEET AND GREET	GATHER SUPPORT	N.Allies-Partner-Industry-StartUpsOrVentureCapital	0.0%	0.0%	1.0%	0.2%	0.2%	0.1%	1.0%	0.5%						1			1					0
BRIDGE	MEET AND GREET	GATHER SUPPORT	N.Allies-Partner-Industry-X	0.2%	0.4%	0.8%	0.5%	0.5%	0.1%	1.4%	1.1%			0.1%						0				1	1
BRIDGE	MEET AND GREET	GATHER SUPPORT	*N.Allies-Partner-InternationalEquivalents*	0.0%	0.0%	0.0%	0.8%	0.5%	0.4%	0.0%	1.7%	0.8%	0.5%	0.3%	1.7%	1	1	1	1	4					0
BRIDGE	MEET AND GREET	GATHER SUPPORT	*N.Allies-Partner-OtherAgencies*	0.0%	0.0%	0.0%	0.8%	0.7%	0.5%	0.0%	2.0%	0.8%	0.7%	0.5%	2.0%	1	1	1	1	4					0
BRIDGE	MEET AND GREET	GATHER SUPPORT	*N.Allies-Partner-OtherResearchInstitutions*	0.0%	0.0%	0.0%	0.2%	0.2%	0.1%	0.0%	0.5%	0.2%			0.2%		1			1					0
BRIDGE	MEET AND GREET	GATHER SUPPORT	N.Allies-Partner-Universities	0.2%	0.2%	0.6%	0.7%	0.4%	1.0%	1.0%	2.0%	0.9%		0.7%	1.6%	1	1	1	1	4				1	1
BRIDGE	MEET AND GREET	GATHER SUPPORT	O.Rivals-Competitor-HomeAgency-Science	0.4%	0.8%	0.4%	0.3%	0.2%	0.0%	1.6%	0.5%	0.4%	0.4%	0.3%	1.1%					0		1		1	2
BRIDGE	MEET AND GREET	GATHER SUPPORT	O.Rivals-PoliticalCritic-Congress	0.2%	0.0%	1.4%	0.0%	0.0%	0.0%	1.6%	0.0%	0.5%	0.5%	0.5%	1.6%					0			1		1

BRIDGE	MEET AND GREET	GATHER SUPPORT	P.RulesOfEngagement-EvaluateSocioEconomicImpacts																							
ISLAND	BE BETTER	DESERVE TRUST	*F.ProjectChoiceWho-Manager*	0.0%	0.0%	0.0%	0.6%	1.1%	0.6%	0.0%	2.3%	0.6%	1.1%	0.6%	2.3%	1	1	1	1	4						0
ISLAND	BE BETTER	DESERVE TRUST	G.Bureaucracy-Empowered-Manager	3.0%	1.8%	0.4%	0.5%	1.3%	0.2%	5.2%	2.0%	1.3%	0.5%	1.5%	3.2%	1		1	1	3	1		1	1	3
ISLAND	BE BETTER	DESERVE TRUST	I.ExecutionHow-NoMicroManagement	0.4%	0.4%	0.6%	0.2%	0.2%	0.1%	1.4%	0.5%	0.2%	0.3%	0.2%	0.9%		1			1		1		1	2
ISLAND	BE BETTER	DESERVE TRUST	M.PortfolioAllocation-JudgementCall	0.8%	0.8%	0.6%	0.2%	0.4%	0.2%	2.3%	0.8%	0.5%	0.4%	0.5%	1.4%		1		1	2	1	1	1	1	4
ISLAND	BE BETTER	DESERVE TRUST	U.Steps-02.Program-PitchToDirector	1.7%	1.2%	0.2%	0.0%	0.0%	0.0%	3.1%	0.0%	1.0%	1.0%	1.0%	3.1%					0	1		1		2
ISLAND	BE BETTER	DO WHAT OTHERS WON'T	F.ProjectChoice-NoOneElse	2.1%	2.1%	1.6%	0.1%	0.0%	0.0%	5.7%	0.1%	1.8%	1.9%	1.9%	5.6%					0	1	1	1	1	4
ISLAND	BE BETTER	DO WHAT OTHERS WON'T	P.RulesOfEngagement-HaloEffect	0.8%	1.4%	1.2%	0.7%	0.7%	0.7%	3.5%	2.1%	0.5%	0.4%	0.2%	1.3%	1	1		1	3	1	1	1	1	4
ISLAND	BE BETTER	DO WHAT OTHERS WON'T	Q.ResultsInApplication-NotAlwaysButWorthIt	1.1%	0.4%	0.8%	0.1%	0.0%	0.2%	2.3%	0.4%	0.6%	0.8%	0.5%	1.9%					0	1	1	1	1	4
ISLAND	BE BETTER	DO WHAT OTHERS WON'T	U.Steps-07.Project-SelectionWithReviewers	0.8%	1.6%	3.9%	0.1%	1.1%	0.4%	6.4%	1.6%	2.0%	1.1%	1.8%	4.9%	1		1		2	1		1	1	3
ISLAND	BE BETTER	SHOW THE WORLD	F.ProjectChoice-HighRisk	2.1%	2.1%	3.0%	0.3%	0.0%	0.1%	7.1%	0.5%	2.0%	2.4%	2.3%	6.7%					0	1	1	1	1	4
ISLAND	BE BETTER	SHOW THE WORLD	K.Termination-DemonstrateNotRefine	0.6%	0.4%	0.2%	0.0%	0.0%	0.0%	1.2%	0.0%	0.4%	0.4%	0.4%	1.2%					0		1			1
ISLAND	BE BETTER	SHOW THE WORLD	P.RulesOfEngagement-ThrowRocksFromOutside	1.3%	0.6%	0.0%	0.2%	0.2%	0.0%	1.9%	0.4%	0.4%	0.4%	0.6%	1.5%					0			1		1
ISLAND	BE BETTER	SHOW THE WORLD	R.ApplicationHow-ShowTheWorld-WhatIsCostEffective	1.1%	1.2%	1.2%	0.7%	0.7%	0.4%	3.5%	1.8%	0.5%	0.4%	0.8%	1.7%	1	1		1	3	1	1	1	1	4
ISLAND	BE BETTER	SHOW THE WORLD	R.ApplicationHow-ShowTheWorld-WhatIsPossibleToDo	1.5%	0.0%	0.2%	0.7%	0.5%	0.7%	1.7%	2.0%					1	1		1	3			1		1
ISLAND	FEEL FREE	DRIVE CHANGE	C.ManagerTalent-DrivenToChange	1.3%	1.0%	0.0%	0.1%	0.4%	0.1%	2.3%	0.6%	0.7%	0.4%	0.6%	1.7%					0	1		1		2
ISLAND	FEEL FREE	DRIVE CHANGE	E.ManagerRole-Entrepreneur	1.1%	0.6%	0.6%	0.0%	0.5%	0.2%	2.3%	0.8%	0.8%	0.2%	0.5%	1.5%					0	1	1	1	1	4
