Emergence of a new research and development paradigm: The structured contest

DWAIN K. BUTLER, Alion Science and Technology, Vicksburg, Mississippi, USA

A recent article in *Popular Science* magazine really resonated with me, in light of other recent experiences with "research and development programs." "DARPA's Debacle in the Desert" (Hooper, 2004) concerned the much-hyped 2004 DARPA Grand Challenge, which offered a US$1 million prize to the individual or team that developed an unmanned, completely autonomous vehicle that could navigate itself over a 150-mile course in the shortest time (under 10 hours). DARPA, the U.S. Department of Defense's premier basic research and out-of-the-box funding agency, gambled on technology development/advancement via a structured contest. The price tag to plan, advertise, provide support and logistics, monitoring and scoring, etc., was likely several times the potential prize. The best performer in the contest dropped out approximately 7.5 miles from the starting line. The contest certainly generated excitement, and it pulled together some interesting and talented teams. Did the contest spur technological advancement? Likely, although much will be quickly lost or diluted as most of the transient teams that came together dissolve. But the whole affair was so much fun that DARPA conducted DARPA Grand Challenge 2005 with a prize of $2 million! Following historical precedent for such contests, the criteria for "winning" would predictably be simplified. The DARPA contests are actually examples of a much broader trend in research and development that may be the embodiment of a new paradigm.

Regardless of our fondness for the "old ways of doing things," change will and is happening; it's one of the certainties of life, and the hallowed halls of R&D are no exception.

Figure 1. *Civil engineering students at Texas Tech University design and construct a concrete canoe and participate in the American Society of Civil Engineers' Concrete Canoe Competition.*

Scientists and engineers are familiar with "contests" associated with research and development. Historically and traditionally, however, the contests involved (1) formal competition through the proposal submission, review, and selection process; (2) informal competition between research groups to be first to achieve an objective and publish the findings in a peer-reviewed journal; and (3) competition in a corporate research setting to gain a competitive advantage through process or product. Scientists and engineers are familiar with other types of contests also, such as golf tournaments associated with professional society meetings, science fairs, and student concrete canoe competitions (Figure 1). Is anything learned from these other types of contests? Certainly! Sometimes our golf games improve, students are "turned on" to science and engineering, and students learn the value of working as part of a team.

Arguably, major science and engineering discoveries, breakthroughs, or advances can be loosely grouped into three major categories: accidents, flashes of insight, and systematic development. Some discoveries commonly attributed to accident, such as the discovery of radioactivity, often come as the result of systematic study of other phenomena or systems. Flashes of insight have occurred throughout history, and commonly are an individual researcher phenomenon. The new and pervasive group or team mentality and approach to R&D must proactively encourage and cultivate individual initiative and insight; flashes of insight are something that society can't afford to lose. For example, as my daughter went through an undergraduate engineering curriculum, there was so much emphasis on team and group work that I feared she would not develop the ability to think and work through challenges on her own; fortunately my fears were not realized.

By far, most major discoveries and advances come as the result of systematic development over time. Major advances of the 20th century, e.g., the transistor, the laser, the structure of DNA, and the computer, all resulted from systematic and persistent efforts. Even though the advances may be attributed to a select few researchers, many other scientists and engineers are generally involved in assisting, conducting complementary research, and contributing ideas. Invention of the laser is dated to 1958, with a paper by Arthur L. Schawlow and Charles H. Townes that was published in *Physical Review*. The invention, however, resulted from systematic development work in the 1940s and early 1950s by Schawlow, Townes, and others.

"Big science" (Goldberg, 1995) is a phenomenon of the 20th century, specifically during and after World War II, e.g., the Manhattan Project, the Apollo Program, high-energy physics, the Human Genome Project. Building on existing knowledge and technology, major and fundamental breakthroughs were achieved (or not achieved, as in the case of the SSC) by multiple collaborative teams and individual researchers under strong program management. Looking back on Manhattan and Apollo, we marvel at their achievements. We wonder, given the current research climate, if we could do it again. Perhaps more important than the achievements was the new paradigm that placed the government prominently central to funding and planning research.

The new paradigm. Perhaps calling the recent proclivity of government to fund research through contests or competitions a paradigm change is stretching the definition. However, as one who began and completed most of my career during the age of the government-funded/directed paradigm, I see a profound change occurring that in fact began at least a decade ago. This change included or coincided with the assumption of power and leadership positions in government agencies, corporations, and university research centers of the ubiquitous third-generation managers (Waymouth, 2001). The third-generation managers, known for their lack of fundamental knowledge of the technology they manage, bring new terminology, perspectives, and approaches to R&D, such as metrics, products, business areas, business practice, research portfolio, return on investment, and product lines.

