Funding Breakthrough Research: Promises and Challenges of the "ARPA Model"

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Abstract

From its 1958 origin in defense, the Advanced Research Projects Agency (ARPA) model for research funding has, in the last two decades, spread to other parts of the US federal government with the goal of developing radically new technologies. In this paper, we propose that the key elements of the ARPA model for research funding are: organizational flexibility on an administrative level, and significant authority given to program directors to design programs, select projects and actively manage projects. We identify the ARPA model's domain as mission-motivated research on nascent S-curves within an inefficient innovation system. Finally, we describe some of the challenges to implementing the ARPA model, and we comment on the role of ARPA in the landscape of research funding approaches.

Note: All four authors were involved in the National Academies review of ARPA-E, "An Assessment of ARPA-E" (NRC 2017). Azoulay and Fuchs were Committee Members, and Goldstein and Kearney were consultants to the Committee.

1. Introduction

In October 1957, the Soviet Union launched Sputnik 1 into low-Earth orbit, igniting both a space race and an innovation race with the United States. The United States countered with the Apollo Program and a focus on broader innovation efforts, particularly within the Department of Defense (DOD). In 1958, the DOD launched the Advanced Research Projects Agency (ARPA) to make sure that the United States did not suffer any further technological surprises. Later renamed the Defense Advanced Research Projects Agency (DARPA), the agency has grown to maintain an annual budget of $3 billion for extramural research funding overseen by roughly 100 program managers (Gallo 2018).

DARPA is frequently heralded for its successes in funding high-risk, high-reward research,. In a report celebrating its 50th anniversary, DARPA highlighted areas of impact on military applications, such as missile defense and stealth technology, as well as innovations that have impacted consumer markets as well, including GPS, the Internet, and autonomous navigation (DARPA 2008). Following these perceived successes, the ARPA model has expanded to federal agencies outside of the DOD, to include HSARPA for Homeland Security in 2002, IARPA for the intelligence agencies in 2006, and ARPA-E in the Department of Energy (DOE) in 2009.

Although it is seen as the originator of significant technological successes, DARPA's operations and strategic decisions remain largely secret, due to the sensitive nature of its defense mission. In recent years, motivated researchers have begun to lift the veil on the "ARPA model" of innovation. Literature on the ARPA model typically emphasizes a few defining characteristics, including (a) the agile and flexible nature of the organization, (b) the identification of technical white space and design of programs to fill that void, (c) the selection of projects with extraordinary discretion, and (d) the active management of each project to specific technical milestones and time commitments. Each of these features rely heavily on the quality of the program manager, who is often said to be the key to the ARPA model.

Beyond the numerous anecdotes of success from DARPA's history, rigorous empirical evaluation of the ARPA model remains elusive. This is partly due to an incompatibility between traditional innovation metrics and the lofty goals of ARPA organizations, e.g. "to make pivotal investments in breakthrough technologies" for DARPA,[1] or "accelerating transformational technological advances" for ARPA-E (110th Congress 2007). The returns to innovation are notoriously skew-distributed (Scherer and Harhoff, 2000), and for a program that makes long-

[1] https://www.darpa.mil/about-us/mission

term and high-risk investments, many failures can be justified by a single success. The search for rare outcomes handicaps formal program evaluation, which is geared toward measuring short-term outputs like patents and publications, or even medium-term outputs like commercialization activity. It is impossible to accurately measure the incidence of one-in-a-thousand ideas, much less one-in-a-million ideas, on a timescale relevant to political decision-making around program authorization.

Nevertheless, the prospect of being able to stimulate transformational innovation holds great appeal in the US and across the world, and the idea that the ARPA model is appropriate for stimulating transformational innovation is widespread. The question for policymakers remains: should the ARPA model be replicated, and if so, under what conditions? To help answer this question, we provide a summary of how the ARPA model has been implemented and what factors may be important for its success. Section 2 provides a history of (D)ARPA and its younger siblings. Section 3 outlines the elements of the ARPA model. Section 4 discusses what is "ARPA-ble," to help identify technological areas where the model might bear fruit. Section 5 draws attention to several challenges for the model, and Section 6 concludes by placing ARPA-like agencies in context of the larger innovation system.

2. History of (D)ARPA & Younger Siblings

Since its inception in 1958 as ARPA, one consistent truth about DARPA has been its amoeba-like ability to evolve with new Directors in ever-changing political environments. Indeed, throughout its history, DARPA's research focus has oscillated between two modes depending on the political dynamics of the time, specifically with respect to the United States' military posture. In wartime, Directors tended to shift ARPA's research portfolio toward applied research in defense technologies, while in peacetime, the portfolio expanded to include basic research across broader technology areas. Here, we provide a brief overview of this history, drawing primarily from work by Fuchs (2010), which offers a more detailed narrative.

ARPA was initially formed in response to the Soviet Union's launch of Sputnik, a major step forward for the United States' Cold War adversary in the space race. As such, the core mission of ARPA at inception was the prevention of technological surprise (NRC, 1999). ARPA was initially tasked with overseeing America's burgeoning space program while the National

Aeronautics and Space Administration (NASA) was launched (Fuchs, 2010). The agency was to intended to explore new technological trajectories with promise (Fuchs, 2010).

In 1960, with NASA up and running, ARPA shifted its focus from space to high-risk military technology that wasn't garnering support in the private sector, with the goal of transitioning the technologies from the lab to proof of concept (Roland, 2002; Fuchs, 2010). As an independent entity within the Department of Defense, the agency had to be mindful of the distribution of research and development efforts across the military services. Focusing on earlier stage research enabled ARPA to avoid overlap with more applied research and systems development. ARPA became known as a hub of radical ideas, a reputation that has been largely maintained across the arc of the organization's evolution.

Over the course of the 1960s, ARPA developed a flat organizational architecture, staffed with top-notch engineers and scientists from industry and academia (NRC, 1999). Under Director Jack Ruina, office directors and program managers operated with significant autonomy and were encouraged to make use of available tools like "no-year money" and unsolicited proposals (NRC, 1999; Fuchs, 2010). As staff were empowered to build programs independently, the agency's influence was dependent on the personalities of these staffers and the vision set forth by the Director.