ISLAND	FEEL FREE	DRIVE CHANGE	E.ManagerRole-VisionaryDreamer	1.3%	0.8%	0.0%	0.3%	0.7%	0.0%	2.1%	1.1%	0.4%	0.2%	0.7%	1.0%					0	1		1		2
ISLAND	FEEL FREE	DRIVE CHANGE	G.Bureaucracy-BrutalFast	2.5%	0.8%	0.2%	0.0%	0.0%	0.0%	3.6%	0.0%	1.2%	1.2%	1.2%	3.6%					0	1		1		2
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	B.ManagerWorked-inGovernment	0.8%	0.2%	0.8%	1.2%	1.1%	0.5%	1.8%	2.8%	0.6%	0.5%	0.2%	1.3%	1	1	1	1	4		1			1
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	B.ManagerWorked-inIndustry	0.8%	0.4%	0.8%	0.3%	0.4%	0.0%	2.0%	0.7%	0.3%	0.3%	0.7%	1.3%					0	1	1		1	3
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	C.ManagerTalent-AdministrationBureaucracy	0.4%	0.2%	1.0%	0.5%	0.7%	1.1%	1.6%	2.3%	0.2%	0.2%	0.5%	0.9%	1	1	1	1	4			1		1
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	D.HandsOnWith-AdminStaff	0.4%	1.0%	0.4%	0.6%	0.7%	0.9%	1.8%	2.2%					1	1	1	1	4		1	1	1	3
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	E.ManagerRole-ProblemSolverFacilitator	0.4%	0.2%	0.6%	0.6%	0.7%	0.7%	1.2%	2.0%	0.2%	0.2%	0.2%	0.6%	1	1		1	3		1			1
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	G.Bureaucracy-NotParticularlyFlexible	0.0%	0.0%	2.4%	1.1%	1.1%	0.9%	2.4%	3.1%	0.6%	0.2%	0.2%	0.9%	1	1	1	1	4	1		1		2
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	G.Bureaucracy-Streamlined-Contracting	0.4%	0.0%	0.0%	0.3%	0.5%	0.2%	0.4%	1.1%	0.4%	0.2%	0.2%	0.8%		1		1	2					0
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	*G.Bureaucracy-Streamlined-HiringFiring-CI*	0.0%	0.0%	0.0%	0.8%	0.0%	0.4%	0.0%	1.2%	0.8%	0.0%	0.2%	1.2%			1		1					0
ISLAND	FEEL FREE	NAVIGATE BUREAUCRACY	G.Bureaucracy-TeamWork	0.2%	0.4%	0.6%	0.1%	0.2%	0.0%	1.2%	0.3%	0.5%	0.2%	0.2%	0.9%					0		1		1	2
ISLAND	FEEL FREE	SIMPLIFY MANAGEMENT	B.ManagerStudied-Management	0.2%	0.2%	0.6%	0.0%	0.0%	0.0%	1.0%	0.0%	0.3%	0.3%	0.3%	1.0%					0				1	1
ISLAND	FEEL FREE	SIMPLIFY MANAGEMENT	G.Bureaucracy-FlatHierarchy	1.7%	0.8%	0.4%	0.3%	0.4%	0.2%	2.9%	1.0%	0.6%	0.6%	0.7%	2.0%		1		1	2	1		1	1	3
ISLAND	FEEL FREE	SIMPLIFY MANAGEMENT	G.Bureaucracy-Streamlined-X	1.7%	0.6%	0.6%	0.0%	0.0%	1.2%	2.9%	1.2%	1.0%	1.0%	0.7%	1.7%			1		1	1		1	1	3
ISLAND	FEEL FREE	SIMPLIFY MANAGEMENT	J.Performance-MetricsMilestones-X	0.0%	1.2%	0.4%	0.7%	0.4%	0.2%	1.6%	1.3%	0.2%	0.2%	0.2%	0.6%		1		1	2			1		1
ISLAND	FEEL FREE	SIMPLIFY MANAGEMENT	M.PortfolioAllocation-AbovePM	1.5%	0.6%	0.8%	0.2%	0.5%	1.0%	2.9%	1.8%	0.7%	0.4%	0.2%	1.1%	1		1	1	3	1	1	1	1	4
ISLAND	HERD NERDS	HAVE TECHNICAL CHOPS	B.ManagerStudied-STEM	0.8%	0.8%	0.8%	0.9%	0.9%	1.4%	2.5%	3.2%	0.2%	0.2%	0.2%	0.6%	1	1	1	1	4	1	1	1	1	4
ISLAND	HERD NERDS	HAVE TECHNICAL CHOPS	B.ManagerWorked-inAcademia	0.6%	0.6%	0.4%	0.8%	0.4%	1.2%	1.6%	2.4%	0.2%	0.2%	0.7%	1.0%	1		1	1	3		1		1	2
ISLAND	HERD NERDS	HAVE TECHNICAL CHOPS	C.ManagerTalent-TechnicalDirection	1.3%	1.2%	1.8%	0.9%	0.4%	0.7%	4.3%	2.0%	0.5%	1.1%	0.7%	2.3%	1	1	1	1	4	1	1	1	1	4
ISLAND	HERD NERDS	HAVE TECHNICAL CHOPS	D.HandsOnWith-PIs	0.8%	1.0%	0.6%	0.9%	0.5%	0.7%	2.5%	2.2%	0.4%	0.4%	0.2%	1.0%	1	1	1	1	4	1	1	1	1	4
ISLAND	HERD NERDS	HAVE TECHNICAL CHOPS	*D.HandsOnWith-University-Faculty*	0.0%	0.0%	0.0%	0.1%	0.4%	0.4%	0.0%	0.8%	0.4%	0.2%	0.2%	0.8%		1			1					0
ISLAND	HERD NERDS	HAVE TECHNICAL CHOPS	F.ProjectChoice-TechnicalMerit	0.2%	0.4%	1.2%	0.0%	0.2%	0.2%	1.8%	0.4%	0.6%	0.4%	0.2%	1.2%					0			1	1	2
ISLAND	HERD NERDS	LEAD ORCHESTRAS	C.ManagerTalent-MotivateCreatives	0.4%	0.4%	0.2%	0.6%	0.2%	0.4%	1.0%	1.1%	0.2%	0.2%	0.2%	0.6%		1		1	2		1			1
ISLAND	HERD NERDS	LEAD ORCHESTRAS	C.ManagerTalent-SystemsThinker	2.3%	1.6%	0.4%	0.8%	0.5%	0.4%	4.4%	1.7%	0.7%	0.9%	1.1%	2.6%	1	1	1	1	4	1		1	1	3
