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Power Play

WILLIAM B. BONVILLIAN

New technology is a key to solving the energy problem, and we have a proven model for generating breakthrough technologies. So why don't we use it?

The idea that technological innovation can be a driver of both winning armies and growing economies is at least as old as the Appian Way. A transportation network very sophisticated for its time, the Appian Way was an accelerator for Roman military prowess and commerce. It allowed Romans to move armies quickly and with better command and control, and it facilitated commerce—fueling a growing economy that sustained the Republic and later the Empire. It was, literally, an early information superhighway.

For nearly the next two millennia the example of the Appian Way inspired imitation. Libraries are full of books that discuss the history of science and technology, and virtually all of them have one thing in common: the conviction that innovation matters, sometimes decisively, in the economic, social, military and political affairs of mankind.

True enough, but something important happened on the way to the 21st century. Even as military technology grew in lethality, it was still very rarely decisive in military or political outcomes. In theory at least, Julius Caesar and George Patton could have sat discussing tactics for desert warfare or crossing the Rhine and understood one another tolerably well. Weapons mattered, but not necessarily more than soldiers' skill, morale, leadership, planning, training, weather and luck. That began to change during World War II, when it first became apparent that new technology by itself—not just more sophisticated implements in the hands of competent soldiers—could win wars. The foremost examples were microwave radar and proximity fuse advances, which emerged from MIT's Radiation Laboratory and, of course, the atom bomb from Los Alamos. These were war-*winning* technologies from which we learned that applied science had reached a stage where it could transform war, and geopolitics with it, in ways heretofore barely imaginable.

The evolution of late-20th-century military technology was part of a much bigger picture of innovation transformation. Carlotta Perez has argued persuasively that, starting with the onset of the Industrial Revolution in Britain in 1770, an industrial transformation has occurred roughly every half century.[1] 1.
Perez, *Technological Revolutions and Financial Capital* (Edward Elgar, 2002). See also Robert D. Atkinson, *The Past and Future of America's Economy—Long Waves of Innovation That Power Cycles of Growth* (Edward Elgar, 2004).
Technology-based innovation cycles have flowed out in long, multi-decadal waves, transforming economies and the way we organize societies around them. Military innovation and power have spun out from these waves in such a way that world military leadership has tended to parallel leadership in technological innovation.

The United States has led the last three innovation cycles, with information technology at the epicenter of the latest wave. As with the Appian Way, the core techniques of the present innovation wave generate mutually reinforcing economic and military advantages. The obvious insight here is that the relative power of political entities has a great deal to do with technological leadership. What is less obvious is that military applications of technological innovation are rarely direct and cannot be sustained in isolation from technological change in society as a whole.

What is also not obvious is that the relative importance of military technology to national power is not constant. The United States today is without question the strongest military power. But even with its immense military power the U.S. government arguably cannot achieve political ends comparable, say, to those achieved by the Wilson Administration in 1917–18. As we think about ways to apply core scientific-technological innovation to U.S. national power today, we clearly do not wish to fall behind others in military sophistication. Force is still the *ultima ratio* in the political affairs of our species, like it or not. But it does not follow that the application of cutting-edge innovation to the military arts is the *only* domain that should concern government.

It doesn't take a rocket scientist, as the aphorism goes, to realize that the United States, its allies and the world at large have a potentially serious energy problem. Economic power is the heart of American soft power and the backbone of its military power; energy has become a potential Achilles' heel to both. The record shows that every presidential administration since that of Richard Nixon has not only acknowledged the problem and understood its broader geopolitical implications, but has pledged to actually do something about it. All of them have failed. The history of U.S. energy policy over the past three decades, under Republican and Democratic stewardship alike, is one of the saddest stories in American political history. For more than thirty years we have understood that science and technology would ultimately provide the basis for a solution to the energy dilemma, yet we have failed to apply to the energy sector the innovation paradigm that keeps the U.S. military the most sophisticated in the world.