Until the Vietnam War, ARPA maintained its commitment to funding high-risk, long-term research, but once the country shifted toward a wartime footing, research was redirected toward military applications (Flamm, 1987). In fact, under President Nixon, Congress required that all military funding have a "direct and apparent relationship to a specific military function" (NRC, 1999). Accordingly, ARPA rebranded as Defense ARPA (DARPA) in 1972. With these shifts, the research portfolio at DARPA took on a stricter mission orientation, which continued through the Directorship of George Heilmeier.

In the decades after the Vietnam War, economic concerns returned DARPA to a broader mission related to technological competitiveness (Fuchs, 2010). In the 1980s, analysts worried about the departure of microelectronics and computer industries to Japan (NRC, 1999), and correspondingly, DARPA positioned itself be a prominent player in the advancement of computing. In 1993, DARPA rebranded again as ARPA, and returned to supporting more early-stage research, though in 1996, ARPA became DARPA once again (Fuchs, 2010).

During the George W. Bush Administration, the agency explicitly returned to a mission-driven applied research organization. Tony Tether took over as Director in 2001 and repositioned the agency to "bridge the gap" between fundamental discovery and military use (Fuchs, 2010). As a result, the proportion of DARPA funding allocated toward academic researchers declined significantly—almost 50 percent (Lazowska and Patterson, 2005; Markoff, 2005; Fuchs, 2010). Tether also increased usage and shortened the timeline for go/no-go milestones for projects that, if un-met, resulted in the termination of projects (Fuchs, 2010).

It was in this context that the ARPA model first appeared in other Departments of the federal government. In 1998, the Central Intelligence Agency (CIA) launched the Advanced Research and Development Activity (ARDA), which became the Disruptive Technologies Office (DTO) in 2006. DTO was then combined with the newly created IARPA in 2007, with the goal of exploring potentially transformative technical breakthroughs for intelligence activities, including data collection, analysis and computing. Similarly, the Department of Homeland Security launched HSARPA in 2002 to identify operational gaps that could be filled with technological advance within the agencies of Homeland Security: U.S. Customs and Border Protection, Secret Service, and the Coast Guard.

In its 2007 report *Rising Above the Gathering Storm*, a committee of the National Academies called for the creation of a DARPA-like agency within the Department of Energy (NRC, 2007). ARPA-E was In the same year, the America COMPETES Act authorized the creation of ARPA-E, which was tasked with "identifying and promoting revolutionary advances in fundamental science; translating scientific discoveries and cutting-edge inventions into technological innovations; and accelerating transformational technological advances in areas that industry itself is not likely to undertake because of technical and financial uncertainty" (110th Congress, 2007). ARPA-E received initial funding in the American Recovery and Reinvestment Act of 2009, and has been in operation since.

There have been four agencies to use the ARPA name (DARPA, IARPA, HSARPA and ARPA-E), and others such as the Advanced Technology Program (ATP) in the National Institute of Standards and Technology (NIST) and Biomedical Advanced Research and Development Authority (BARDA) in the Department of Health and Human Services (HHS) have been inspired by the model. However, these organizations are not equivalent in their approach or their impact. DARPA is by far is the most well-known of the ARPA-like agencies and is the subject of several

books in the popular press (Belfiore, 2010; Jacobsen, 2015; Weinberger, 2017). More recently, ARPA-E has been subject to scrutiny and empirical review (National Research Council, 2017). In the forthcoming section, we will focus on DARPA and ARPA-E as exemplars of the ARPA model, and we identify common elements that comprise the core of the model.

3. Elements of the ARPA model

Once shrouded in secrecy, the organizational structure and processes that govern ARPA model agencies have made their way into the public consciousness through case studies of DARPA-funded innovations and recent empirical assessments of ARPA-E. Much of the discussion about the ARPA model relates to the responsibilities of the program manager, who directly administers the agency's research spending. DARPA has a reputation for its flexibility and speed, partially as a result of the forward-looking and aggressive culture of program managers, often described as "100 geniuses connected by a travel agent."[2] ARPA-E has also attempted to cultivate an "innovative startup-like culture" with energetic, highly-qualified program managers (Rohlfing, 2017).

Serving as a DARPA program manager has been a launching pad for prominent careers in science and technology. Program managers from DARPA have gone on to lead prestigious science and engineering programs across the country. Pradeep Khosla (former Dean of Engineering at Carnegie Mellon University and current Chancellor of UCSD), Zachary Lemnos (former Chief Technology Officer of MIT Lincoln Laboratory and former Assistant Secretary of Defense for Research and Engineering), Arati Prabhakar (former Director of the National Institute of Standards and Technology and former Director of DARPA), and Robert Taylor (founder of Xerox PARC's Computer Science Laboratory) were all DARPA program managers earlier in their careers.

Defense analyst Richard Van Atta, in his Congressional testimony on lessons from DARPA the establishment of ARPA-E, summed up the importance of the program manager:

> The DARPA program manager is, in fact, the key. He or she is the technical champion who conceives and owns the program. He is not told

[2] Program Directors at ARPA-E are roughly equivalent to the position of Program Manager at DARPA. We use each term when referring to the program staff of the relevant agency.

> what to do, though he does have to have approval from his office director, and from the DARPA Director. Once he starts that program, it is his, and he makes it happen, and he has to make the choices involved in that. So, in essence, they are risk-taking, idea-driven entrepreneurs heading up their own practice. (Van Atta, 2007)

Other commentary and research on DARPA has captured different facets of the agency, many of which are centered in some way on the interactions between an ARPA program manager and the research community, illustrated in Figure 1. Bonvillian describes DARPA as the heir to a WWII model of "'connected' science, where technology breakthroughs at the fundamental science stage were closely connected to the follow-on applied stages of development, prototyping and production" (2009). Fuchs describes DARPA's function as "embedded network governance" based on the program manager's engineering social networks in order to influence technology directions (2010)—a view supported by Colatat (2015), who found that DARPA funding spurred the creation of new collaborations. Greenstein (2011) portrays DARPA as a "skunk works" for the military; this is similar to Sen's view of ARPA-like agencies as "islands" separated from conventional operations as well as "bridges" between R&D activities and the broader context.

