

# Spotlight

ARTWORK Monika Grzymala, *The River*, 2012  
Handmade cotton rag paper, fishing line and wire,  
weaving material of local plants





**Regina E. Dugan** and **Kaigham J. Gabriel** were formerly the director and deputy director of the Defense Advanced Research Projects Agency. Dugan is currently a senior vice president at Google's Motorola Mobility business, where she leads the Advanced Technology and Projects (ATAP) group. Gabriel is a corporate vice president at Motorola Mobility and the deputy director of ATAP.



# “Special Forces” Innovation: How DARPA Attacks Problems

*by Regina E. Dugan and Kaigham J. Gabriel*

Over the past 50 years, the Pentagon’s Defense Advanced Research Projects Agency (DARPA) has produced an unparalleled number of breakthroughs. Arguably, it has the longest-standing, most consistent track record of radical invention in history. Its innovations include the internet; RISC computing; global positioning satellites;

stealth technology; unmanned aerial vehicles, or “drones”; and micro-electro-mechanical systems (MEMS), which are now used in everything from air bags to ink-jet printers to video games like the Wii. Though the U.S. military was the original customer for DARPA’s applications, the agency’s advances have played a central role in creating a host of multi-billion-dollar industries.

What makes DARPA’s long list of accomplishments even more impressive is the agency’s swiftness, relatively tiny organization, and comparatively modest budget. Its programs last, on average, only three to five years. About 100 temporary technical program managers and a vibrant mix of contract “performers”—individuals or teams drawn from universities, companies of all sizes, labs, government partners, and nonprofits—do the project work. The support staff comprises only 120 people in finance, contracting, HR, security, and legal. The annual budget for the roughly 200 programs that are under way at any given time is about \$3 billion. With its unconventional approach, speed, and effectiveness, DARPA has created a “special forces” model of innovation.

Not surprisingly, in recent decades there have been many attempts to apply the DARPA model in other organizations in the private and public sectors. All those efforts—or at least the ones with which we’re familiar—have had mixed results or failed. These disappointments have led people to conclude that the successes of this extraordinary agency simply can’t be replicated outside the Department of Defense.

We disagree. We led DARPA from mid-2009 until mid-2012. Since then, we have been implementing the agency’s model of innovation in a new organization—the Advanced Technology and Projects (ATAP) group at Motorola Mobility, which was acquired by Google in May 2012. We believe that the past efforts

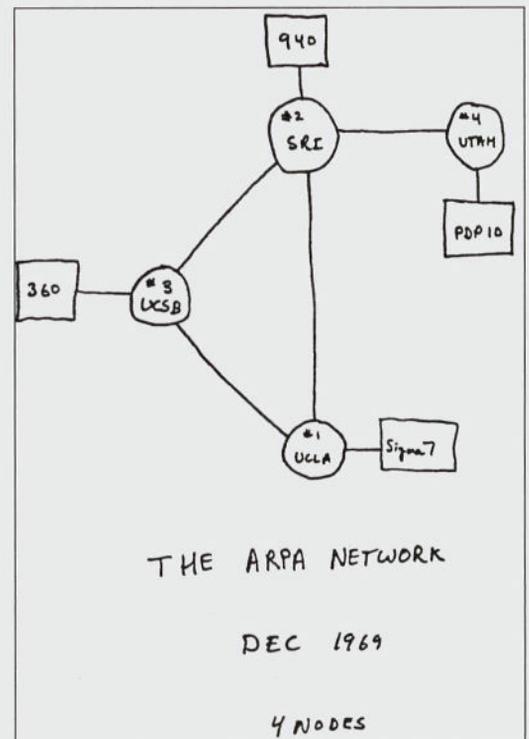
failed because the critical and mutually reinforcing elements of the DARPA model were not understood, and as a result, only some of them were adopted. Our purpose is to demonstrate that DARPA’s approach to breakthrough innovation is a viable and compelling alternative to the traditional models common in large, captive research organizations.

The DARPA model has three elements:

**Ambitious goals.** The agency’s projects are designed to harness science and engineering advances to solve real-world problems or create new opportunities. At Defense, GPS was an example of the former and stealth technology of the latter. The problems must be sufficiently challenging that they cannot be solved without pushing or catalyzing the science.