That paradigm can be summed up in a single Beltway-savvy acronym: DARPA (the Defense Advanced Research Projects Agency). A question our leaders should be asking, but mostly aren't—especially within this sometimes science-challenged administration—is how the DARPA model can be applied to our energy problem. When politicians make speeches calling for a "Manhattan Project for energy" they are actually onto something—or barely clinging, at least, to the edge of a thought. But few such speechmakers have the slightest idea how the Manhattan Project was created and why it succeeded.

Thanks in significant part to DARPA's lessons, we actually do know a fair bit about the causal factors behind innovation and its successful application. Growth economics teaches that innovation yields growth through two direct factors: state-of-the-art R&D; facilities and the human talent behind that R&D.; There is also a critical third factor, however, which involves not science as such or the fabrication of the hardware derived from it, but rather the institutional setup in which research facilities and human talent best combine. The deliberate creation of the nexus where science and technology is best organized we call "innovation organization."

Innovation organization in turn operates at two interwoven levels: personal and institutional. At the personal level, innovation differs from scientific discovery or invention. Solo operators can produce discovery, but innovation is team-and-network intensive.[2]2.
On innovation and "great groups", see Warren Bennis and Patricia Ward Beiderman, *Organizing Genius* (Basic Books, 1997). On innovation and "collaborative networks", see Robert Rycroft and Don Kash, "Innovation Policy for Complex Technologies", *Issues in Science and Technology* (Fall 1999).
Systemic innovation requires linking scientific discovery to technological invention, and then multiplying applications of breakthrough inventions to

create sharp productivity gains with the potential to transform an economy. This requires deep institutional connections between the “R” and the “D” stages.

The DARPA model, if we understand and apply its innovation organization lessons, has the potential to transform our energy technology dramatically. If U.S. power in this century falls victim to the multiple implications of a global energy situation run amok, we will have no one but ourselves to blame. We therefore need to understand the history and nature of DARPA, distill out its optimal innovation system, and set up as quickly as possible a new innovation system aimed at a range of energy technologies.

Science, Connected and Pipelined

The precursors of U.S. government science and technology organization go back to the Lincoln Administration, when the National Academy of Sciences was created. But for our purposes the relevant history dates from World War II and comes from a kind of Dr. Dolittle “Pushmi-Pullyu” relationship between civilian economic and defense sectors. Acting as President Roosevelt’s personal science executive during the war, Dr. Vannevar Bush led this charge. He was allied to a remarkable group of fellow science organizers, including Alfred Loomis, an investment banker and scientist, Berkeley physicist Ernest Lawrence, and two university presidents: James Conant of Harvard and Karl Compton of MIT.

Vannevar Bush, 1957Time Life Pictures/Getty Images
Vannevar Bush, 1957Time Life Pictures/Getty Images

Loomis was a particularly interesting and critical character in all this. He loved science, but family needs compelled him to become a lawyer.[3] 3. See Jennet Conant, *Tuxedo Park* (Simon & Shuster, 2002). Loomis nevertheless found a way to combine his science and legal skills to become a leading Wall Street financier for the emerging electric utility industry in the 1920s. Anticipating the market crash, Loomis cashed out in 1928 with his great fortune intact, which he then used to set up a private lab at his Tuxedo Park, New York, estate. There in the 1930s Loomis assembled a “who’s who” of pre-war physicists. Loomis’ personal obsession was microwave physics, but his organizational talents were also evident. So as World War II loomed, Vannevar Bush asked Loomis to join Roosevelt’s National Defense Research Council (NDRC) to mobilize scientists for the war effort.

At about this point, one of those inexplicably odd moments in history jumped forth. The U.S. military expressed no interest in Britain's work on microwave radar, fearing they would have to trade U.S. secrets for it. To rescue America from its own short-sightedness, one night in 1940 Loomis took a delegation of British scientists to his penthouse in the Shoreham Hotel in Washington. There, the British handed over to Loomis a suitcase containing their knowledge of microwave radar. With the Battle of Britain raging, Loomis' microwave expertise enabled him to grasp immediately the military implications of the technology for air warfare. He promptly persuaded his cousin and mentor, Secretary of War Henry Stimson (who ever doubted the power of WASP family connections?) that this technology must be developed and exploited without delay. With Bush and Roosevelt's immediate approval, Loomis set up the Radiation Laboratory at MIT in a matter of weeks. Drawing on the connections he had formed at his Tuxedo Park lab, Loomis, along with his friend Ernest Lawrence, was able to convince nearly the entire talent base of U.S. physicists to join the Rad Lab. Because the U.S. government was not accustomed to establishing major labs overnight, Loomis personally funded the startup until government approvals and procurement caught up.