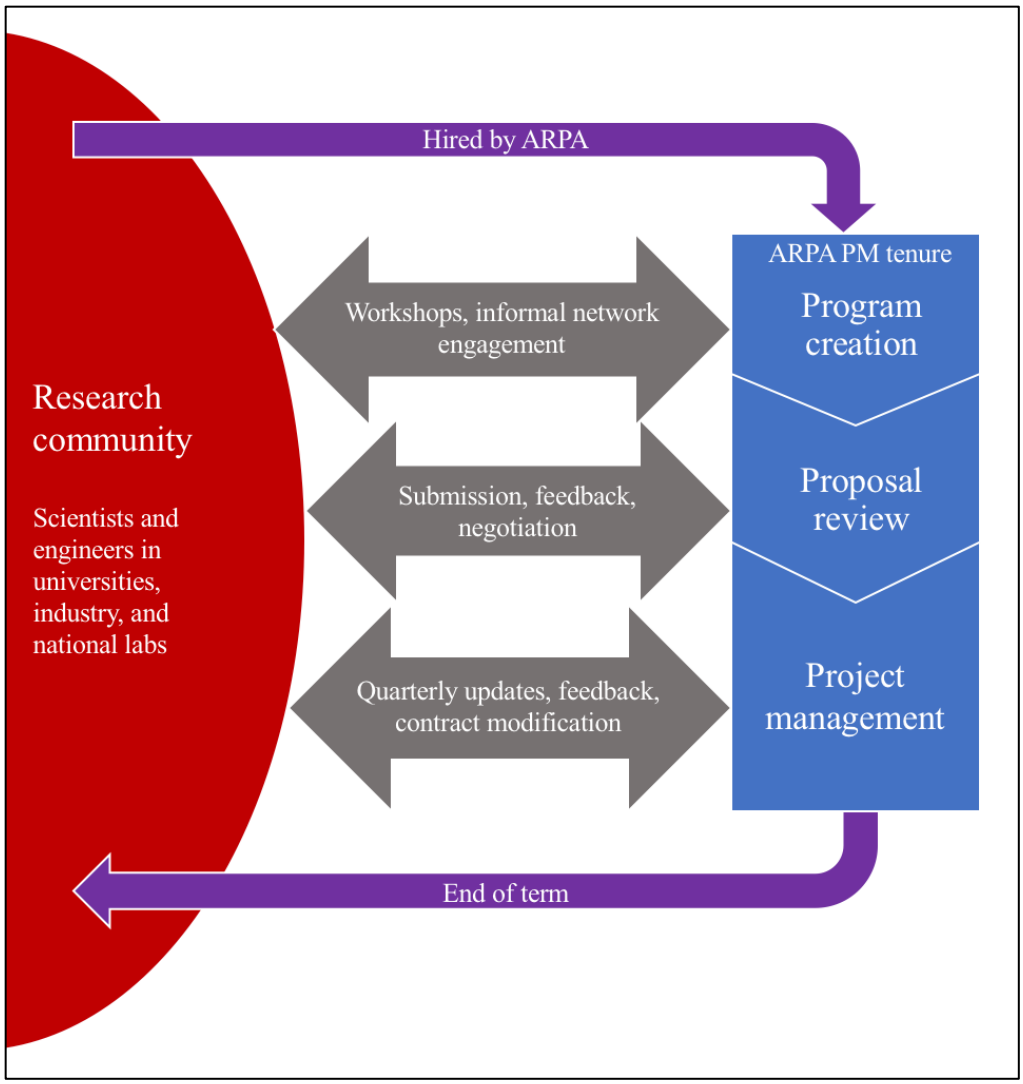


Figure 1. The Lifecycle of a Program Manager at an ARPA Agency

In this section, we structure our discussion of the ARPA model by highlighting four common institutional features of DARPA and ARPA-E. Although each of these may be adhered to by an ARPA-like agency to varying degrees at any given time, together they form the core of the ARPA model. They fit into four buckets: first, general organizational flexibility, and three elements of program manager responsibility, each corresponding to one stage of an ARPA program: (2) bottom-up program design, (3) discretion in project selection and (4) active project management.

General Organizational Flexibility

Contrary to most agencies in the federal bureaucracy, the ARPA model entails significant organizational flexibility, allowing agencies to respond more quickly to changing technological conditions. Between DARPA and ARPA-E, there are a number of policies and practices that provide this flexibility:

Fungible appropriations. The Department of Energy budget includes only two line items for ARPA-E projects: Transportation Systems, and Stationary Power Systems (DOE 2016). For comparison, the Office of Energy Efficiency and Renewable Energy has 11 programs budgeted separately by Congress. This flexibility allows program directors to pursue topics of their choosing. While DARPA's appropriations include breakdowns for specific technology areas, the program managers still have autonomy to define a specific challenge within those areas.

Flat organization. The Director of ARPA-E reports directly to the Secretary of Energy, which allows ARPA-E to operate outside the many levels of bureaucracy encumbering other research organizations within DOE. Within the agencies, ARPA-E and DARPA both have a flat internal structure. All ARPA-E program directors report to the agency's Deputy Director for Technology. DARPA has one additional degree of hierarchy with its six program offices, and yet this structure is still less subdivided than other agencies such as the Army Research Office, which is organized into directorates consisting of multiple divisions.

Hiring outside of civil service regulations. DARPA and ARPA-E can hire program managers without being confined by the typical requirements for hiring federal employees or contractors. This allows them to hire top talent quickly and with more competitive salaries.

Fixed-term employment. Program managers are hired for three-year terms. This practice is advantageous both for the organization and the program directors themselves. Theoretically, the agency benefits from the constant churn of program managers, with infusions of new ideas on a regular basis. For the individuals, the short term contract means that the position is not considered a destination, but rather a prestigious appointment that can serve as a stepping stone to top-tier science and engineering leadership positions.