**THE INTERNET**

In 1969, DARPA launched ARPANET, the predecessor of the internet, to allow project teams working with incompatible computer systems at different locations to communicate with packets of data that contained both messages and routing information. The first version (in the sketch at right) had four nodes, at UCLA, UC-Santa Barbara, the Stanford Research Institute, and the University of Utah.



PHOTOGRAPHY: COURTESY OF DARPA

Idea in Brief

**THE PROBLEM**

Traditional approaches to corporate research and development have difficulty consistently delivering breakthrough innovations.

**WHY THIS HAPPENS**

Compromises made to reduce risk or to avoid disrupting existing businesses result in evolutionary—but rarely breakthrough—innovations.

**THE SOLUTION**

A “special forces” style research group modeled after the Defense Advanced Research Projects Agency. Its key elements include tackling projects that advance science *and* solve significant problems; assembling the best minds from industry and academia for limited time periods to create diverse, agile, and scalable teams; and allowing independence from the mainstream organization in project selection and execution.

The presence of an urgent need for an application creates focus and inspires greater genius.

**Temporary project teams.** DARPA brings together world-class experts from industry and academia to work on projects of relatively short duration. Team members are organized and led by fixed-term technical managers, who themselves are accomplished in their fields and possess exceptional leadership skills. These projects are *not* open-ended research programs. Their intensity, sharp focus, and finite time frame make them attractive to the highest-caliber talent, and the nature of the challenge inspires unusual levels of collaboration. In other words, the projects get great people to tackle great problems with other great people.

**Independence.** By charter, DARPA has autonomy in selecting and running projects. Such independence allows the organization to move fast and take bold risks and helps it persuade the best and brightest to join.

**Decoding DARPA**

DARPA was created in 1958, shortly after the Soviet Union launched *Sputnik*, the first man-made satellite to reach space, sparking a national crisis in the United States. Concern that the Russians had achieved technological superiority led to the formation of the agency. Its founding mission was simple: “to prevent and create strategic surprise.”

The day before we assumed the agency’s helm in July 2009, one of its former directors took us aside and said: “DARPA is one of the gems of the nation. Take good care of her.”

We had both held other jobs at DARPA. They included roles as program managers for micro-electro-mechanical systems and land-mine detection and running the electronics technology office. It felt entirely different, however, to be responsible for the organization itself.

Both of us had a visceral sense that we had been handed a high-performance engine. We were intent on running it at the maximum RPM but also wanted to preserve it for the long term. Truth was, no one seemed to fully understand how the engine worked.

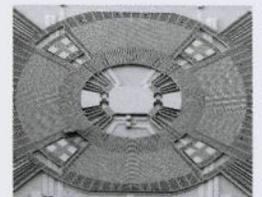
Adding to our concerns were disagreements in the DARPA community that had taken place over the preceding decade. Out of a desire to contribute more directly to current military conflicts, changes had been made in how funds were allocated to researchers and how decisions to continue or kill projects were reached.

With this weighing on us, we set out to discover what made DARPA so successful. We began by comparing notes on what each of us thought were its core attributes. Then we talked with dozens of colleagues—people who were working or had worked at the agency as program managers and office directors—as well as leaders from industry and universities.

This foundational work led us to understand the key elements of the agency’s success—the things we had to preserve and even strengthen. And although it’s too early to declare victory, we think the reason we have made such rapid progress at Motorola Mobility is that we grasped which battles we had to fight in order to translate the DARPA model into one that could work in industry.

**A Dedication to Pasteur’s Quadrant**

A central reason DARPA has been so successful over time is its unwavering commitment to work in what the late political scientist Donald E. Stokes, of Princeton, described as “Pasteur’s Quadrant.” (See the exhibit “Expanding Basic Science and Solving Society’s Problems.”) It entails pushing the frontiers of basic science to solve a well-defined, use-inspired need. Stokes named the quadrant for Louis Pasteur, one of the founders of microbiology. Throughout his



**MOTION-SENSING MICROMACHINES**

In the early 1990s, DARPA broke new ground in devices that combine sensors, actuators, and electronics on a chip. The U.S. military used them in systems that tracked soldiers inside buildings, armed torpedoes, detected biological weapons, and improved avionics. Today they provide critical functions in air bags, virtual-reality animation, gaming systems like the Wii and Kinect, ink-jet printers, smartphones, tablets, and high-definition TVs.

## Expanding Basic Science and Solving Society's Problems

The late political scientist Donald E. Stokes distinguished four categories of research and highlighted how basic science can be employed to create practical applications.