The Rad Lab was non-hierarchical, with only two levels: project managers and project teams. Each "great group" team was devoted to a particular technology path. The lab worked intense and long hours, and did so in high spirits. Loomis and Bush purposely kept it out of military uniform and reach. The Rad Lab used a talent base with a mix of science disciplines and technology skills. It was highly collaborative, organized around a problem-solving science-challenge model, and deployed connected-science management to move from fundamental breakthrough to development, prototyping and initial production.[4] 4.

The norms of the Rad Lab's "great groups" are common to other innovations—both before and after—including the lightbulb at Edison's Menlo Park "Invention Factory", the transistor at Bell Labs, the integrated circuit and microchip efforts at Fairchild Semiconductor and Intel, the personal computer at Xerox Parc and Apple, and biotech advances at Genentech and Craig Venter's genomics projects. Venture capitalists typically try to find groups with similar characteristics.

Before long, the Rad Lab had developed microwave radar and other advances that led to the proximity fuse. The Rad Lab produced 11 Nobel laureates, formed the organizational model for Los Alamos, and laid some of the foundations for modern electronics. It also embodied another common feature of successful groups: The Rad Lab had direct access to the top decision-makers, including the president and the secretary of war.

As Loomis and his colleagues constituted the core talent reservoir, Vannevar Bush created the organizational foundation—first the NDRC and then the Office of Science Research and Development (OSRD)—for this talent to succeed. Bush brought all defense research efforts under one loose coordinating tent and set up non-bureaucratic, interdisciplinary project teams oriented to the major technology challenges of the day as implementing task forces. He created "connected science", where technology breakthroughs at the fundamental science stage were closely linked to follow-on applied stages of development, prototyping and production, operating under what we may call a technological-challenge model. Because Bush and Loomis could get direct support from President Roosevelt through Secretary Stimson and presidential aide Harry Hopkins, Bush made his organizational model stick throughout the war, despite relentless pressure from the uniformed services—especially the U.S. Navy—to capture it.

Immediately after the war, Bush systematically dismantled his remarkable connected-science creation. Envisioning a period of world peace, he was convinced that wartime levels of government science investment would be slashed. He was also probably wary of a permanent alliance between the military and science. Bush decided, however, to try to salvage some residual level of Federal science investment. He had written for Roosevelt in late 1944 the most influential polemic in the history of American science: "Science: The Endless Frontier." In that masterful essay Bush argued that the U.S. government should fund basic research, which would deliver continual progress in economic well-being and national security. In other words, he proposed ending his wartime model of connected-science research and development, organized around major technology challenges, in favor of making the Federal role one of funding only a single stage of technology advance: basic research.

Bush's approach became known as the "pipeline" model for science investment. The Federal government would dump basic science into one end of an innovation pipeline. Somehow, early- and late-stage technology development and prototyping would occur inside the pipeline, and new technology products would magically emerge at the other end. Because he had assembled a connected-science/challenge model during World War II, Bush must have realized the problems inherent to the pipeline model, but he probably reasoned that salvaging Federal basic-research investment was the best he could achieve in the coming period of peace.

Bush did argue that this basic research approach should be organized and coordinated under "one tent" to direct all the nation's research portfolios. To this end he proposed what became the National Science Foundation (NSF). Because he deeply desired this entity to be controlled by a scientific elite separate from the nation's political leadership (and certainly separate from its generals and admirals), Bush fell into a quarrel with Roosevelt's successor, Harry Truman. In his characteristically feisty, take-charge way, Truman insisted that the scientific buck would stop on his desk, not on that of some Brahmin scientist. Truman wanted key NSF appointments to be controlled by the president; Bush disagreed. Truman therefore vetoed Bush's NSF legislation, stalling its creation for another five years.