Flexible contracting. Awardees at ARPA-E and DARPA may be funded through common mechanisms (grants, contracts, cooperative agreements), but both DOD and DOE also provide agencies "other transaction authority" or OTA, which they may use in cases where the terms of traditional agreements are not suitable. Examples include transactions for prototyping, or

technology investment agreements (TIA) with customized terms related to intellectual property (Institute of Medicine, 2012).

Bottom-Up Program Design

> "I have described the role of DARPA PM like that of a conductor of an orchestra. The conductor does not play any instrument, but he must understand every instrument and the music. He brings all the harmony together." – DARPA Program Manager

Using the flexibility of the organization as described above, program managers at ARPA-like agencies are able to pursue technical areas based on the real-time changing need for innovation and opportunities for progress. These short-lived technical programs contribute to the reputation for agility at ARPA-like agencies, as they can redirect resources with relative speed. This bottom-up approach to designing technical programs is the second core element of the ARPA model.

At the start of an ARPA-E program director's three-year term, they are tasked with designing a technical program—a specific challenge under which a set of projects will be funded, e.g. $30 million for a program that may award 10 teams with $3 million each. Program directors engage with their target research communities and host stakeholder gatherings to refine their idea, and then they pitch their program to ARPA-E leadership, who either accept the idea or encourage further exploration. This process can take up to 18 months of a 36-month contract, so each program director typically creates one program during their tenure. With 15 or so program directors at a time at ARPA-E, and each program consisting of projects that are typically 3 years long, there have already been dozens of programs completed in the short lifespan of the agency.

DARPA has used similar processes for program creation. DARPA program managers consult with military liaisons to understand their mission-related needs, and they convene scientists to brainstorm new ideas (Fuchs, 2010). When choosing an area of "white space" in which to operate, program directors are not confined to their own specific area of research; instead, they consult with industry and other experts, and they seek to address bottlenecks in the innovation system. They craft programs to tackle areas where a need exists and where a targeted investment could make a significant difference. The agency's administration may seed this process by hiring program directors from a certain technical background, e.g. ARPA-E hiring someone with

experience in geology in hopes that they will explore advanced geothermal energy, but the program directors are then allowed to explore with relative freedom.

ARPA programs are often designed with specific technical targets. For example, the ARPA-E Grid-Scale Rampable Intermittent Dispatchable Storage (GRIDS) program was created in 2010 to support novel energy storage or battery concepts, but a pre-requisite for funding was a cost projection of $100 per kilowatt-hour—a step-change in storage costs that would fundamentally transform the electricity industry. Interestingly, the cost threshold set in this program has become the *de facto* benchmark for industry costs going forward, as well.[3] The specificity of these targets at the program-level translates to concrete benchmarks for success of each project, which is critical for active project management, discussed later in this section. These targets also ensure that each program is directly aligned with some component of the agency's mission.

The structure of bottom-up program creation is key to the ARPA model, because it enables program managers to deliberately connect people who are otherwise unconnected and shape the direction of research. As one program manager described this process, "You get communities together that don't naturally talk and you give them some latitude and some life, and you push them forward and see what comes out of it" (Fuchs, 2010). Beginning with pre-program workshops and continuing through annual meetings of ARPA awardees, a community of people is built around a particular goal, and this function can have lasting impact beyond the specific outcomes of any particular project.

Discretion in Project Selection

The third important feature of the ARPA model is the use of discretion by program directors to decide how to allocate funds within a program. Any research funding program must tackle fundamental questions: Which researchers should we support? Who can offer the greatest return on investment? What technologies fit best within the agency's mission? These questions are extremely difficult to answer due to the uncertainty of research outcomes (Rosenberg, 1996). Research creates new knowledge or information, and so by definition, it is impossible to predict the result of a particular project. Furthermore, even if a project meets its technical goals, there is

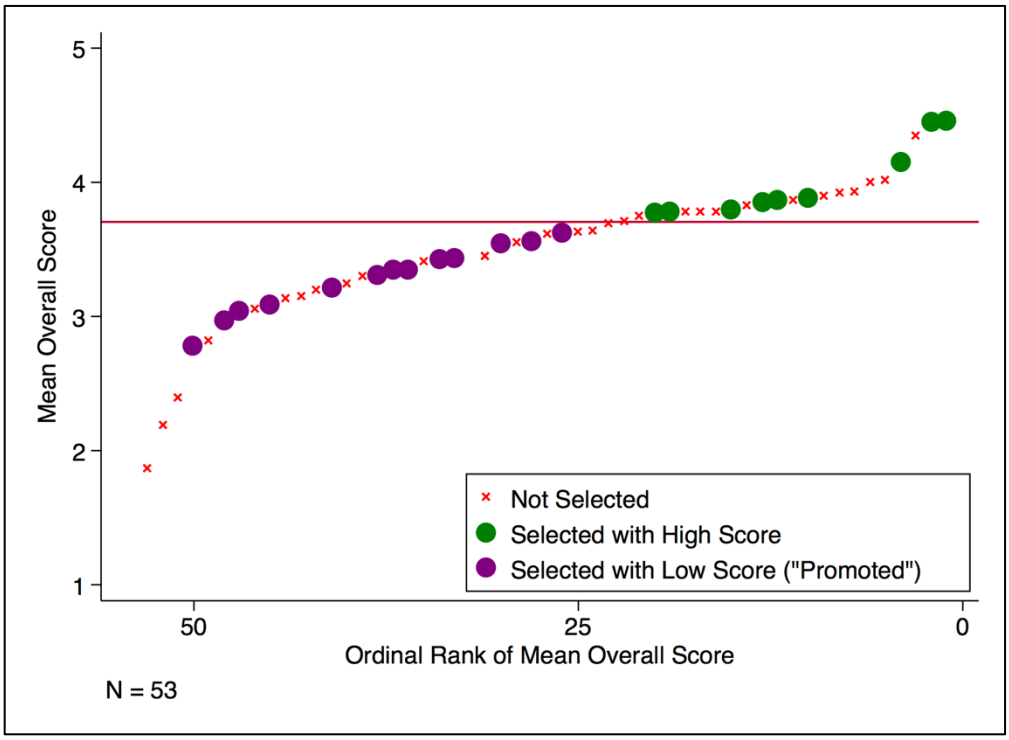
[3] See for example, "Eos Energy Storage drives down costs on battery systems to below $100 per kWh": https://www.renewableenergymagazine.com/energy_saving/eos-energy-storage-drives-down-costs-on-20170419

great uncertainty in how the research will be used and what the impact of that application will be. Regardless of which selection criteria a funding program uses, e.g. scientific merit or applicability (Gans & Murray, 2012), the inherent uncertainty of research presents serious difficulty for evaluating proposed projects.