**Bohr's Quadrant.** Work here is curiosity-driven basic research, which seeks foundational knowledge without consideration of practical use. This quadrant is named for the early-20th-century atomic physicist Niels Bohr.

**Edison's Quadrant.** This category is pure applied research, aimed at finding a solution to a practical problem, and has no interest in explaining or understanding the phenomena of a scientific field. It's named for the inventor Thomas Edison.

**Pasteur's Quadrant.** Research here expands basic scientific knowledge in order to meet pressing societal needs. This quadrant is named for Louis Pasteur, a founder of the field of microbiology, who invented ways to prevent disease and food spoilage.

Stokes didn't bother to give the fourth quadrant a label; both the science and the use here tend to be uninteresting.

		PRACTICAL USE?	
		NO	YES
QUEST FOR FUNDAMENTAL UNDERSTANDING?	YES	PURE BASIC RESEARCH BOHR	USE-INSPIRED BASIC RESEARCH PASTEUR
	NO	UNNAMED	PURE APPLIED RESEARCH EDISON

SOURCE DONALD E. STOKES, *PASTEUR'S QUADRANT: BASIC SCIENCE AND TECHNOLOGICAL INNOVATION*, BROOKINGS INSTITUTION PRESS, 1997

career, Pasteur conducted research that advanced basic science—the fundamental understanding of phenomena—as part of his quest to address pressing societal needs, such as fighting diseases like tuberculosis, anthrax, and rabies, and preventing food spoilage.

DARPA is not the only organization to live in Pasteur's Quadrant. There are several examples in the life sciences industry and at the intersection of art and engineering, for instance.

But in the private sector in general, examples of work in Pasteur's Quadrant are rare. Knowingly or unknowingly, many companies continue to subscribe to the linear model of technological innovation: Basic research is exploratory; applied research connects new discoveries to a practical end; and commercialization is focused on developing a product that incorporates the technology and manufacturing it at scale. Many business executives view basic research as high risk, to be undertaken with extreme caution because predicting what, if anything, the research will produce is hard, and calculating the value of any discoveries can be difficult.

Some of the research that companies conduct—usually a small portion of the total R&D budget—may be in basic science. But typically, the basic research is dubbed “blue sky,” “exploratory,” or “speculative,” and is divorced from concrete needs or problems. Often companies try to make this research more practical by mandating that the business units select and fund it. Not surprisingly, the probability that they will select projects that challenge or even threaten existing products and services is low. During the negotiations between the R&D department and the business units, compromises are made. The result is work that is the worst of both worlds. It lands in the lower-left quadrant of Stokes's matrix, where the science is not interesting and no one cares about the

goals being pursued. Talent exits, and projects fail more often, not less.

Most corporate research budgets are devoted to innovations critical to maintaining companies' competitiveness in their existing industries. The research agenda is dictated by technology road maps designed to ensure that R&D investments produce reliable outcomes.

A company can usually map user needs and how existing or emerging technologies should advance to meet them. The road map often looks three to five years out, maybe even a decade. Typically, everybody in the industry has developed a similar picture. For example, all semiconductor manufacturers have known how fast to expect circuits and devices to shrink and what manufacturing advances were needed to achieve increasingly smaller features. As a result, companies have used essentially the same manufacturing technologies and chosen to differentiate themselves by the types of circuits and products they created, not how they made them.

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. They should take to heart the lesson that the U.S. government learned from the launch of *Sputnik*: The best way to prevent surprise is to create it. And if you don't create the surprise, someone else will.

**Identifying projects.** There are two ways of identifying projects to pursue.

One is to recognize that a scientific field has emerged or reached an inflection point, and that it

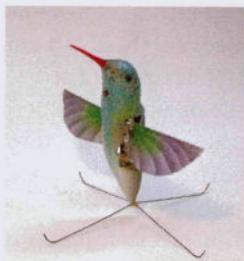
can solve, often in a new way, a practical problem of importance.

MEMS is an example. In the early 1990s, research on the use of sensors and actuators to create micro-electro-mechanical systems that could be built with standard semiconductor-fabrication methods was a promising, emerging field. The lion’s share of the research was being conducted at universities, largely with National Science Foundation funding; it was aimed at expanding basic scientific knowledge and going in a thousand directions.