Researchers now find that they spend as much time documenting and justifying work as they actually spent executing the work, e.g., the quarterly or even monthly progress reports, meeting projected obligation and expenditure rates, and bi-annual or yearly in-progress reviews. Considerable emphasis is placed on short-duration projects and short-term milestones and products, with quantity of product(s) keyed to both funding level and project duration. Long-term, systematic research efforts are increasingly difficult to maintain in the atmosphere of societal, managerial, and legislative drive for instant "return on investment" gratification (similar to the corporate model that has evolved from long-term vision to the yearly, quarterly, and even daily bottom line) (Office of Science and Technology Policy, 1997). Even the individual investigator conducting basic or fundamental research faces the daunting challenge of identifying in advance the follow-on applications and products that can utilize or benefit from the results of the research. The new atmosphere in research laboratories contrasts markedly with the atmosphere that fostered major 20th-century inventions and fundamental developments, e.g., the laser, about which Schawlow remarked after receiving the 1981 Nobel Prize in Physics for laser spectroscopy. "We thought it might have some communications and scientific uses, but we had no application in mind. If we had, it might have hampered us and not worked out as well."

The paradigm shift to R&D contests is perhaps a natural outgrowth of the advent of the third-generation manager, the societal shift from long-term to short-term vision, the insatiable desire for instant gratification, and the growing entrepreneurial approach to all societal and business endeavors. The structured contest fosters an entrepreneurial approach to R&D and is an ideal vehicle for third-generation managers: they don't need to fundamentally understand the concepts or technology involved; they just need to (1) understand the politically and financially driven research environment, (2) understand sound management concepts, (3) understand contests (how to set them up and publicize them), and (4) utilize both positive and negative outcomes to shape future investments and product marketing.

Examples. The DARPA Grand Challenge is the prototypical example and embodiment of the new R&D paradigm. Autonomous navigation over real-world distances and terrain is a daunting technological challenge. DARPA seeks a "low-ball" achievement of a "big science" objective by the structured contest. The logic is that the prize carrot, the thrill of competition, and the promise of a competitive market advantage that could be achieved with a win will serve to drive technology development. Publicity and increasing the size of the prize carrot are seen as tools to achieve success in the next chapter of the Grand Challenge. The DARPA model uses a large carrot but otherwise relies totally on external infusion of technology and funds, i.e., the contestants design, fund, and build their entries. Like the student concrete canoe competition, a major positive factor in the DARPA competition is the building of teams. For example, the Team ENSCO for Grand Challenge 2004 entry was built by a teaming of industry and academia, including high school students (Figure 2). If nothing else was accomplished, the high school and college participants will be emboldened and empowered to undertake big challenges in the future.

Figure 2. DARPA Grand Challenge 2004. This was an entry for Team ENSCO—an all-volunteer group of ENSCO researchers and engineers, joined by several individuals from The George Mason University Geography Department, an individual from the Georgetown University Physics Laboratory, and interns from the Thomas Jefferson High School for Science and Technology (Virginia, USA). Sponsors National Instruments, MathWorks, OmniStar, and Whelen Engineering donated hardware and software technology components for the entry vehicle.

Grand Challenge 2005 was certainly much more successful and satisfying for DARPA, with five entries finishing the 132-mile course and with four entries under the 10-hour target time. Commendably, the criteria for success were only marginally decreased from 2004, i.e., the finishing time target was kept the same and the course length was only 12% shorter. In this case, DARPA can certainly view the approach embodied in the new R&D paradigm as a success.

Cleanup of unexploded ordnance (UXO) on formerly used defense sites and sites undergoing closure is the highest-priority environmental restoration requirement of the U.S. Department of Defense, with nominally 40 000 km^2 potentially contaminated and projected cleanup costs of $50 billion or more (Department of Defense, 2003). Detecting and accurately locating the buried UXO is a major technological challenge, although the yearly R&D investment is relatively modest compared to the potential liability. Magnetometry and electromagnetic induction (EMI) systems are the geophysical technologies most applicable to UXO detection surveys (Figure 3).