Given this inherent uncertainty, the most common method of coping with technical uncertainty is peer review, i.e. to solicit opinions of the technical proposals from a panel of expert reviewers, who are presumably the most informed and least burdened by uncertainty. Of course, subject matter experts may disagree, so programs typically combine multiple opinions in order to arrive at a minimally controversial set of proposals. For example, proposals may be sorted on the basis of mean numeric ratings received from the review panel. There exists a vast literature on the deficiencies of peer review for research proposals, such as its biases based on demographics, seniority and, importantly in this context, novelty (Boudreau et al, 2016). This bias against novelty is particularly concerning for research organizations that are committed to pursuing transformative research, e.g. ARPA-style programs.

The alternative strategy employed in the ARPA model allows program directors to use their own expert judgment in deciding which proposals to fund. Program managers at DARPA have great latitude in selecting awardees and distributing funds without soliciting external peer review. ARPA-E tends to be under greater scrutiny than DARPA—clean energy being more politically vulnerable than defense—and so it has adopted a more moderate strategy of collecting external peer reviews as informational inputs. Goldstein and Kearney (2018a) describes how ARPA-E program directors select projects from across the distribution of review scores, i.e. average review scores are only slightly correlated with an increased probability of selection. The authors construct a counterfactual cut-off line for review scores, below which an application would be rejected by a selection method based on ranking average scores. In the example program shown in Figure 2 below, approximately half of selected projects were selected despite an average score lower than 3.7; across the entire ARPA-E portfolio 2009-2015, approximately half of all projects were similarly “promoted” (Goldstein and Kearney, 2018a).

Figure 2: Review Score Distribution and Project Selection for an ARPA-E Technical Program



Rather than adhering to peer review scores, some program directors report selecting projects based on the review comments instead (Goldstein and Kearney, 2018a). Program directors at ARPA-E and DARPA often balance programs with complementary, and potentially competitive, technologies, in order to create a diverse portfolio (Fuchs, 2010). This use of individual discretion results in a portfolio of projects that carry a higher degree of uncertainty, as defined by disagreement among reviewers. To the extent that uncertainty is a corollary to novelty, individual discretion is an antidote to novelty bias in peer review.

In addition to using discretion to select awardees, ARPA program staff take an additional step of actually engineering the teams that submit proposed ideas. At ARPA-E, program directors work to create connections between different researchers with complementary expertise, such as introducing academics to potential commercial partners. At DARPA, evidence suggests that program managers are even more involved in the engineering of research teams. Berlin (2017) documents the case of ARPANET, where the program manager decided to award the contract to an assembled team of performers including Bolt, Beranek and Newman, rather than to Raytheon. Ultimately, the program manager believed the assembled team to be a better cultural fit.

Active Project Management

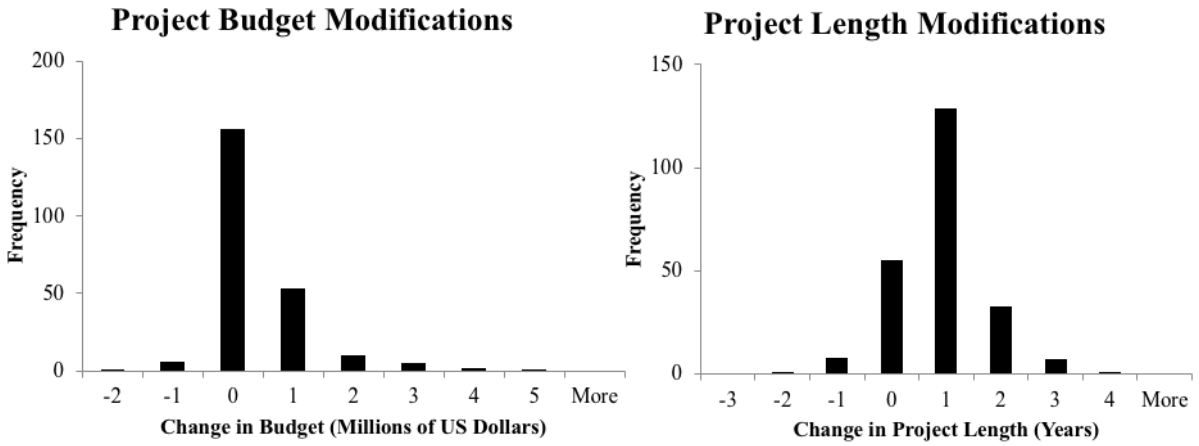
The fourth core element of the ARPA model is active project management, which entails empowering program staff to make decisions related to capital, tasks, milestones and technical goals throughout the project. The practice of active project management is supported by Huchzermeier and Loch (2001), who detail the importance of maintaining flexibility for a firm through the use of "real options" for managing a research project, such as delay, abandon, contract, expand and/or switch. Similarly, Trigeorgis (1997) finds that flexibility improves the potential upside of a project while limiting downside losses. Dixit and Pindyck (1994) find that as uncertainty in project payoffs increase, additional measures should be taken by managers to maintain flexibility.

ARPA agencies stand out among other grant-making organizations in the public sector for their choice to retain control rights over the research after allocating funds. The conventional approach is to allow researchers to manage their own research process independently. This approach is especially well-supported for academic researchers, who value having creative control over their research directions (Aghion et al., 2008; Stern, 2004). An often-cited model of investigator independence is the Howard Hughes Medical Institute (HHMI), which funds "people, not projects" and gives its awardees authority to choose their own research direction. Azoulay et al. (2011) found that HHMI awardees, who are free to choose and modify their own research directions, publish more high-impact papers compared to similarly qualified scientists funded by the National Institutes of Health (NIH) with pre-defined project aims.