The DARPA program focused MEMS research on delivering new capabilities in three applications of interest to the military: inertial navigation (for applications like tracking soldiers inside buildings and arming weapons), optical switches and displays (for avionics and ground-to-air communication systems), and lightweight laboratories on a chip that could quickly perform tasks in the field like detecting the presence of biological weapons and identifying remains. The project broke new ground in several areas of science, including plasma physics, fluid dynamics, and materials.

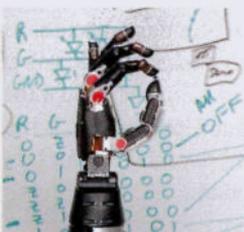
The second way to identify projects is to uncover an emerging user need that existing technologies cannot address. An example is DARPA’s ongoing hypersonic test-vehicle program to develop an unmanned glider that can fly at Mach 20 after being boosted to near space. The national security objective is to create the capability to reach any point on the globe in less than 60 minutes from the continental United States, with a vehicle whose course can be changed during flight and whose trajectory does not signal a ballistic missile launch. At this speed the surface of the airfoil is 3,500 degrees Fahrenheit, the temperature of a blast furnace, and is burning as it flies. Overcoming all the challenges involved requires advancing the science of materials performance and assembly, hypersonic aerodynamic control, boundary layer transitions at such speed, heat-transfer-modeling capabilities, and automatic systems for flight termination.

A project portfolio should include a healthy balance of both kinds of initiatives—projects that are focused on new possibilities created by scientific advances and projects that are focused on solving long-standing problems through new scientific development. Both can be identified through quantitative analyses. This instills discipline in project selection and execution. It is the project leader’s first task.



### UNMANNED AERIAL VEHICLES

DARPA has been doing research on remotely piloted aircraft, or drones, since the 1960s. One of its most recent inventions is the Nano Hummingbird vehicle, which weighs less than an AA battery and broadcasts video from a tiny onboard camera. It could allow soldiers to scout out threats unobtrusively and from a safe distance.



### REVOLUTIONARY PROSTHETICS

For service members who have lost limbs, DARPA is creating advanced prosthetics. It has already produced artificial arms that offer an increased range of motion and dexterity and currently is working to connect them directly to the brain and simulate a sense of touch. In initial tests, human subjects have been able to operate the limbs with their thoughts.

Take recent DARPA work in cybersecurity. An analysis showed that over the past 20 years, the average number of lines of code in a malware program had held nearly constant, at 125. But as malware proliferated, the number of lines of code needed to protect computers from it had skyrocketed, to more than 10 million. It was clear that current efforts were divergent with the threat. This prompted the agency to sponsor several initiatives, including one aimed at designing computers that thwart malware by constantly changing how they operate at the basic level where programs attack—but in a way that does not affect users’ interactions with the operating system or applications. And at ATAP, quantitative analysis showed that the percentage of all 3-D printing that was used to manufacture finished products, not just prototypes, had risen from 4% in 2003 to 25% in 2012, suggesting it may be possible to make individually customized electronic products en masse, creating the hardware equivalent of the software ecosystem.

**Defining a project.** Quantitative analysis should also be used in execution plans to help clarify the goals of the project and the technical challenges it must overcome. Both the capabilities and the technical solutions may need to be adjusted as the program proceeds. The original goal may be overtaken by the discovery of a different and better application. Some technical challenges may prove easier or more difficult than expected. As a result, entirely new problems may need to be solved. Since the project leader has the best technical insight and project awareness, he or she must be allowed to make choices about how to reallocate resources, assess progress, and revise goals along the way.

**Tracking progress.** Typical methods for planning and tracking product-development projects are not well suited to Pasteur’s Quadrant. In product development, experience with comparable projects is a good guide for estimating the time and resources required to hit milestones such as the completion of the design, the production of a successful prototype, and ramp-up to full-scale production. Projects in Pasteur’s Quadrant require different techniques. They involve fast iterations. Planning should be light and nimble. Progress can be assessed by tracking iterations to see if they are converging on goals, revealing dead ends, uncovering new applications, or identifying the need for unforeseen scientific advances.

Insisting that a team steadily hit milestones established in initial plans can cause it to adhere to a path

that—based on something the team has learned—no longer makes sense. Sometimes a setback or a failure is the most effective tool for discovery. If people working on a particular piece of a project experience a failure, it's often because something they encountered surprised them. That's to be expected in high-risk projects. When such events occur, the project leader has to let the team members press forward as long as they can see that the approach might

**Project leaders are focused on managing constant flux—building, replanning, changing tack, and moving talent in and out as needs shift.**

ultimately work within project constraints, even if they deviate from the original course.