Unexploded ordnance (UXO) technology demonstrations were conducted at Jefferson Proving Ground (JPG), Indiana, in five phases from 1994 to 2001 at a cost greater than $30 million. The JPG UXO technology demonstrations were a congressionally mandated program that specified where the demonstrations were to be conducted, which agency was to manage the demonstration, and technical details constraining the demonstration. The program was to be structured as a contest, where the demonstrators were selected and funded based on proposals for a technology demonstration on either or both of 16- and 32-hectare test sites. The National Research Council reported in 2000 that

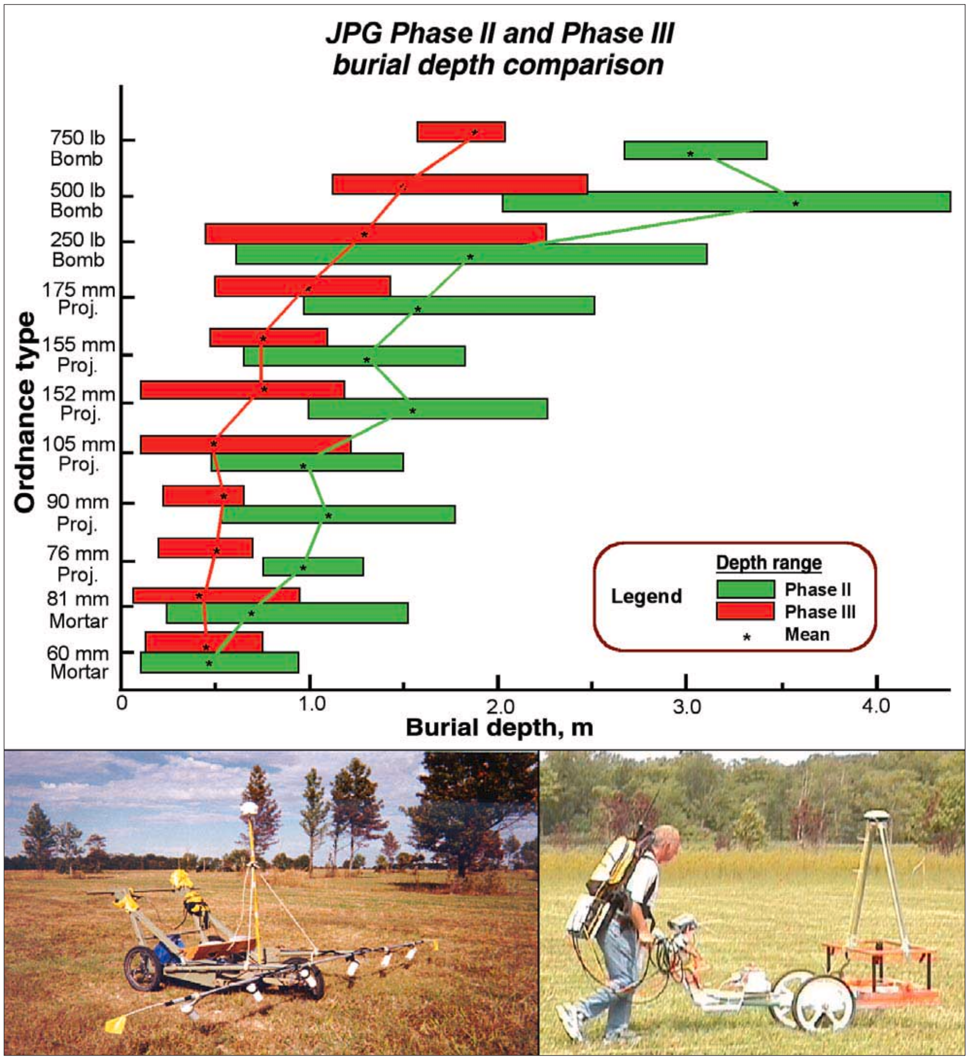

Figure 3. Comparison of JPG UXO technology demonstration phases II and III target burial depths, illustrating the progressive simplification of the detection challenge (top). Depth range bars and mean burial depths for phase I targets were not readily available, but definitely to the right of the phase II depths (in green). Examples of demonstration systems at phase IV (bottom left, a total field magnetometer array) and phase V (bottom right, an EMI system).

"Congress reacted to the complex technological requirements (for UXO cleanup) by attempting to specify the 'solution,' requiring off-the-shelf technology demonstrations in the form of a competition."

The test sites had a large number of inert ordnance items and scrap (false-alarm sources). Demonstrators received a score based on their performance, and they clearly recognized the process as a competition. Initially the emphasis was on detection but eventually shifted to discrimination (ordnance from scrap) and classification. Contrasted with the DARPA model, the JPG demonstrators (contestants) were paid for their efforts regardless of their performance, with the carrot for good performance (high score) being a competitive advantage in obtaining future UXO cleanup work.