In this context, reducing the level of authority and flexibility held by funded researchers might be expected to hinder exploratory research (Manso, 2011). However, at ARPA agencies, this loss coincides with the empowerment of ARPA program managers. In the ARPA model, the nexus of exploration occurs first and foremost at the level of the program director, through the program creation and project selection processes described above. The program directors themselves have tolerance for failure as they make decisions, i.e. setting the technical goals, selecting a preferred set of projects, and managing those projects along the way. For this reason, hiring talented program staff with a penchant for exploration is pivotal to the success of ARPA-like programs.

Over its history, DARPA's project management practices have varied between more and less active; milestones and "go/no-go" reviews were emphasized under Directors Heilmeier (1975-1977) and Tether (2001-2008) (Fuchs 2010). At ARPA-E, program directors remain closely engaged with researchers over the course of a project, receiving quarterly progress updates and giving feedback on the same schedule (Goldstein and Kearney 2018b). Program directors frequently revise project milestones, budget, and timeline for deliverables; in some cases, they cancel projects that are not able to meet technical milestones.

Figure 3: Award Amount and Project Length Modifications at ARPA-E



As seen in Figure 3, budgets and project lengths are more likely to be expanded than to be cut. These adjustments correlate significantly with overall quarterly status ratings; projects rated well are likely to see their budgets increased, and projects that miss major milestones are far more likely to be terminated. By cutting projects short in response to poor performance, the agency is able to mitigate any increased risk in their portfolio caused by funding more controversial projects.

Complementarities

So far in this section, we have outlined the four policy decisions related to organizational structure, program design, project selection and project management that form the ARPA model. Together these choices lodge much more control and decision-making power with the funding organization than is typical for public research funders. We propose that this is no coincidence—

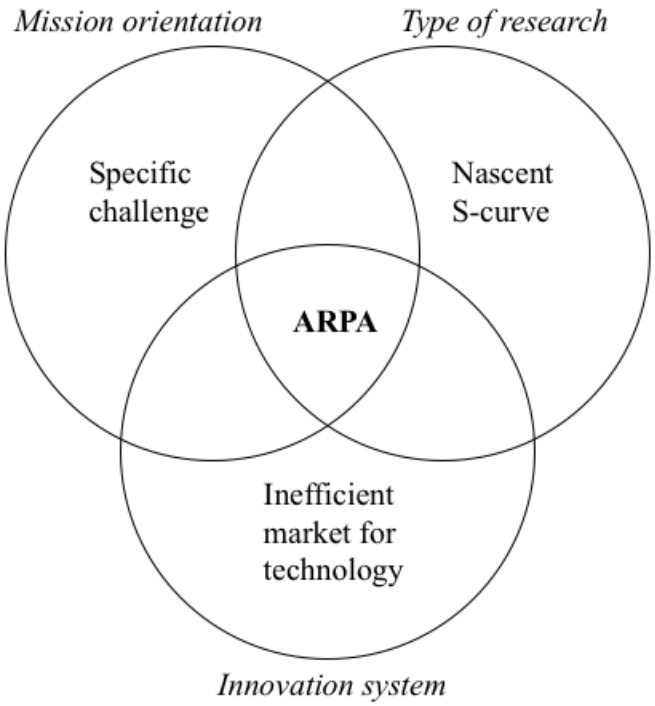
the elements of the ARPA model may comprise a bundle of complementary practices. Milgrom and Roberts (1990) articulate both the benefits of complementary elements within a bundle of organizational practices and the drawbacks to selecting elements of a bundle that do not cohere; many examples of complementary business practices have been identified (Brynjolfsson and Milgrom, 2013). The parameters of innovation management discussed in this section may function the same way: adopting one feature increases the return to adopting another.

Each of the four elements of the ARPA model relies on a highly talented, independent, and empowered program staff. These individuals, having been given great autonomy in some aspects of their position, are better prepared (and may be expecting) to exercise autonomy in other aspects as well. For this reason, mixing and matching among these elements would be less effective than implementing them all together. A program that uses individual discretion to select funding recipients may underperform in the absence of active project management, as in the NIH postdoctoral fellowship program studied by Heggeness et al. (2018). These interactions among policy choices are as yet unexplored for R&D programs and are an important area for future research.

4. What is ARPA-ble?

In the previous section, we described four elements of the ARPA model and proposed that these four are complements with respect to empowerment of the program staff. The obvious next question is: when should this particular set of research management practices be chosen over another? In short, what is ARPA-ble? We draw attention here to three important characteristics of a research area that determine whether it is appropriate for an ARPA agency: the type of research being conducted, the mission orientation of the funding organization and the status of the broader innovation system within which technology is researched, developed and deployed (Figure 4).

Figure 4: What is ARPA-ble?



A simple place to begin this discussion is with the opposite question: what is *not* ARPA-ble? On one hand, it is clear that the ARPA model is not appropriate for so-called basic research, which "is performed without thought of practical ends" (Bush, 1945). Research efforts that are far upstream from any commercial activities, e.g. high energy physics, are not a match for ARPA-like agencies, which are explicitly organized around a technology-related mission.

Mission Orientation

The first requirement for an ARPA-ble challenge is the nature of the mission. According to Sen (2013), transformational innovation programs are targeted specifically toward desired social or economic outcomes, rather than intangible goals or undefined technical development. Mission orientation serves as a unifying element within the ARPA organization. The agency's mission is the foundation of the program design process described in Section 3. A program manager envisions a particular innovation or technological change, and then manages the program in pursuit of breakthroughs to enable this change (Carleton 2010). Bonvillian (2009) calls this type of challenge-based model "right-to-left," in that it moves opposite the archetypal innovation

pipeline, starting with a desired technology and working backwards to determine the research needs.