That said, if it becomes clear that a given scientific approach won't work or requires multiple miracles in a row, that particular effort should be shut down and the resources shifted to other approaches. At DARPA or ATAP, performers who sign on to a project understand that their participation might end if the science doesn't work, the pace of progress is not commensurate with other efforts, and ideas for how to make it work cannot be found.

### **Time Limits and Temporary Teams**

One of the most effective ways to attract talented performers from a wide array of disciplines, organizations, and backgrounds—and to keep them intensely focused—is to set a finite term for a project and staff it with people working under contracts that last only as long as the jobs they perform contribute to the overall goal.

**Fixed durations and tenures.** Projects with set time frames (up to five years at DARPA and up to two at ATAP), leaders who leave when the projects end, and the scalability, diversity, and agility of contract performers have an edge over traditional captive

research organizations. All of those things make it possible to recruit high-caliber team members from a broader pool and get them on board faster. In addition, you can change the makeup of the group more quickly along the way as the team overcomes certain technical obstacles and others emerge.

In one project, ATAP was able to contract 40 of the world's best computer-vision experts from 30 entities (including universities, component suppliers, and systems integrators) in five countries and solve the most significant technical challenge in less than six months. We are convinced that we would not have been able to hire even a small fraction of them as permanent employees. Even if we could have, it would have taken more than a year to recruit them and get them working.

The DARPA model also allows a company to alter its portfolio of projects faster and at a much lower cost than a conventional internal research organization can. During our recent tenure at the agency, we were able to shift significant investments from programs in space and large air and ground systems to programs in cybersecurity, synthetic biology, and advanced manufacturing in less than a year.

Individual performers can be quickly reassigned to new work as well. If an organization involved in the project isn't getting results but its work is important to achieving program objectives, its efforts may be redirected and its contract renewed.

Another benefit of limited tenures is that—combined with a clearly articulated important need and a scientific challenge—they create a sense of urgency. This forces the team to act as a whole to benchmark progress and to continually challenge “how things have always been done.” The hypersonic test-vehicle project didn't set out to investigate the science of flying at Mach 20. It set out to demonstrate *within the five-year program* all the technologies necessary to launch a vehicle that could fly from point A to point B at Mach 20 and whose flight could be controlled. On the first flight, there was no aerodynamic control of the vehicle, but we collected nine minutes of Mach 17–20 data (more than had been collected in 30 years' worth of ground tests combined). Less than 18 months later, during the second flight, we achieved more than three minutes of fully aerodynamically controlled Mach 20 flight—a first.

As a practical matter, a high-risk effort by a diverse set of world-class experts can be sustained for only a limited period. One reason is the intensity. Another is that both the problems and the novelty of

the scientific advances needed to solve them are perishable. If the desired capability cannot be created within a fixed time frame, it is likely that someone else will create it or come up with another solution.

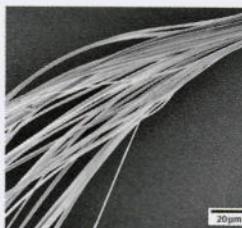
**Teams of contractors.** DARPA doesn't have any laboratories of its own. Its programs fund performers who work at their respective organizations and get together at least twice a year to review progress and objectives.

DARPA's MEMS program team, for instance, included experts in basic materials science, design and simulation tools, and semiconductor production. They were employed at the University of Michigan, Stanford, and other schools; large companies such as Honeywell, Alcatel, and Analog Devices; a number of smaller companies; and government labs such as Sandia and Brookhaven. And the hypersonic test-vehicle program has drawn world-class researchers in computational fluid dynamics, aerodynamic control, and materials science, and experts on manufacturing, rocket control, range safety, data collection, and telemetry.

On DARPA projects, people who normally wouldn't interact collaborate and inform one another. A scientist in a new field often has so many possibilities to pursue that it can be difficult to choose among them and concentrate. And often people in industry trying to create new applications encounter a need to make an advance, but some aspect of the science blocks them. When these diverse performers work together, the industry team members focusing on the applications may say to the scientists, "I don't have enough yield here," or "I'm not generating enough photons." And the scientists may say, "I can solve that problem for you" or "I cannot solve that problem, but I can solve this one. Does that help?" The same kind of interactions may happen across disciplinary lines. This dynamic produces a tremendously creative, fast, iterative cycle and generates breakthroughs in time frames that seem impossibly short.