ISLAND	HERD NERDS	LEAD ORCHESTRAS	E.ManagerRole-CoachMentor	0.2%	0.2%	0.4%	0.3%	0.7%	0.5%	0.8%	1.6%	0.2%	0.2%	0.2%	0.6%	1	1		1	3		1			1
ISLAND	HERD NERDS	LEAD ORCHESTRAS	E.ManagerRole-ProducerOrchestrator	1.5%	0.2%	0.4%	0.5%	0.4%	0.2%	2.1%	1.1%	0.2%	0.2%	0.2%	0.6%		1		1	2	1		1	1	3
ISLAND	HERD NERDS	LEAD ORCHESTRAS	*E.ManagerRole-Translator*	0.2%	0.4%	0.4%	0.1%	0.4%	0.1%	1.0%	0.6%	0.2%	0.2%	0.2%	0.6%					0		1		1	2
ISLAND	HERD NERDS	LEAD ORCHESTRAS	L.PortfolioDefinition-ByTechnology-ComplementaryThusts	0.6%	0.8%	0.4%	0.2%	0.2%	0.2%	1.8%	0.7%	0.4%	0.4%	0.4%	1.2%		1		1	2		1		1	2
ISLAND	HERD NERDS	LEAD ORCHESTRAS	*L.PortfolioDefinition-ByTechnology-SingleOrSystem*	2.1%	0.6%	0.0%	0.7%	0.0%	0.0%	2.7%	0.7%	0.4%	0.9%	0.5%	2.0%					0	1		1		2
ISLAND	STAY SMALL	DO IT ELSEWHERE	I.ExecutionHow-CooperativeAgreements	0.0%	1.0%	2.6%	1.2%	0.9%	2.1%	3.6%	4.3%	1.1%	1.1%	0.5%	2.7%	1	1	1	1	4	1		1		2
ISLAND	STAY SMALL	DO IT ELSEWHERE	*I.ExecutionWho-NotHere-CIemployeesAtUniversities*	0.0%	0.0%	0.0%	0.0%	0.0%	0.9%	0.0%	0.9%			0.5%	0.5%			1		1					0
ISLAND	STAY SMALL	DO IT ELSEWHERE	I.ExecutionWho-NotHere-PrivateIndustry-JointVentures	0.0%	0.2%	1.2%	0.0%	0.0%	0.0%	1.4%	0.0%	0.5%	0.5%	0.4%	1.4%					0			1		1
ISLAND	STAY SMALL	DO IT ELSEWHERE	I.ExecutionWho-NotHere-PrivateIndustry-X	1.1%	0.4%	1.2%	0.0%	0.0%	0.0%	2.6%	0.0%	0.9%	0.9%	0.9%	2.6%					0	1	1	1	1	4
ISLAND	STAY SMALL	DO IT ELSEWHERE	I.ExecutionWho-NotHere-Universities	0.6%	1.0%	0.4%	0.0%	0.9%	0.2%	2.1%	1.2%	0.7%	0.2%	0.4%	0.9%			1		1	1	1	1	1	4
ISLAND	STAY SMALL	DO IT ELSEWHERE	I.ExecutionWho-NotHere-X	1.3%	0.0%	0.2%	0.0%	0.0%	0.0%	1.5%	0.0%	0.5%	0.5%	0.5%	1.5%					0			1		1
ISLAND	STAY SMALL	DO IT ELSEWHERE	S.EngagePrivateSector-FundCreativeCompanies	0.4%	0.6%	0.2%	0.3%	0.2%	0.2%	1.2%	0.8%	0.2%	0.2%	0.2%	0.6%		1		1	2		1			1
ISLAND	STAY SMALL	DO IT FOR A TIME	A.ManagerTenure-Temporary	0.8%	0.6%	0.2%	0.0%	0.0%	0.0%	1.7%	0.0%	0.6%	0.6%	0.6%	1.7%					0		1			1
ISLAND	STAY SMALL	DO IT FOR A TIME	D.HandsOnWith-ContractSETAs	0.2%	1.2%	0.0%	0.0%	0.2%	0.0%	1.4%	0.2%	0.5%	0.4%	0.4%	1.3%					0			1		1
ISLAND	STAY SMALL	DO IT FOR A TIME	K.Duration-5YearsOrLess	0.0%	0.0%	1.2%	0.6%	0.4%	0.5%	1.2%	1.4%	0.2%	0.2%	0.2%	0.6%		1		1	2			1		1
ISLAND	STAY SMALL	DO IT FOR A TIME	K.Termination-ChallengeMetSuccess	0.2%	0.2%	0.6%	0.2%	0.0%	0.0%	1.0%	0.2%	0.2%	0.2%	0.2%	0.6%					0				1	1
ISLAND	STAY SMALL	DO IT FOR A TIME	K.Termination-NoProgressNoPlanB	0.6%	0.6%	0.8%	0.1%	0.2%	0.1%	2.0%	0.4%	0.6%	0.5%	0.6%	1.6%		1			1	1	1		1	3
ISLAND	STAY SMALL	DO IT FOR A TIME	K.Termination-WhenTimeRunsOut	0.6%	0.6%	0.8%	0.0%	0.0%	0.0%	2.0%	0.0%	0.7%	0.7%	0.7%	2.0%					0	1	1		1	3
ISLAND	STAY SMALL	DO IT FOR A TIME	U.Steps-10.Project-Termination	1.9%	2.3%	3.2%	0.6%	0.7%	0.6%	7.3%	1.9%	1.9%	1.7%	1.8%	5.4%	1	1		1	3	1	1	1	1	4
LAND	FARM FACTS	HEED NERDS	C.ManagerTalent-TechnicalExpertise	0.8%	0.0%	0.6%	0.5%	0.7%	0.5%	1.4%	1.7%					1	1		1	3					0
LAND	FARM FACTS	HEED NERDS	*E.ManagerRole-LeadScientist*	0.0%	0.0%	0.0%	1.2%	0.4%	1.4%	0.0%	3.0%	1.2%	0.5%	1.4%	3.0%	1		1	1	3					0
LAND	FARM FACTS	HEED NERDS	*F.ProjectChoiceWho-PIs*	0.0%	0.0%	0.0%	1.5%	0.5%	2.1%	0.0%	4.1%	1.5%	0.5%	2.1%	4.1%	1		1	1	3					0
LAND	FARM FACTS	HEED NERDS	*G.Bureaucracy-Empowered-PIs*	0.0%	0.0%	0.0%	1.1%	0.5%	0.5%	0.0%	2.2%	1.1%	0.5%	0.5%	2.2%	1	1	1	1	4					0
LAND	FARM FACTS	HEED NERDS	*I.ExecutionHow-GetIdeasFromAcademia*	0.0%	0.0%	0.0%	0.5%	0.9%	0.9%	0.0%	2.2%	0.5%	0.9%	0.5%	2.2%	1	1	1	1	4					0
LAND	FARM FACTS	HEED NERDS	*L.PortfolioDefinition-ByApproach-ScienceOrTechnology*	0.0%	0.0%	0.0%	0.6%	0.9%	0.5%	0.0%	2.0%	0.6%	0.9%	0.5%	2.0%	1	1	1	1	4					0
LAND	FARM FACTS	HEED NERDS	*M.PortfolioAllocation-WithPIs*	0.0%	0.0%	0.0%	0.3%	0.2%	0.1%	0.0%	0.6%	0.3%	0.2%	0.2%	0.6%		1			1					0