Not every technical area united by a specific mission or challenge is a good fit for the ARPA model. The mission must be associated with quantifiable goals and traceable progress. Technical pursuits like the NIH BRAIN Initiative, with amorphous goals ("Revolutionizing our understanding of the human brain"[4]), may not be a good fit for an ARPA model organization. The degree to which scientists understand the human brain is not practically measurable. In contrast, one element of ARPA-E's mission is "decreasing our nation's dependence on foreign energy sources," which can be measured over time through metrics such as oil imports. DARPA's mission to avoid technological surprise is measurable through the creation of new tools to promote national security.

Nascent S-Curve

From the discussion of mission orientation, it is apparent that ARPA-ble research is distinct from so-called applied research, which is geared toward performance improvement of existing technologies. The emphasis on risk-taking and novelty make the ARPA model a poor fit for a program focused on extending an existing technological paradigm. But if ARPA agencies do neither "basic" nor "applied" research, then how can we describe their function?

Recent research by Goldstein and Narayanamurti (2018) suggests that ARPA-E occupies a productive middle ground between these two extremes. ARPA-E awards have a higher rate of patenting and the same or higher rate of publishing than similar awards from other branches of the Department of Energy, which are sharply divided between "basic" and "applied". ARPA-E awards are also more likely to produce *both* a patent and a publication, indicating an openness to both invention and discovery. This points to another distinguishing characteristic of ARPA-ble research problems: position on the technology S-curve.

One way to define the ARPA domain takes advantage of the concept of technology S-curves by Foster (1986), where technical performance tends to improve as a sigmoidal function of research effort. Individual technologies progress along an individual S-curve, with occasional discontinuities resulting from a radical breakthrough and creation of a new S-curve. The

[4] https://www.braininitiative.nih.gov

beginning of an S-curve represents a novel concept that has not yet been the focus of much effort. Here, the returns to effort are small. The middle portion of the S-curve includes the inflection point where the returns to effort are highest. The final segment of the S-curve entails diminishing returns to effort and slow improvements to technical performance.

It seems that the ARPA model is optimized for technical areas that reside in nascent S-curves – the technology exists, is relatively unexplored, and has great potential for improvement. The practice of program design identifies those areas; the practice of project selection finds uncertain but promising research efforts in pursuit of the specific technical challenge; and the practice of project management focuses effort as more information becomes known and some ideas begin to show more potential than others. Organizational flexibility gives the agency the ability to quickly respond to opportunities with minimal administrative friction.

It is instructive to consider the ARPA agencies in the taxonomy introduced by Stokes (1997). Research activities are divided across two dimensions: whether the use is considered, and whether fundamental understanding is the goal. In this framework, research is either purely basic (Bohr's quadrant), purely applied (Edison's quadrant), or use-inspired basic research (Pasteur's quadrant). The ARPA model certainly puts "use", i.e. technology, at the forefront—the potential applicability of research is the foundation of the ARPA-like agency's strategy—but what is the role of science in the ARPA model?

At the early stage of an S-curve, there may still be issues of science to be resolved before the technology can progress. Indeed, ARPA agencies are open to advancing the scientific frontier. And yet, because fundamental understanding is not *in itself* a goal of the ARPA-like agency, it is not properly identified as "use-inspired basic research" in Pasteur's quadrant. Instead, the ARPA model occupies a portion of Edison's quadrant wherein the "use" under consideration is still novel and immature. If scientific discovery is necessary for a given research project, then that project may journey incidentally into Pasteur's quadrant as it furthers fundamental understanding.

Inefficient Innovation System

One final important element to consider for use of the ARPA model is the state of the broader innovation system in a given technical area. The economic justification of government funding for research is that private firms would not conduct the same work. The private sector

chronically underinvests in R&D because firms cannot accrue all of the benefit of an innovation. This justification is commonly applied to early-stage, upstream research, and yet it can also apply to later-stage R&D if private markets are not able to successfully move technologies through development, demonstration, and deployment.

Defense is an obvious example of an area where the public sector is necessarily involved in furthering technological progress. In the US, DOD is both a funder of R&D and, ultimately, the user of defense technology. Much has been said about the willingness of the military to pay for innovations resulting from DARPA funding, with some questioning whether the ARPA model could work without a public customer for technology (Mowery 2006). And yet, while the energy industry is very different from defense, it suffers from the same inability of the private sector to produce transformational technology on its own.

The path from idea generation to impact for a new energy technology is extraordinarily arduous. Innovations require large amounts of capital for demonstration and scale-up, and innovators generally have to compete with legacy providers immediately upon market entry. With no clear path to commercialization, and significant regulatory constraints, financing the development of new energy technologies is often extremely challenging. Nonetheless, there are major environmental externalities associated with conventional energy usage, and the social welfare to be gained from a cleaner and more efficient energy economy necessitates public intervention.

5. Challenges for ARPA-Like Agencies

Even in areas where the ARPA model is a good fit, there remain challenges that must be overcome to maximize the effectiveness of the model:

Measuring long-term transformation. The mission of an ARPA-like agency often involves transformation of some industry or market to address a societal need. This transformation invariably occurs over the long-term, and yet, the ARPA model requires that individual projects be held to strict timelines; projects often only run for three years. In the case of ARPA-E, for example, a project that aims to create a new design for an energy storage device may be a tremendous success, and still the impact in the market and/or the environment will not be seen

for decades. This limitation must be kept in mind during short-term efforts to measure the agency's progress toward its mission.

One size does not fit all. ARPA model organizations fund a diverse set of performers, including large firms, academics, start-ups and national labs. They generally have one set of practices that apply equally to all research teams, despite the very different incentives and organizational contexts underlying participation across these different types of performers. Successfully implementing the ARPA model means navigating these differences to avoid conflicting motivations for researchers.

The dark side of discretion. In the ARPA model, program directors have significant freedom to craft programs and select and manage projects, and the Director also enjoys freedoms afforded by the organization's flexibility. As more freedom is given to both the Director and the program staff, the quality of the agency's activities are increasingly dependent on the talent of these individuals. Any research agency requires a Director with strong leadership ability and staff members with high technical acumen, and yet the organizational flexibility and program staff empowerment in the ARPA model raises the stakes on this requirement. The core elements of the ARPA model cannot exist under micromanagement by the Director or with under-qualified program staff.