The results of JPG UXO technology demonstration phase I, while not as spectacularly disappointing as the DARPA "debacle," were clearly unanticipated; only one ground-survey demonstrator succeeded in detecting more than 50% of the buried targets. None of the much-touted airborne system demonstrators exceeded 10% target detection. Either the problem is much more difficult than thought, the demonstrators were ill prepared and inexperienced, the test was too difficult, or a combination of the previous three factors. To ensure that follow-on demonstration phases were more positive and in a sincere effort to truly baseline capabilities, phases II and III were each successively easier than phase I (Figure 3). Although the detection challenges were significantly easier, it was still gratifying that performance in phases II and III were improved and the true baseline capabilities were better defined. Phases IV and V focused on the ability to discriminate targets as UXO or non-UXO and to classify as to type of UXO. Clearly, the carrot of competitive advantage motivated enhanced or improved demonstration performance during the successive phases, although the improvements were apparently transitory (competition-specific) and not general capability improvements. True advancements would come under other programs involving more traditional and systematic research, development, and demonstration approaches.

A third example also relates to the UXO detection problem. Three government agencies with UXO R&D programs sponsored a workshop on the use of EMI systems for UXO detection/location and discrimination (Butler, 2004). The three agencies are executing a systematic approach to technology advancement. The goals of the workshop were to achieve closer coordination and establish a roadmap for future investments in EMI R&D. A consensus conclusion was the desirability of developing a computational (numerical) simulator for EMI systems over a UXO target model embedded in a realistic earth model (Figure 4). While detailed models of the three components (EMI system model, UXO target model, earth model) exist, a rigorous, coupled model does not exist. Interestingly, the response to this perceived need was a recommendation for a computational "bake off" or contest, rather than a systematic R&D effort. That is, interested parties would all simulate a specified problem with their models/codes and compare their computational results to real-world measured results. The winner of the "bake off" could then claim to have the UXO computational simulator and have an inside track for future funding. The ultimate approach for achieving a UXO computational simulator, whether by systematic research or through a contest, will be an interesting test of paradigms.

The prior examples were all government-funded efforts (directly or indirectly); however, private sector interests are also evident in the promoting and funding of contests. A good example of private sector contests is the Ansari X Prize. The private X Prize Foundation was founded in 1996 and offered a prize of US$10 million to the first team that (1) privately finances, builds, and launches a spaceship capable of carrying three passengers to an altitude of 100 km; (2) returns safely to earth, and (3) can repeat the launch with the same ship within two weeks. Clearly, the technological challenge is daunting; but the foundation seeks to jump-start the commercialization of space travel, including the space tourism industry, by enticing entrepreneurs and rocket experts into a competition. More than 26 teams from seven countries registered to compete for the prize, which was awarded on 4 October 2004. The successful accomplishment of the X Prize challenge by Burt Rutan and Mojave Aerospace Ventures was an event with great public and media interest. The major societal goals of the foundation include creation of a new generation of heroes; inspiring, educating, and challenging students, explorers, and the public; and focusing investment capital on a major new business frontier.

The future. Assuming the new paradigm, what does the future hold for the R&D enterprise? Will the pace of discovery and technological innovation of the 20th century continue? Is the marketplace truly the best driver of R&D? Can the R&D enterprise thrive under third-generation managers? While the answers to these and other questions remain for the future, tentative answers can be proposed for debate. The R&D enterprise will find a way to thrive under whatever paradigm emerges. The pace of discovery of the 20th century will continue and likely accelerate. The marketplace is clearly a driver for R&D, but it is not always the best. The R&D enterprise will thrive under or in spite of the third-generation managers. As new and unforeseen technologies and innovations arise, some scientists and engineers will rise through the corporate ranks to become new first-generation managers. New first-generation managers, with fundamental knowledge of the corporate technology base as well as 21st-century management skills, can breathe new life and excitement into the R&D enterprise regardless of the dominant paradigm.