Meanwhile, science and science organizing in the U.S. government did not stand still. New agencies proliferated and the outbreak of the Korean War led to a renewal of defense-science efforts. By the time NSF was established and funded in 1950, its potential coordinating role had in effect been bypassed. It also became a much smaller agency than Bush anticipated, and only one among many—Bush's "one tent" model had gone by the boards. Instead, the government adopted a highly decentralized model for its science endeavors.[5] 5. There are advantages to decentralized science. It creates a variety of pathways to scientific advance and a series of safety nets to ensure that multiple routes can be explored. Since scientific success is unpredictable, the "science czar" approach risks major failures that a broad front of advance does not. Nonetheless, the United States largely lacks the ability to coordinate its science efforts across agencies, particularly where advances that cut across disciplines require coordination and learning from networks. The solution is to better coordinate R&D; across stovepipes without centralizing control. The current multi-agency nanotechnology effort marks one such attempt. Bush's concept of Federal funding focused on basic science did prevail, however, as most of the new science agencies adopted the pipeline model.

These twin developments left U.S. science fragmented at the institutional level in two ways: Overall science organization was split among numerous science agencies, and Federal investment was focused only on one stage of the technological pipeline—exploratory basic research. Bush thus left a legacy of two conflicting models for science organization: the connected, challenge model of World War II, and the basic science-focused, disconnected, multi-headed model of postwar U.S. science institutional organization.

DARPA Rising

DARPA reversed this legacy of convolution and confusion. President Eisenhower created DARPA in 1957 to be a unifying force for defense R&D.; Eisenhower, who also initiated the Solarium exercise in 1953 that led to the first articulation of a coherent U.S. strategy for the Cold War, rarely gets credit for being an organizational master—but a master he was. Eisenhower beheld the military services' stovepiped, disconnected space programs that had led to America's Sputnik failure and demanded change.

Thanks to Eisenhower's initiative, DARPA became a unique entity. In many ways, the agency directly inherited the connected-science, challenge and great-group organization models of the Rad Lab and Los Alamos. However, unlike these models, which only operated on the personal level, DARPA has operated at both the institutional and personal levels. DARPA became a bridge connecting the institutional and personal organizational elements unlike any other R&D; entity in government.

The DARPA model is perhaps best illustrated by one of its most successful practitioners, J.C.R. Licklider. As a DARPA project manager, Licklider founded and worked with a series of great technology teams, laying the foundations for two of the 20th century's technology revolutions—personal computing and the Internet.[6] 6. These details are from Licklider's biography by M. Mitchell Waldrop, *The Dream Machine* (Viking, 2001).

In 1960, Licklider, who was trained in psychology as well as physics and mathematics, wrote about what he called the "man-machine interface" and "human-computer symbiosis": "The hope is that in not too many years, human brains and computing machines will be coupled together very tightly, and that the resulting partnership will think as no human brain has ever thought."[7] 7.
Licklider, "Man-Computer Symbiosis", *IRE Transactions on Human Factors in Electronics* (March 1960). He envisioned real-time personal computing (as opposed to the then-dominant mainframe computing model), digital libraries and the Internet (he called it the "Intergalactic Computer Network"). He also foresaw most of the personal computing functions we now take for granted—graphing, simulations, modeling and more.

These insights served Licklider well in the new assignment coming his way. President Kennedy and Defense Secretary Robert McNamara were deeply frustrated by the profound command and control problems they encountered during the Cuban Missile Crisis, particularly the inability to obtain and analyze real-time data and interact with on-the-scene military commanders. DARPA asked Licklider to tackle the problem. Strongly backed by early DARPA directors Jack Ruina, Charles Herzfeld and George Heilmeir, Licklider stood up a remarkable support network of early information technology researchers at universities and firms that, over time, built the sinews of personal computing and the Internet.