LAND	FARM FACTS	HEED NERDS	*U.Steps-05.a.Project-PIsSubmitProposalsExternally*	0.0%	0.0%	0.0%	1.1%	0.0%	1.4%	0.0%	2.5%	1.1%		1.4%	2.5%	1		1		2					0

LAND	FARM FACTS	KNOW BETTER	*F.ProjectChoice-NeedsOfScienceCommunity*	0.0%	0.0%	0.0%	0.8%	0.7%	0.0%	0.0%	1.5%	0.8%	0.7%		1.5%	1		1		2					0
LAND	FARM FACTS	KNOW BETTER	*F.ProjectChoice-WithinTheme*	0.0%	0.0%	0.0%	0.6%	0.5%	1.0%	0.0%	2.1%	0.6%	0.5%	1.0%	2.1%	1	1	1	1	4					0
LAND	FARM FACTS	KNOW BETTER	*J.Performance-MetricsMilestones-Specific-DataUseOrCitations*	0.0%	0.0%	0.0%	1.4%	0.9%	0.5%	0.0%	2.8%	1.4%	0.9%	0.5%	2.8%	1	1	1	1	4					0
LAND	FARM FACTS	KNOW BETTER	J.Performance-MetricsMilestones-Specific-PaperPublished	0.0%	0.2%	0.2%	1.4%	0.7%	1.4%	0.4%	3.4%	1.2%	0.6%	1.2%	3.0%	1	1	1	1	4					0
LAND	FARM FACTS	KNOW BETTER	L.PortfolioDefinition-ByApproach-SeedsOrTrees	0.6%	0.6%	0.0%	0.9%	0.4%	0.0%	1.2%	1.3%	0.5%	0.1%	0.4%				1		1					0
LAND	FARM FACTS	KNOW BETTER	*L.PortfolioDefinition-ByScienceTheme*	0.0%	0.0%	0.0%	1.4%	0.9%	1.7%	0.0%	4.0%	1.4%	0.9%	1.7%	4.0%	1	1	1	1	4					0
LAND	FARM FACTS	KNOW BETTER	*L.PortfolioDefinition-ByScienceTheme-Complementary*	0.0%	0.0%	0.0%	0.3%	0.4%	0.1%	0.0%	0.8%	0.3%	0.4%	0.1%	0.8%		1			1					0
LAND	FARM FACTS	KNOW BETTER	*Q.ResultsInApplication-ScienceIsTheApplication*	0.0%	0.0%	0.0%	0.6%	0.9%	0.1%	0.0%	1.6%	0.6%	0.9%	0.1%	1.6%	1		1		2					0
LAND	FARM FACTS	KNOW BETTER	*R.ApplicationHow-AdoptersApproachUs*	0.0%	0.0%	0.0%	1.1%	0.2%	0.1%	0.0%	1.4%	1.1%	0.2%	0.1%	1.4%			1		1					0
LAND	FARM FACTS	KNOW BETTER	*R.ApplicationHow-NothingIntentional*	0.0%	0.0%	0.0%	0.1%	0.4%	0.2%	0.0%	0.7%	0.1%	0.4%	0.2%	0.7%		1			1					0
LAND	FARM FACTS	KNOW BETTER	*R.ApplicationHow-ShowTheWorld-WhatWeHaveLearned*	0.0%	0.0%	0.0%	1.7%	0.7%	0.9%	0.0%	3.3%	1.7%	0.7%	0.9%	3.3%	1	1	1	1	4					0
LAND	FARM FACTS	SERVE SCIENCE	A.ManagerTenure-Time-MoreThan5Years	0.0%	0.0%	1.0%	0.6%	0.4%	0.6%	1.0%	1.5%	0.2%		0.3%	0.6%	1	1		1	3					0
LAND	FARM FACTS	SERVE SCIENCE	*I.ExecutionHow-ProjectsAreFuzzy*	0.0%	0.0%	0.0%	0.5%	0.5%	0.2%	0.0%	1.2%	0.5%	0.5%	0.2%	1.2%		1		1	2					0
LAND	FARM FACTS	SERVE SCIENCE	*I.ExecutionHow-SustainDataService*	0.0%	0.0%	0.0%	0.7%	0.9%	0.6%	0.0%	2.2%	0.7%	0.9%	0.6%	2.2%	1	1	1	1	4					0
LAND	FARM FACTS	SERVE SCIENCE	*I.ExecutionWho-Here-OARLabResearchers*	0.0%	0.0%	0.0%	1.2%	0.0%	0.0%	0.0%	1.2%	1.2%			1.2%			1		1					0
LAND	FARM FACTS	SERVE SCIENCE	*K.Duration-OngoingMultiDecadal*	0.0%	0.0%	0.0%	1.5%	0.5%	1.1%	0.0%	3.1%	1.5%	0.5%	1.1%	3.1%	1	1	1	1	4					0
LAND	FARM FACTS	SERVE SCIENCE	L.PortfolioDefinition-ByApproach-HighOrLowRisk	0.2%	0.4%	0.0%	0.5%	0.4%	0.4%	0.6%	1.2%	0.2%	0.2%	0.2%	0.6%		1		1	2					0
LAND	FARM FACTS	SERVE SCIENCE	*L.PortfolioDefinition-ByApproach-ObsOrElse*	0.0%	0.0%	0.0%	0.8%	0.7%	0.5%	0.0%	2.0%	0.8%	0.7%	0.5%	2.0%	1	1	1	1	4					0
LAND	FARM FACTS	SERVE SCIENCE	*N.Allies-Customer-NGOs*	0.0%	0.0%	0.0%	0.3%	0.2%	0.1%	0.0%	0.6%	0.3%	0.2%	0.1%	0.6%		1			1					0
LAND	FARM FACTS	SERVE SCIENCE	N.Allies-Customer-OtherAgencies	0.0%	0.4%	0.0%	1.0%	1.1%	0.2%	0.4%	2.4%	0.9%	1.0%	0.1%	1.9%	1		1	1	3					0