Tension between autonomy and accountability. Another risk faced by the ARPA model is that, with fewer checks and balances in place, empowered staff members are less accountable for the impact of their decisions. The Director is accountable to Congress and must defend the organization's budget, and the program directors are accountable to the Director, who decides the funding amount allocated to each program. However, the short tenure of program directors means that each program is an isolated game, and success or failure of the program does not necessarily affect the program director's next move. Some accountability is levied informally by the network of program directors within the technical community, in that program directors who are perceived as having good performance can use ARPA as a stepping stone to subsequent high-level R&D management opportunities.

Building trust. The program design and proposal solicitation phases of the ARPA model depend heavily on the willingness of the researcher to share their ideas and their ongoing, and potentially confidential, work. If program directors do not treat these ideas with appropriate care, the agency can lose its reputation as a trustworthy partner (Fuchs, 2010). The agency must

cultivate a healthy relationship with the research community in order to maintain a steady influx of ideas.

Nurturing culture with high turnover. An energetic and enterprising culture is a hallmark of the ARPA model. However, culture is fundamentally a function of the individuals that make up the organization, and in ARPA-like agencies, the program directors work on three-year contracts. It is a challenge to maintain and nurture an organization's culture with such high turnover. Nonetheless, there are a few mechanisms for perpetuating a culture of risk-taking and exploration: first, in the program creation process when new program directors pitch their ideas to existing program directors and the agency's administration, and second, when the program directors meet again with the administration to justify their selections of which proposals to fund. The alumni networks of program directors also help to maintain the reputation and culture of the organization.

Transitioning to market. The focus of the ARPA model is on making progress along technical S-curves, but innovation also requires diffusion of new technologies. Overseeing the transition from research to development is a major challenge for ARPA-like agencies, especially when there is no public customer for the innovation. ARPA-E has a tech-to-market team dedicated to working with all awardees on their strategy for transitioning technologies out of the lab, and yet this aspect of the program needs to be "reconceptualized," according to a recent assessment by a committee of the National Academies (NRC 2017).

6. Conclusion

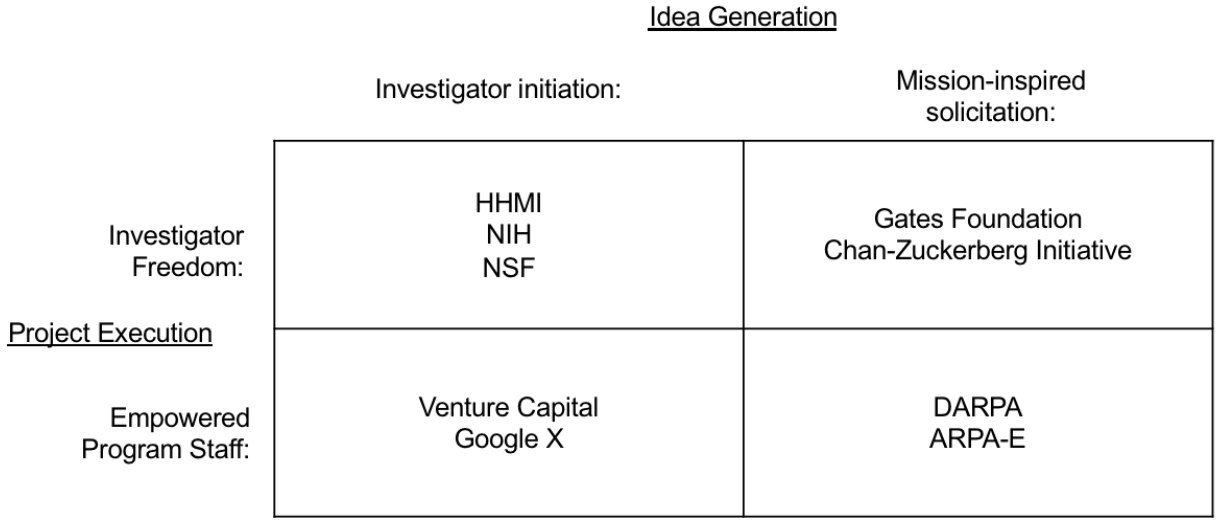
In this paper, we review the key elements of the ARPA model for research funding: organizational flexibility on an administrative level, alongside significant authority vested with program directors to design programs, select projects and actively manage projects. We identify the ARPA model's domain as mission-motivated research on nascent S-curves within an inefficient innovation system. We also describe some of the challenges to implementing the ARPA model.

It is important to note that the ARPA model is but one part a landscape of research funding programs, each with different goals and accordingly different approaches. In Figure 5 below, we depict two dimensions of research management: the source of idea generation, and the locus of control for project execution. The bottom right quadrant represents the ARPA model, based on

two of the four features that we describe above: ideas are submitted in response to mission-inspired programs created by the funder, and projects are then actively managed by the funder.

A complete national innovation system should feature some programs that adhere to the ARPA model, while others fill out the rest of the landscape, producing different types of outcomes and providing different benefits. The top left quadrant represents programs where researchers generate ideas and then pursue them with little oversight and often over long time horizons. HHMI and some programs within NIH or NSF fit this category. Programs in the top right quadrant solicit proposals for a particular mission-inspired area and then allow the researcher to freely explore, as with the Gates Foundation or the Chan-Zuckerberg Initiative. In the bottom left quadrant, researchers propose ideas, but program staff are deeply involved in the execution of the project, as is common in venture capital.

Figure 5: Innovation management strategies



One of the challenges of the ARPA model is the difficulty of evaluating its effectiveness toward long-term goals, and yet the task of evaluation is critically important nonetheless. Following on the recent assessment of ARPA-E, research on the ARPA model should continue to probe the effectiveness of specific features. Within the existing ARPA agencies, there is some low-hanging fruit for evaluation: varying the tenure of program directors, frequency of reviews, duration of projects, and funding levels. Experiments like these would shed more light on the efficacy of the ARPA model and its role in systems of innovation.

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