At the institutional level, DARPA and Licklider became a collaborative force throughout the 1960s and 1970s among Defense Department research agencies controlled by the uniformed services. They used DARPA investments to leverage their participation to solve common problems using the connected-science and technological challenge models. DARPA and Licklider also kept their own research bureaucracy to a bare minimum, using the service R&D; agencies to carry out project management and administrative tasks. Institutionally, DARPA became more a research supporter and collaborator and less a rival to the Defense Department research establishment. DARPA also provided an institutional example within the Defense Department for creating a flexible, cross-agency, cross-discipline model among separate U.S. R&D; agencies. At the personal level meanwhile, Licklider created not only a remarkable base of information-technology talent within DARPA, but also, through the vehicle of DARPA contracts, a major collaborative network of great research groups around the country.

Even that is not all. Because DARPA was willing to patiently nurture long-term R&D; investments in a way that corporations and venture capital firms were not, Licklider's DARPA model came with a native capacity for self-renewal. DARPA internally institutionalized innovation so that successive generations of talent would sustain the IT technology revolution over the long term. The great groups Licklider started shared key features of the Rad Lab group that came before; his Information Processing Techniques group remains the first and greatest success of the DARPA model. But this was not its only victory. DARPA also achieved similar accomplishments in other technology areas, supporting remarkable advances in such areas as stealth, high-energy lasers, robotics, and computer hardware, software and chip fabrication.

Finally, DARPA was eager to catalyze technology advances not only in the defense sector but in the non-defense economy as well. Its directors, senior scientists and managers recognized that an entire economy has to embrace innovation for the defense sector to thrive. The Department of Defense was thus able to take advantage of a broad acceleration of technology development. By seeding the private sector, DARPA reduced DoD's development and acquisition costs over a range of military-relevant technologies. The Defense Department also acquired assets it never dreamed of. When Andrew Marshall, DoD's legendary in-house defense theorist and head of its Office of Net Assessment, argued in the late 1980s that U.S. forces were creating a “revolution in military affairs”, this defense transformation was built around many of the IT breakthroughs DARPA initially sponsored. At the same time, IT innovations originally sponsored because of their military utility ended up spurring an unprecedented innovation wave that swept into the U.S. economy in the 1990s, creating strong productivity gains and new business models in dozens of industries. These have led to a vast creation of new societal wealth that, in turn, is still funding ongoing defense transformation. DARPA has created, in short, a new Appian Way.

The Sea Shadow, an experimental stealth craft based on DARPA technologyU.S. Navy
The Sea Shadow, an experimental stealth craft based on DARPA technologyU.S. Navy

The Innovation Model

What, then, does a successful innovation organization look like “in the raw”, so to speak? If the U.S. government ever finds the good sense to apply the DARPA model to our energy problem, what would, or should, the skeletal organization of a “Manhattan Project for energy” look like?

As DARPA has shown, it would have to work at two levels: the institutional and the personal. And it would be wise to take to heart DARPA’s own 12 organizing elements:[8] 8.
Descriptions taken from *DARPA—Bridging the Gap, Powered by Ideas* (February 2005); and *DARPA Over The Years* (October 27, 2003).

- Small and flexible: DARPA consists of only 100–150 professionals; some have referred to DARPA as “100 geniuses connected by a travel agent.”
- Flat organization: DARPA avoids military hierarchy, essentially operating at only two levels to ensure participation.
- Autonomy and freedom from bureaucratic impediments: DARPA operates outside the civil-service hiring process and standard government contracting rules, which gives it unusual access to talent, plus speed and flexibility in organizing R&D; efforts.
- Eclectic, world-class technical staff: DARPA seeks great talent, drawn from industry, universities, and government laboratories and R&D; centers, mixing disciplines and theoretical and experimental strengths. This talent is hybridized through joint corporate-academic collaborations.
- Teams and networks: At its very best, DARPA creates and sustains great teams of researchers that are networked to collaborate and share in the

team's advances, so that DARPA operates at the personal, face-to-face level of innovation. It isn't simply about funding research; its program managers are dynamic playwrights and directors.