LAND	FARM FACTS	SERVE SCIENCE	Q.ResultsInApplication-MoreOftenThanNot	0.2%	0.0%	0.4%	0.9%	0.4%	0.5%	0.6%	1.8%	0.7%	0.2%	0.3%	1.2%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*G.Bureaucracy-Underresourced*	0.0%	0.0%	0.0%	0.6%	0.4%	1.0%	0.0%	1.9%	0.6%	0.4%	1.0%	1.9%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*I.ExecutionHow-GetMoneyToTalent*	0.0%	0.0%	0.0%	0.8%	0.2%	1.1%	0.0%	2.1%	0.8%	0.2%	1.1%	2.1%	1		1	1	3					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*I.ExecutionHow-GetRecruitDevelopTalent*	0.0%	0.0%	0.0%	1.1%	0.2%	0.4%	0.0%	1.7%	1.1%	0.2%	0.4%	1.7%	1		1	1	3					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*I.ExecutionHow-ReduceCompetition*	0.0%	0.0%	0.0%	0.0%	0.5%	1.2%	0.0%	1.8%		0.5%	1.2%	1.8%	1		1		2					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*I.ExecutionWho-Here-CIemployeesAtOARLabs*	0.0%	0.0%	0.0%	1.0%	0.0%	0.5%	0.0%	1.5%	1.0%		0.5%	1.5%	1		1		2					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*L.PortfolioDefinition-ByFunder-CongressOrNOAAOrCompete*	0.0%	0.0%	0.0%	1.1%	0.5%	1.4%	0.0%	3.0%	1.1%	0.5%	1.4%	3.0%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	*U.Steps-00.a.Program-TalentScoutedAndHired*	0.0%	0.0%	0.0%	1.0%	0.2%	0.5%	0.0%	1.7%	1.0%	0.2%	0.5%	1.7%	1		1	1	3					0
LAND	MAKE SAUSAGE	MANIFEST DESTINY	U.Steps-03.Program-PickProjectsPortfolio	0.0%	0.8%	0.0%	2.0%	0.7%	2.1%	0.8%	4.9%	1.8%	0.5%	1.8%	4.0%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	PLAY NICE	*D.HandsOnWith-OARLabDivisionCheifs*	0.0%	0.0%	0.0%	1.1%	0.0%	0.2%	0.0%	1.4%	1.1%		0.2%	1.4%			1		1					0
LAND	MAKE SAUSAGE	PLAY NICE	*E.ManagerRole-SailorShepherdOrthodontist*	0.0%	0.0%	0.0%	1.7%	0.2%	0.6%	0.0%	2.5%	1.7%	0.2%	0.6%	2.5%	1		1	1	3					0
LAND	MAKE SAUSAGE	PLAY NICE	*G.Bureaucracy-NetworkRelationships*	0.0%	0.0%	0.0%	0.1%	1.1%	1.1%	0.0%	2.3%	0.1%	1.1%	1.1%	2.3%	1		1		2					0
LAND	MAKE SAUSAGE	PLAY NICE	*N.Allies-Partner-HomeAgency-Science-Labs*	0.0%	0.0%	0.0%	0.3%	1.1%	0.4%	0.0%	1.8%	0.3%	1.1%	0.4%	1.8%	1		1	1	3					0
LAND	MAKE SAUSAGE	PLAY NICE	*N.Allies-Partner-HomeAgency-Science-ProgramOffice*	0.0%	0.0%	0.0%	0.6%	0.4%	0.6%	0.0%	1.5%	0.6%	0.4%	0.6%	1.5%	1	1		1	3					0
LAND	MAKE SAUSAGE	PLAY NICE	P.RulesOfEngagement-AddNotSubtract	0.0%	0.8%	0.2%	0.9%	1.1%	1.0%	1.0%	3.0%	0.6%	0.7%	0.6%	2.0%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	PLAY NICE	P.RulesOfEngagement-BeFairGainTrust	0.4%	0.0%	1.0%	0.3%	0.9%	0.2%	1.4%	1.5%	0.1%	0.4%	0.2%		1		1	1	3					0
LAND	MAKE SAUSAGE	PLAY NICE	*P.RulesOfEngagement-FriendsCloseFrenemiesCloser*	0.0%	0.0%	0.0%	0.5%	0.7%	0.5%	0.0%	1.7%	0.5%	0.7%	0.5%	1.7%	1	1		1	3					0
LAND	MAKE SAUSAGE	SHARE POWER	*F.ProjectChoiceWho-NotHere-OARLabDirector*	0.0%	0.0%	0.0%	0.2%	0.0%	1.0%	0.0%	1.2%	0.2%		1.0%	1.2%			1		1					0
LAND	MAKE SAUSAGE	SHARE POWER	*F.ProjectChoiceWho-NotHere-OARProgramOffice*	0.0%	0.0%	0.0%	0.2%	0.2%	1.4%	0.0%	1.8%	0.2%	0.2%	1.4%	1.8%	1		1	1	3					0
LAND	MAKE SAUSAGE	SHARE POWER	*F.ProjectChoiceWho-SubUnitManagers*	0.0%	0.0%	0.0%	0.5%	0.9%	0.4%	0.0%	1.7%	0.5%	0.9%	0.4%	1.7%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	SHARE POWER	*G.Bureaucracy-Empowered-NotManager*	0.0%	0.0%	0.0%	0.7%	0.2%	1.6%	0.0%	2.5%	0.7%	0.2%	1.6%	2.5%	1		1	1	3					0
LAND	MAKE SAUSAGE	SHARE POWER	G.Bureaucracy-TooMuchAtAgencyLevel	0.0%	0.0%	0.0%	0.9%	0.7%	0.2%	0.0%	1.9%	0.9%	0.7%	0.2%	1.9%	1		1	1	3					0
LAND	MAKE SAUSAGE	SHARE POWER	G.Bureaucracy-ToublesWithLeadership	0.0%	0.0%	0.0%	0.8%	0.4%	0.6%	0.0%	1.8%	0.8%	0.4%	0.6%	1.8%	1	1	1	1	4					0
LAND	MAKE SAUSAGE	SHARE POWER	*H.ChallengeVisionWho-ExternalFunderSponsor*	0.0%	0.0%	0.0%	0.8%	0.0%	1.0%	0.0%	1.8%	0.8%		1.0%	1.8%	1		1		2					0
LAND	MAKE SAUSAGE	SHARE POWER	*I.ExecutionHow-ConductWhatOthersFund*	0.0%	0.0%	0.0%	1.1%	0.0%	1.4%	0.0%	2.5%	1.1%		1.4%	2.5%	1		1		2					0
LAND	MAKE SAUSAGE	SHARE POWER	*M.PortfolioAllocation-WithOARProgramOrLab*	0.0%	0.0%	0.0%	0.2%	0.2%	0.5%	0.0%	0.9%	0.2%	0.2%	0.5%	0.9%				1	1					0
x	x	x	U.Steps-00.Program-AreaDefinedManagerHired	0.6%	0.8%	0.6%	0.3%	0.7%	0.6%	2.0%	1.7%	0.3%	0.0%	0.1%	0.4%	1	1		1	3	1	1		1	3