**A special breed of project leader.** The project leader orchestrates the entire effort. He or she determines what pieces of work are needed to produce a specific result, conducts a proposal competition, and contracts organizations to do the work. (These organizations assemble whatever subcontractors they require.)

Project leaders who can successfully lead DARPA-like efforts possess the skills of the best CEOs of science- or engineering-based start-ups. Some



**CARBON COMPOSITES**

To meet the military's needs for lighter, more durable materials, DARPA created new forms of substances like carbon fibers (above), ceramics, and gallium arsenide. They are now used in a wide range of products—such as body armor, cell phones, and golf clubs.

project leaders may have held such positions. Others may come from academia, government labs, corporations, and nonprofits. They need to have deep technical or scientific knowledge, be natural risk takers, and be thought leaders who can create a vision that inspires an entire community.

Project leaders oversee the collection of performers, manage the technical details, and make all major decisions. They handle budgets, contracts, execution issues, speaking engagements, and customer relations. At DARPA that may entail explaining a project in three minutes to a four-star general who may or may not have a technical background, delivering a technical talk at a research conference, or working out intellectual property concerns with a university.

Many, but not all, project leaders have PhDs. Typically, they're in their thirties or early forties, five to 10 years past earning their last degree, and already have made important achievements (delivering a product to market, successfully leading a university research center, starting a company). Confidence is important. These midcareer leaders may recruit people who are older and more accomplished; they must be able to hold their own.

They rarely have MBAs. The skill set that you acquire in business school is often about defining the market opportunity, writing a plan, and then faithfully executing it. By contrast, DARPA and ATAP are more focused on managing constant flux—building, replanning, changing tack, and moving talent in and out as project needs shift.

How do you find such leaders? At DARPA we found them through our networks and those of the agency's current and former program managers, office directors, and performers. In the roughly three years we headed DARPA, about 75 of the 100 program managers changed as programs ended and others began. We did not have trouble finding exemplary people to fill those positions.

At Motorola Mobility, we are tapping our networks too, of course. We also have an expanded set of people within Motorola and Google who seek bold, fast, project-based work. Sometimes we find them; more often, they find us. Additionally, we use industry recruiters to identify talent.

DARPA has multiple attractions: Performing service to your country, the honor of being asked to work for an elite organization with a storied history, and the opportunity to pursue something amazing, often countercultural.



**STEALTH TECHNOLOGY**

To address the threat posed by the proliferation of advanced networked air defenses, DARPA dramatically reduced infrared, visual, acoustic, and radar signatures, which led to the design of new aircraft, such as the F-117 Nighthawk (above).

As an example, in 1992, university and industry efforts in MEMS were focusing on the wrong thing—the miniaturization of the devices. An unconventional minority opinion was that the opportunity lay in the integration of electrical, sensing, and actuation with signal processing and computing, and in building the devices with the same materials and processes used to manufacture semiconductors. That meant placing the highest priority on fabrication, system design, and design tools. DARPA not only permitted that view to emerge; it encouraged it. As a result, the agency was able to recruit talented leaders and performers.

The DARPA model gives exceptional leaders an environment where they can pursue what others may think is a crazy idea, challenge an entire industry, or catalyze the formation of one. Because they're not permanent employees, project leaders worry less about rocking the boat and jeopardizing their careers. They focus instead on changing the world. And many of them do.

At ATAP, we give project leaders the right to be impatient, as we did at DARPA. They refuse to wait for obstacles to be cleared. (One week of delay is 1% of an ATAP project leader's time at the organization.) Because our structure is flat, they elevate issues almost immediately to us. We clear them fast. This creates tremendous speed and momentum.

ATAP doesn't have all of DARPA's attractions. But it does share a vital one: the opportunity to make a difference and realize a bold vision. In addition, it gives people a chance to work for an elite commercially focused team in a fast-paced industry and at higher compensation than DARPA can offer. As ATAP proves in the coming years that it can innovate like DARPA, we expect the network of enthusiastic, qualified candidates to grow.