- Hiring continuity and change: DARPA's technical staff are hired or assigned for three to five years. Like any strong organization, DARPA mixes experience and change. It retains a base of experienced experts who know their way around DoD, but rotates most of its staff from the outside to ensure fresh thinking and perspectives.
- Project-based assignments organized around a challenge model: DARPA organizes a significant part of its portfolio around specific technology challenges. It works "right-to-left" in the R&D; pipeline, foreseeing new innovation-based capabilities and then working back to the fundamental breakthroughs that take them there. Although its projects typically last three to five years, major technological challenges may be addressed over longer time periods, ensuring patient investment on a series of focused steps and keeping teams together for ongoing collaboration.
- Outsourced support personnel: DARPA uses technical, contracting and administrative services from other agencies on a temporary basis. This provides DARPA the flexibility to get into and out of a technology field area without the burden of sustaining staff, while building cooperative alliances with the line agencies it works with.
- Outstanding program managers: In DARPA's words, "The best DARPA Program Managers have always been freewheeling zealots in pursuit of their goals." The DARPA director's most important job historically has been to recruit highly talented program managers and then empower their creativity to put together great teams around great advances.
- Acceptance of failure: At its best, DARPA pursues a high-risk model for breakthrough opportunities and is very tolerant of failure if the payoff from potential success is great enough.
- Orientation to revolutionary breakthroughs in a connected approach: DARPA historically has focused not on incremental but radical innovation. It emphasizes high-risk investment, moves from fundamental technological advances to prototyping, and then hands off the production stage to the armed services or the commercial sector. From an institutional innovation perspective, DARPA is a connected model, crossing the barriers between innovation stages.
- Mix of connected collaborators: DARPA typically builds strong teams and networks of collaborators, bringing in a range of technical expertise and applicable disciplines and involving university researchers and technology firms that are usually not significant defense contractors or beltway consultants (neither of which focus on radical innovation). The

aim of DARPA's "hybrid" approach, unique among American R&D; agencies, is to ensure strong collaborative "mindshare" on the challenge and the capability to connect fundamentals with applications.

A DARPA Energy Franchise

The challenge before us now is to take these 12 essentials of innovation organization and create a new agency for energy technology innovation—perhaps associated with a reinvigorated Department of Energy—that can do for energy innovation what DARPA has done for military innovation. Alternative-energy technology evolution has been sporadic and technology transition has been glacial; a connected DARPA model is a way to attack both problems. The National Academy's noteworthy 2006 report, *Rising Above the Gathering Storm*, has called for exactly this. They call it—surprise!—ARPA-E: Advanced Research Projects Agency–Energy.

ARPA-E is now rattling around Congress in various bills, and that is good. But it is clear that any legislation designed to set up ARPA-E must mandate DARPA-like characteristics from the outset. Legislation that is too generally drawn and given over to bureaucrats to flesh out will almost certainly lead to the wholesale violation of the model characteristics listed above, and thence to the headlong failure of the entire enterprise. For example, legislation has to stipulate a flat entity, with only two levels to ensure productive collaboration. Project managers must be left in control of their R&D; agendas and budgets. There must be absolutely no budget-office layer between the director and project managers. It is also crucial that any ARPA-E director have direct and prompt access to departmental leadership; ARPA-E must not become a subordinate office to a larger R&D; entity at the departmental level.

Obviously, however, significant differences exist between the environment in which DARPA has operated and those in which a DARPA energy clone would operate. DARPA launched its breakthrough technologies in IT largely into niche sectors that faced limited initial competitive pressures and could be supported by the new model DARPA itself helped to encourage—of startups, entrepreneurs, venture capital and angel capital. Some new disruptive energy technologies could be launched into this niche realm, but others face profound competitive pressures from an entrenched energy sector that will resist them. There is also no single energy-technology silver bullet. Energy is a highly complex system so the single technology focus of the Manhattan Project won't work. We need a range of new technology introductions to meet needs in transport, electricity and efficiency.