x	x	x	A.ManagerTenure-Time-2YearsOrLess	0.0%	0.6%	0.0%	0.5%	0.4%	0.0%	0.6%	0.8%	0.2%	0.2%	0.2%	0.2%					0					0
x	x	x	A.ManagerTenure-Time-3YearsOrLess	0.4%	0.4%	0.0%	0.1%	0.5%	0.2%	0.8%	0.9%	0.2%	0.3%	0.0%						0					0
x	x	x	A.ManagerTenure-Time-4YearsOrLess	0.6%	0.0%	0.2%	0.0%	0.0%	0.5%	0.8%	0.5%	0.3%	0.3%	0.2%	0.3%					0					0
x	x	x	A.ManagerTenure-Time-5YearsOrLess	0.4%	0.0%	0.0%	0.1%	0.0%	0.0%	0.4%	0.1%	0.0%	0.1%	0.1%	0.3%					0					0
x	x	x	B.ManagerStudied-Law	0.2%	0.0%	0.0%	0.0%	0.0%	0.1%	0.2%	0.1%	0.1%	0.1%	0.1%	0.1%					0					0
x	x	x	B.ManagerStudied-SocialScience	0.0%	0.0%	0.6%	0.0%	0.5%	0.0%	0.6%	0.5%	0.2%	0.3%	0.2%						0					0
x	x	x	B.ManagerWorked-atStartUp	0.2%	0.2%	0.0%	0.0%	0.0%	0.0%	0.4%	0.0%	0.1%	0.1%	0.1%	0.4%					0					0
x	x	x	C.ManagerTalent-AggressiveNegotiator	0.2%	0.6%	0.0%	0.0%	0.0%	0.0%	0.8%	0.0%	0.3%	0.3%	0.3%	0.8%					0					0
x	x	x	C.ManagerTalent-EconomicAnalysis	0.2%	0.0%	1.0%	0.0%	0.4%	0.0%	1.2%	0.4%	0.4%	0.0%	0.4%	0.8%					0					0
x	x	x	C.ManagerTalent-isDiverse	0.2%	0.4%	0.0%	0.0%	0.0%	0.0%	0.6%	0.0%	0.2%	0.2%	0.2%	0.6%					0					0
x	x	x	*C.ManagerTalent-RaiseMoney*	0.0%	0.0%	0.0%	0.0%	0.0%	0.6%	0.0%	0.6%			0.6%	0.6%					0					0
x	x	x	*D.HandsOnWith-AgencyLeadership*	0.0%	0.0%	0.0%	0.2%	0.5%	0.0%	0.0%	0.8%	0.2%	0.5%		0.8%					0					0
x	x	x	*D.HandsOnWith-OARCIDirectors*	0.0%	0.0%	0.0%	0.0%	0.2%	0.1%	0.0%	0.3%		0.2%	0.1%	0.3%					0					0
x	x	x	*D.HandsOnWith-OARLabDirectors*	0.0%	0.0%	0.0%	0.3%	0.0%	0.2%	0.0%	0.6%	0.3%		0.2%	0.6%					0					0
x	x	x	*D.HandsOnWith-OARProgramManagers*	0.0%	0.0%	0.0%	0.1%	0.5%	0.5%	0.0%	1.2%	0.1%	0.5%	0.5%	1.2%					0					0
x	x	x	*D.HandsOnWith-PolicyMakers*	0.0%	0.0%	0.0%	0.0%	0.2%	0.0%	0.0%	0.2%		0.2%		0.2%					0					0
x	x	x	*D.HandsOnWith-University-Admin*	0.0%	0.0%	0.0%	0.0%	0.2%	0.6%	0.0%	0.8%		0.2%	0.6%	0.8%					0					0
x	x	x	E.ManagerRole-Misfit	0.2%	0.4%	0.0%	0.0%	0.0%	0.0%	0.6%	0.0%	0.2%	0.2%	0.2%	0.6%					0					0
x	x	x	E.ManagerRole-ZealotEvangelist	0.6%	0.4%	0.0%	0.1%	0.0%	0.1%	1.0%	0.2%	0.2%	0.3%	0.2%	0.8%					0					0
x	x	x	F.ProjectChoice-BestIdeas	0.0%	0.2%	0.4%	0.0%	0.0%	0.2%	0.6%	0.2%	0.2%	0.2%	0.0%	0.4%					0					0
x	x	x	*F.ProjectChoice-CuttingEdgeOfResearch*	0.0%	0.0%	0.0%	0.5%	0.0%	0.5%	0.0%	0.9%	0.5%		0.5%	0.9%					0					0

x	x	x	*F.ProjectChoice-Interdisciplinary*		0.0%	0.0%	0.0%	0.2%	0.0%	0.2%	0.0%	0.5%		0.2%		0.2%	0.5%						0						0
x	x	x	*F.ProjectChoice-LowRisk*		0.0%	0.0%	0.0%	0.5%	0.0%	0.4%	0.0%	0.8%		0.5%		0.4%	0.8%						0						0
x	x	x	*F.ProjectChoice-NeedsOfUniversity*		0.0%	0.0%	0.0%	0.0%	0.0%	0.2%	0.0%	0.2%				0.2%	0.2%						0						0
x	x	x	*F.ProjectChoiceWho-NotHere-CongressMandate*		0.0%	0.0%	0.0%	0.1%	0.4%	0.1%	0.0%	0.6%		0.1%	0.4%	0.1%	0.6%						0						0
x	x	x	*F.ProjectChoiceWho-NotHere-UniversityPartners*		0.0%	0.0%	0.0%	0.0%	0.7%	0.1%	0.0%	0.8%			0.7%	0.1%	0.8%						0						0
x	x	x	G.Bureaucracy-Streamlined-HiringFiring		0.6%	0.6%	0.0%	0.1%	0.2%	0.5%	1.2%	0.8%		0.3%	0.2%	0.1%	0.5%						0						0
x	x	x	*H.ChallengeVision-Specific-Science*		0.0%	0.0%	0.0%	0.6%	0.5%	0.1%	0.0%	1.2%		0.6%	0.5%	0.1%	1.2%						0						0
x	x	x	I.ExecutionWho-EarlyCareer		0.0%	0.2%	0.0%	0.1%	0.0%	0.6%	0.2%	0.7%		0.0%	0.1%	0.5%	0.5%						0						0
x	x	x	I.ExecutionWho-NotHere-AgencyLabs		0.0%	0.0%	0.2%	0.0%	0.7%	0.0%	0.2%	0.7%		0.1%	0.7%	0.1%	0.5%						0						0
x	x	x	I.ExecutionWho-NotHere-NonProfit		0.0%	0.0%	0.4%	0.0%	0.0%	0.0%	0.4%	0.0%		0.1%	0.1%	0.1%	0.4%						0						0
x	x	x	*I.ExecutionWho-NotHere-OtherAgencies*		0.0%	0.0%	0.0%	0.0%	0.4%	0.0%	0.0%	0.4%			0.4%		0.4%						0						0
x	x	x	I.ExecutionWho-NotHere-PrivateIndustry-SmallCompanies		0.0%	0.8%	0.0%	0.0%	0.0%	0.0%	0.8%	0.0%		0.3%	0.3%	0.3%	0.8%						0						0