### Independence

An advanced technology and projects group must function in ways that differ from the normal company. Adjustments are needed in such areas as staffing, budgeting, and protecting intellectual property and proprietary information. In addition, the group's breakthrough innovations may lead to new businesses that require major departures for the company—or that threaten existing businesses. (If DARPA had needed the U.S. Air Force's authorization to develop stealth technology in the 1970s, the work may have never been done. The Air Force initially did not want the technology and repeatedly tried to



### GLOBAL POSITIONING SYSTEMS

DARPA's involvement in developing a precision navigation system that used satellites for the U.S. military dates back to 1959. Two decades later, the agency digitized GPS signals and created the technology for small GPS receivers.

stop the project. Only the intervention of the secretary of defense protected DARPA's efforts.) For these reasons, the team must operate with some independence from the rest of the company.

In a corporation, an advanced projects group should report to the chief executive officer or the operational executive ultimately responsible for nurturing and protecting innovations. This person should have control of significant resources and broad P&L responsibilities rather than a staff position. He or she should not be someone who might be motivated to use the resources to supplement product development dollars or to protect an existing business area.

Crucially, decisions about which projects to pursue must not be made by committee. Breakthrough innovations, by their very nature, do not lend themselves to consensus. Instead, the parent company should establish a multiyear budget with critical mass, ensure that the leaders of the advanced research group have visibility into—and the ability to influence—corporate objectives, and then give them the freedom to select projects. Within broad limits, they should also be able to reallocate and reprioritize spending within the group and among projects over time.

Given how new the DARPA model is to industry, it should come as no surprise that at ATAP it has been necessary to challenge existing assumptions about the way things operate. We've also had to push against the mainstream organization's tendency to enforce consistency and uniformity. Uniformity is not always desirable. When different outcomes are wanted, different approaches are necessary.

Hiring is a case in point. A high-tech company has a natural tendency to permanently hire as many world-class technical minds as possible. That is the right approach for much of the organization, where

**Decisions should not be made by committee. Breakthroughs do not lend themselves to consensus.**

## TELEPRESENCE SURGERY

With an eye toward bringing medical help to the wounded through robots on battlefields, DARPA has supported research on remote surgery. The resulting advances helped pave the way for robotic devices such as the Da Vinci Surgical System (right).



experience is essential. It allows the company to build a foundation of expertise, execute with greater probability of success along a road map, generate opportunities for innovation, motivate employees with career advancement, and develop future leaders.

This approach is precisely the wrong way to build a team of project leaders who will challenge the wisdom of experience and take on risky efforts. For the first six months after ATAP was launched, employment offers to project leaders often encountered the same questions: “Why are you hiring this person for only two years? Why not hire them as a permanent employee?” It took a while to get people in the mainstream organization to understand that hiring a technical rock star for his or her entire career might actually be counter to the mission of consistently generating breakthrough innovations over time.

The same kinds of concerns were raised about compensation packages. Project leaders recruited from outside the company take a greater risk because their jobs will last only two years. It’s fair and logical that their compensation reflect that extra risk and be greater than what they would earn as permanent employees but less than the payoff of a successful start-up.

Similarly, we had to develop a much shorter, simpler nondisclosure agreement for use with prospective external performers. We did this to address the specific needs of ATAP’s diverse community of performers, especially start-ups. In its first 14 months, ATAP reviewed 200 to 300 start-ups and contracted with over 100 of them. These young companies move fast and often do not have captive legal resources. Our modified NDA, developed with Motorola’s legal department, recognizes this fact. The result is that we routinely execute an NDA in less than a day rather than the typical weeks or months.

In the case of intellectual property, ATAP’s need for speed and flexibility clashes with conventional

approaches that focus on exclusive ownership of all IP from the beginning. Working with ATAP, Motorola’s legal team created development contracts that ensure access to intellectual property and allow for future negotiations about exclusivity. In so doing, we avoid protracted negotiations in the early stages of development. We gain speed and first-mover advantages.

As a result of adopting the DARPA model, ATAP has been able to launch eight projects, involving more than 120 companies and six universities and expertise from 11 countries, in 14 months. Three of those projects have produced multiple prototypes that demonstrated the viability of the envisioned product. Two of them advanced to low-volume production, were then further developed in collaboration with our colleagues in other parts of the company, and will soon ship in Motorola products. Some ATAP projects have made foundational advances in areas such as big-data analytics, the way graphics are rendered on mobile devices, and a faster, more secure way for users to sign on to their smartphones, tablets, or computers. By any measure, this is fast. It was accomplished with a staff of fewer than 40, including the project leaders and us.