Another difference is that DARPA has an initial “customer” for many of its products. The DoD procurement base is, after all, enormous. ARPA-E’s eventual products could have a significant government-based customer if, for example, Congress ordered all new Federal construction to integrate solar nanotechnology membranes for electrical power generation or if it directed military transport to slash its fuel consumption with hybrids featuring powerful new nanotech batteries. But ARPA-E would not have a government customer base nearly as large as DARPA’s. Nevertheless, even in some niche areas, it could have a non-government customer base orders of magnitude larger than DoD. Because of the complexity of the energy sector a new energy R&D; entity is only part the puzzle, but it is a critical initial step—new technologies are the prerequisite to other governmental interventions.

Given these realities, it would be wise to begin construction of ARPA-E by seconding seasoned veterans of DARPA to it. An agency is its culture, not just its enabling statute or organizational chart. Only those who have worked within the DARPA culture understand it well enough to lead and mentor the first generation of ARPA-E senior staff. (A major error was made when the Department of Homeland Security’s Science and Technology Directorate, which Congress mandated to stand up a DARPA clone, failed to empower its ex-DARPA veterans.)

What are the institutional barriers to a DARPA clone at the Department of Energy? The first barrier is fear. The existing national energy labs, no longer needing to work flat out on building new generations of nuclear weapons and searching for new missions, dread an in-house competitor. To survive at DoE, ARPA-E instead will have to be perceived as a collaborator, potentially seeding new technologies with them, just as DARPA seeded the established service R&D; organizations. It cannot become just another DoE lab or support its own infrastructure; it must be light and flexible, operating as a connector for the established labs and building strong teams of personal-level tech enablers.

ARPA-E can’t simply fund existing labs, either. It will have to break some lab china, like DARPA, by providing strong funding for competing corporate-academic research groups. The best lab talent understands that the labs need more competitive pressure because too little technology is transitioning from them. The ARPA-E name already carries heavy baggage within the Department of Energy, but it can be changed. The important thing is not the name, but rather to understand how and why the DARPA model has worked so well, and to adopt the right form of its 12 essentials for success. Given the vast size of the current energy infrastructure and capital plant, even if we start to realize breakthrough innovation, pervasive deployment will be slow. So we need to begin as soon as possible.

Finally, there is no inherent reason why other DARPA clones—for bio-sciences under the heavily stovepiped National Institutes of Health, for example—could not also be created. That would depend, of course, on leadership in both the Executive and Legislative branches. If the current Administration and congressional leadership do not appreciate the importance of bold investment in the future of American science and technology—and clearly there is a problem here, with the Administration's first, extremely modest competitiveness initiative still languishing in Congress after six years in office—perhaps another era of political leadership will. It is not simply a matter of R&D; investment levels; innovation organization is also important. We must apply the organizational lessons we have already learned.

How important is innovation organization to America's national power and economic health? Let's end as we began, by considering ancient Rome. Roman children played with a toy called an aeolipile, made up of a metal ball suspended by pins on each side so that it could spin freely with directional nozzles on the top and bottom. When the water in the ball was heated, steam would jet out and spin the ball. The aeolipile was, in short, a rudimentary steam engine.

Imagine if some innovative Roman had envisioned this child's toy enlarged and hooked to a set of wheels moving under its own power on the Appian Way. As it happened, there was no such Roman. Rome lacked the scientific institutions to capitalize on this latent technology, precisely the function for which DARPA has been organized. Think of the loss that results when a society fails to dedicate itself to innovation, even when the organizational tools are at hand. What a waste, and how embarrassing to posterity.

1.
Perez, *Technological Revolutions and Financial Capital* (Edward Elgar, 2002). See also Robert D. Atkinson, *The Past and Future of America's Economy—Long Waves of Innovation That Power Cycles of Growth* (Edward Elgar, 2004).

2.
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William B. Bonvillian directs the Washington office of the Massachusetts Institute of Technology. He previously served as legislative director and chief counsel to Senator Joe Lieberman (D-CT). The views in this article are his own.