x	x	x	*J.Performance-HistoricalAccomplishments*		0.0%	0.0%	0.0%	0.1%	0.0%	0.0%	0.0%	0.1%		0.1%			0.1%						0						0
x	x	x	*J.Performance-Inputs-ContinuedFunding*		0.0%	0.0%	0.0%	0.0%	0.2%	0.6%	0.0%	0.8%			0.2%	0.6%	0.8%						0						0
x	x	x	*J.Performance-Inputs-ResearchTalent*		0.0%	0.0%	0.0%	0.2%	0.0%	0.2%	0.0%	0.5%		0.2%		0.2%	0.5%						0						0
x	x	x	J.Performance-MetricsMilestones-Specific-Collaboration		0.0%	0.2%	1.0%	0.1%	0.2%	0.4%	1.2%	0.7%		0.3%	0.2%		0.5%						0						0
x	x	x	J.Performance-MetricsMilestones-Specific-PatentsAwarded		0.0%	0.2%	0.6%	0.0%	0.2%	0.1%	0.8%	0.3%		0.3%	0.1%	0.1%	0.5%						0						0
x	x	x	J.Performance-StageGate-NotStandard-X		0.4%	0.0%	0.2%	0.0%	0.0%	0.0%	0.6%	0.0%		0.2%	0.2%	0.2%	0.6%						0						0
x	x	x	J.Performance-StageGate-TRLs		0.0%	0.0%	0.0%	0.1%	0.0%	0.1%	0.0%	0.2%		0.1%		0.1%	0.2%						0						0
x	x	x	K.Duration-2YearsOrLess		0.0%	0.0%	0.0%	0.6%	0.7%	0.1%	0.0%	1.4%		0.6%	0.7%	0.1%	1.4%						0						0
x	x	x	K.Duration-3YearsOrLess		0.2%	0.6%	0.0%	0.0%	0.4%	0.7%	0.8%	1.1%		0.3%	0.1%	0.5%	0.3%						0						0
x	x	x	K.Duration-4YearsOrLess		0.8%	0.4%	0.0%	0.0%	0.2%	0.2%	1.3%	0.4%		0.4%	0.2%	0.2%	0.8%						0						0
x	x	x	K.Duration-MoreThan5Years		0.0%	0.0%	0.0%	0.0%	0.0%	0.2%	0.0%	0.2%				0.2%	0.2%						0						0
x	x	x	K.Termination-ManagementConcerns		0.0%	0.4%	0.8%	0.0%	0.0%	0.0%	1.2%	0.0%		0.4%	0.4%	0.4%	1.2%						0						0
x	x	x	K.Termination-OffTrackOutOfScope		0.0%	0.0%	1.0%	0.0%	0.2%	0.0%	1.0%	0.2%		0.3%	0.1%	0.3%	0.8%						0						0
x	x	x	K.Termination-PerformerDropsOut		0.0%	0.0%	0.2%	0.0%	0.0%	0.0%	0.2%	0.0%		0.1%	0.1%	0.1%	0.2%						0						0
x	x	x	*K.Termination-WhenMoneyRunsOut*		0.0%	0.0%	0.0%	0.3%	0.2%	0.0%	0.0%	0.5%		0.3%	0.2%		0.5%						0						0
x	x	x	*K.Termination-WhenPrioritiesChange*		0.0%	0.0%	0.0%	0.0%	0.2%	0.1%	0.0%	0.3%			0.2%	0.1%	0.3%						0						0
x	x	x	*L.PortfolioDefinition-ByApproach-NaturalOrSocial*		0.0%	0.0%	0.0%	0.0%	0.7%	0.5%	0.0%	1.2%			0.7%	0.5%	1.2%						0						0
x	x	x	L.PortfolioDefinition-ByApproach-OpenOrFocused		0.0%	0.0%	0.6%	0.0%	0.0%	0.0%	0.6%	0.0%		0.2%	0.2%	0.2%	0.6%						0						0
x	x	x	*L.PortfolioDefinition-ByApproach-PullOrPush*		0.0%	0.0%	0.0%	0.1%	0.2%	0.0%	0.0%	0.3%		0.1%	0.2%		0.3%						0						0
x	x	x	L.PortfolioDefinition-ByPerformer-IntramuralExtramural		0.0%	0.0%	0.2%	0.0%	0.0%	0.2%	0.2%	0.2%		0.1%	0.0%	0.2%							0						0
x	x	x	L.PortfolioDefinition-ByPerformer-SingleOrJoint		0.0%	0.0%	0.8%	0.0%	0.0%	0.0%	0.8%	0.0%		0.3%	0.3%	0.3%	0.8%						0						0
x	x	x	L.PortfolioDefinition-ByPerformer-SmallOrLarge		0.2%	0.0%	0.6%	0.0%	0.0%	0.0%	0.8%	0.0%		0.3%	0.3%	0.3%	0.8%						0						0
x	x	x	*L.PortfolioDefinition-ByResource-HumansOrEquipment*		0.0%	0.0%	0.0%	0.2%	0.0%	0.0%	0.0%	0.2%		0.2%			0.2%						0						0
x	x	x	*M.PortfolioAllocation-HaircutsAndPeanutButtter*		0.0%	0.0%	0.0%	0.3%	0.7%	0.1%	0.0%	1.2%		0.3%	0.7%	0.1%	1.2%						0						0
x	x	x	*M.PortfolioAllocation-WithOtherFunders*		0.0%	0.0%	0.0%	0.1%	0.5%	0.2%	0.0%	0.9%		0.1%	0.5%	0.2%	0.9%						0						0
x	x	x	*M.PortfolioAllocation-WithOtherInstitutesInCI*		0.0%	0.0%	0.0%	0.0%	0.0%	0.1%	0.0%	0.1%				0.1%	0.1%						0						0
x	x	x	N.Allies-Advocate-DepartmentLeadership		0.4%	0.6%	0.0%	0.0%	0.0%	0.0%	1.0%	0.0%		0.3%	0.3%	0.3%	1.0%						0						0
x	x	x	*N.Allies-Advocate-HomeAgency-OtherCIs*		0.0%	0.0%	0.0%	0.0%	0.0%	0.1%	0.0%	0.1%				0.1%	0.1%						0						0
x	x	x	N.Allies-Advocate-Industry		0.0%	0.2%	0.0%	0.0%	0.0%	0.0%	0.2%	0.0%		0.1%	0.0%	0.1%	0.2%						0						0
x	x	x	N.Allies-Advocate-Performer		0.2%	0.0%	0.0%	0.0%	0.4%	0.0%	0.2%	0.4%		0.1%	0.3%	0.1%	0.2%						0						0