**THERE IS A** detrimental divide between efforts to advance science and the development of new products and applications. The DARPA model is the only approach that has bridged that divide on a sustained basis. It has allowed the agency to recruit the best scientific and engineering minds, wherever they might reside, and to engage them to solve difficult problems.

The “special forces” model is a radical departure from the “spend a lot of money on research and hopefully something good will eventually come out of it” approach, which makes companies reluctant to undertake ambitious internal research. DARPA’s model offers an alternative, and its record of success proves that breakthrough innovations can be produced consistently, in remarkably short time frames, with a small, flexible, and agile organization.

Our current efforts suggest that organizations in the public and private sectors can dramatically increase their production of breakthroughs by adopting this model. The products and services created by these breakthroughs will improve the competitiveness of companies and countries. They also may restore a belief, that we can, indeed, shape the future. ♥

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# Executive Summaries October 2013

SPOTLIGHT ON BREAKTHROUGH INNOVATION



Traditional corporate research groups have tremendous difficulty achieving true breakthroughs. This package lays out the extraordinarily successful approach that DARPA introduced and Google now uses. It also explores current best practice in corporate venturing.

## INNOVATION & CREATIVITY

### “Special Forces” Innovation: How DARPA Attacks Problems

Regina E. Dugan and Kaigham J. Gabriel  
page 74

The Pentagon’s Defense Advanced Research Projects Agency arguably has the longest-standing track record of radical invention in history. Over the past 50-plus years, DARPA has produced an unparalleled number of breakthroughs, including the internet, global positioning satellites, stealth technology, MEMS, and carbon composites. The agency’s advances are now used in everything from smartphones to sporting equipment to artificial limbs, and it has played a central role in creating a host of multibillion-dollar industries.

What makes these accomplishments even more impressive is DARPA’s swiftness (its programs last only three to five years) and relatively tiny organization (about 120 permanent staffers) and modest budget (about \$3 billion annually for roughly 200 programs). With its unconventional speed, efficiency, and effectiveness, DARPA has created a “special forces” model of innovation.

In this article, two former DARPA leaders who now head an advanced research group at Google decipher the DARPA model and explain how it can be adapted for the private sector. The model has three critical, mutually reinforcing elements:

**Ambitious goals.** Projects are designed to push the boundaries of science to solve urgent real-world problems or create new opportunities.

**Temporary project teams.** DARPA contracts world-class experts from industry and academia to work on projects with relatively short durations. They are led by about 100 technical managers who have been brought in for fixed terms.

**Independence.** By charter, DARPA has autonomy in selecting and executing projects. Its model also challenges assumptions about how personnel are hired, and how contracting and intellectual property are managed. Such independence allows the organization to move fast and take bold risks, and helps it persuade the best and the brightest to join it.

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## ENTREPRENEURSHIP

### Corporate Venturing

Josh Lerner | page 86

For decades, large companies have been wary of corporate venturing. But as R&D organizations face pressure to rein in costs and produce results, companies are investing in promising start-ups to gain knowledge and agility.

The logic of corporate venturing is compelling: A well-run fund can help a firm respond quickly to changes in markets and gain a better view of threats. In some cases, it can stimulate demand for a company’s own products. And its investments may earn attractive returns. During their first three years as public companies, firms backed by corporate venture funds show better stock price performance, on average, than companies backed by traditional VCs.

Managing corporate venture funds is not easy. Some companies have seen their venture initiatives fail, and even firms with successful funds have struggled to make use of the knowledge gained from start-up investments. Six steps can help companies avoid the pitfalls.

**Align goals.** Corporate venture funds are more successful if the business of the corporate parent and of the portfolio firm overlap.

**Streamline approvals.** A complicated decision process can burden the fund with too many goals and lead to ineffective investing patterns.

**Provide powerful incentives.** Companies that don’t offer adequate compensation to their venture capitalists will face a steady stream of defections.

**Tolerate failure.** A zero failure rate may indicate that the fund is playing it too safe.

**Stick to your commitments.** If a company is seen as a fickle investor, professionals will be wary of joining its venture unit, entrepreneurs will be reluctant to accept its funds, and independent VCs will be hesitant to join in.

**Harvest valuable information.** Companies need to invest as much in learning from their start-ups as they do in making and overseeing deals.

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