x	x	x	N.Allies-Advocate-President		0.0%	0.2%	0.8%	0.0%	0.0%	0.0%	1.0%	0.0%		0.3%	0.3%	0.3%	1.0%						0						0
x	x	x	N.Allies-Customer-Industry		0.0%	0.6%	0.8%	0.5%	0.0%	0.1%	1.4%	0.6%			0.5%	0.3%	0.8%						0						0
x	x	x	N.Allies-NotRival-OtherAgencies		0.0%	0.2%	0.6%	0.2%	0.0%	0.0%	0.8%	0.2%		0.1%	0.3%	0.3%	0.6%						0						0
x	x	x	N.Allies-NotRival-Universities		0.0%	0.2%	1.0%	0.0%	0.0%	0.2%	1.2%	0.2%		0.4%	0.4%	0.2%	0.9%						0						0
x	x	x	*N.Allies-Partner-HomeAgency-CIs*		0.0%	0.0%	0.0%	0.3%	0.0%	0.2%	0.0%	0.6%		0.3%		0.2%	0.6%						0						0
x	x	x	*N.Allies-Partner-HomeAgency-CIs-InstitutesWithin*		0.0%	0.0%	0.0%	0.0%	0.0%	0.5%	0.0%	0.5%				0.5%	0.5%						0						0
x	x	x	N.Allies-Partner-Industry-SystemsIntegrators		0.4%	0.0%	0.0%	0.0%	0.4%	0.1%	0.4%	0.5%		0.1%	0.2%								0						0
x	x	x	*N.Allies-Partner-ResearchPerformers*		0.0%	0.0%	0.0%	0.0%	0.5%	0.1%	0.0%	0.7%			0.5%	0.1%	0.7%						0						0
x	x	x	*O.Rivals-Competitor-Contractors*		0.0%	0.0%	0.0%	0.0%	0.0%	0.1%	0.0%	0.1%				0.1%	0.1%						0						0
x	x	x	*O.Rivals-Competitor-HomeAgency-OARProgramOffice*		0.0%	0.0%	0.0%	0.0%	0.5%	0.0%	0.0%	0.5%			0.5%		0.5%						0						0
x	x	x	*O.Rivals-Competitor-HomeAgency-OtherCIs*		0.0%	0.0%	0.0%	0.0%	0.0%	0.5%	0.0%	0.5%				0.5%	0.5%						0						0
x	x	x	O.Rivals-Competitor-HomeAgency-Service		0.0%	0.2%	0.0%	0.1%	0.2%	0.0%	0.2%	0.3%		0.0%	0.1%	0.1%							0						0
x	x	x	*O.Rivals-Competitor-InternationalEquivalents*		0.0%	0.0%	0.0%	0.2%	0.0%	0.0%	0.0%	0.2%		0.2%			0.2%						0						0
x	x	x	*O.Rivals-Competitor-OtherResearchInstitutions*		0.0%	0.0%	0.0%	0.0%	0.2%	0.2%	0.0%	0.4%			0.2%	0.2%	0.4%						0						0
x	x	x	*O.Rivals-Competitor-Universities*		0.0%	0.0%	0.0%	0.0%	0.0%	0.5%	0.0%	0.5%				0.5%	0.5%						0						0
x	x	x	*O.Rivals-Competitor-UnselectedPIs*		0.0%	0.0%	0.0%	0.0%	0.2%	0.4%	0.0%	0.6%			0.2%	0.4%	0.6%						0						0
x	x	x	*O.Rivals-Incumbant-HomeAgency-Science-Labs*		0.0%	0.0%	0.0%	0.0%	0.0%	0.1%	0.0%	0.1%				0.1%	0.1%						0						0
x	x	x	O.Rivals-Incumbant-Industry		0.0%	0.2%	0.2%	0.0%	0.0%	0.0%	0.4%	0.0%		0.1%	0.1%	0.1%	0.4%						0						0
x	x	x	*O.Rivals-Incumbant-SciencePurists*		0.0%	0.0%	0.0%	0.5%	0.5%	0.0%	0.0%	1.0%		0.5%	0.5%		1.0%						0						0
x	x	x	*O.Rivals-Incumbant-X*		0.0%	0.0%	0.0%	0.0%	0.2%	0.0%	0.0%	0.2%			0.2%		0.2%						0						0
x	x	x	*O.Rivals-NotAdvocate-AgencyLeadership*		0.0%	0.0%	0.0%	0.1%	0.5%	0.4%	0.0%	1.0%		0.1%	0.5%	0.4%	1.0%						0						0
x	x	x	O.Rivals-SecurityThreats		0.4%	0.0%	0.0%	0.1%	0.0%	0.0%	0.4%	0.1%			0.1%	0.1%	0.3%						0						0
x	x	x	P.RulesOfEngagement-KeepALowProfile		0.4%	0.0%	0.0%	0.0%	0.0%	0.0%	0.4%	0.0%		0.1%	0.1%	0.1%	0.4%						0						0
x	x	x	P.RulesOfEngagement-KickBacksToLabs		0.0%	0.0%	0.8%	0.0%	0.2%	0.0%	0.8%	0.2%		0.3%	0.1%	0.3%	0.6%						0						0
x	x	x	Q.ResultsInApplication-CantSayTooEarly		0.0%	0.8%	0.2%	0.1%	0.5%	0.1%	1.0%	0.8%		0.2%	0.2%	0.2%	0.2%						0						0
x	x	x	Q.ResultsInApplication-LessOftenThanNot		0.2%	0.0%	0.4%	0.1%	0.2%	0.0%	0.6%	0.3%		0.1%		0.2%	0.3%						0						0

x	x	x	Q.ResultsInApplication-NotDirectCantTrace		0.8%	0.0%	0.4%	0.0%	0.0%	0.0%	1.2%	0.0%		0.4%	0.4%	0.4%	1.2%						0						0
x	x	x	*R.ApplicationHow-FindAdopters-Government-International*		0.0%	0.0%	0.0%	0.3%	0.0%	0.2%	0.0%	0.6%		0.3%		0.2%	0.6%						0						0
x	x	x	*R.ApplicationHow-TransferHelpNeeded*		0.0%	0.0%	0.0%	0.5%	0.0%	0.2%	0.0%	0.7%		0.5%		0.2%	0.7%						0						0
x	x	x	S.EngagePrivateSector-AwardPrizes		0.4%	0.0%	0.0%	0.0%	0.0%	0.0%	0.4%	0.0%		0.1%	0.1%	0.1%	0.4%						0						0
x	x	x	T.EngagePublicSector-ProcurementForAdoption		0.8%	0.2%	0.0%	0.1%	0.5%	0.2%	1.0%	0.9%		0.2%	0.2%	0.1%							0						0