Regardless of any perceived fondness for the "old ways" of funding and managing the R&D enterprise, change will and is happening; it's one of the certainties of life, and the hallowed halls of R&D are no exception. Evidence of the new paradigm are everywhere, as noted in the examples and in the appearance of papers with titles like "Optimal design of research contests," (Che and Gale, 2002) funded by NSF, and "Do we need new competition policy in the 'New Economy'?" (Stenborg, 2002). Many positive aspects of the new R&D paradigm have been mentioned and others will be obvious to the reader. Many negative aspects of the new paradigm are stated directly or at least suggested in this paper. Individual researchers and research teams can learn to function under the new paradigm and many will thrive. Market forces can be powerful, and fundamental ideas that can reasonably assure technological innovation will be assured of funding and a greater likelihood of marketplace success than under the government-funded/managed paradigm. However, continuing development of fundamental ideas and concepts without any obvious or current marketability will require a true entrepreneurial spirit.

Astute individual researchers and R&D teams will learn to succeed in spite of the third-generation managers. Succinctly, researchers will learn what it takes to keep the new breed of managers happy; and when kept happy, the new managers will strive to keep funding streams flowing. In the process of learning to thrive in the third-generation R&D environment, researchers will learn to effectively promote their efforts and understand that technology transition and commercialization potential must be the final milestone.

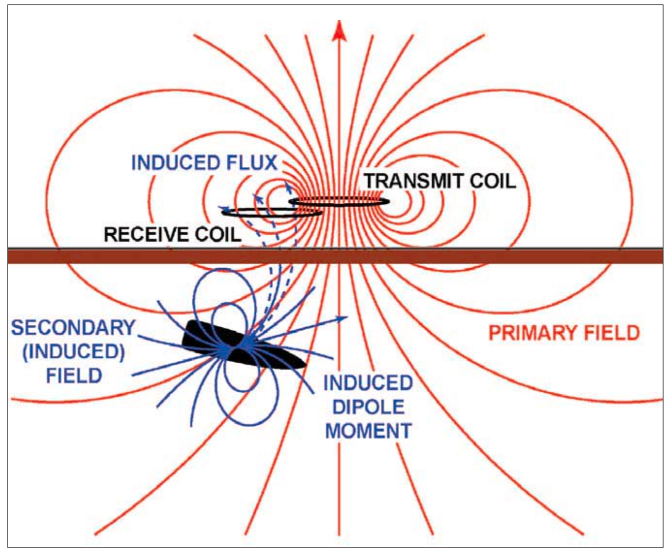

Figure 4. Concept of an EMI system and the induced field in a buried high-contrast target (large EM physical properties relative to surrounding geologic materials). EMI systems operate in time-domain or frequency-domain. For the time-domain systems, the induced transient response of the target is measured during the off-time of the transmitter.

The future under the new paradigm can actually mirror past successful ventures into efforts to achieve technological innovation by competitions by government and private sectors. For example, the British government in 1714 offered the "longitude prize" to the person or persons successful in developing the first accurate method for determination of a ship's longitude. Interestingly, the prize was structured to increase according to the accuracy of the method, increasing from £10 000 for an accuracy of one degree to £20 000 for 0.5° accuracy. It wasn't until 1761 that 0.5° accuracy was achieved, and the prize wasn't awarded in its entirety until 1773, after considerable government debate over the veracity of the method (Sobel, 1995).

A private-sector-promoted example is the Orteig prize ($25 000) awarded to Charles Lindbergh in 1927 for the first nonstop, trans-Atlantic flight. Lindbergh's small professional team succeeded where larger government and private efforts failed. Lindbergh's accomplishment was possible due to the persistence of the Wright brothers in an earlier informal, international contest to be the first to achieve manned powered flight. The Wright brothers were the prototypical, pure R&D practitioners; they refused to accept any outside funding until they had achieved and demonstrated their objective (Butler, 2002). While there was always the prospect of the commercial exploitation of powered flight and the Wright brothers fully recognized and ultimately seized the opportunity, the motivating force for the brothers was the sheer joy of discovery. The Wright brothers, as first-generation managers, succeeded in marketing their discovery in a spectacularly successful manner.

As indicated previously, advances in geophysical technology have been and are being made by structured contest or demonstration. The SEG/EAGE 3-D Modeling Project (now continuing its work as the SEG Advanced Modeling project [SEAM]), for example, involved a form of structured contest. The project solicited different finite difference modeling codes to be used in generating the 3D synthetic volumes. Several codes from different companies and R&D groups were considered. Among the selection criteria were ability to publish the code and make it available to everyone interested, accuracy of the results, and computation time. No rigorous competition or contest was carried out, but more or less by default a code was declared the "winner" from the results submitted, and the project proceeded (Aminzadeh, 2005). Motivation generated by the prospect of competitive advantage drives equipment development, process advances (data processing and interpretation), and research. Marketplace forces have always been strong drivers for geophysical R&D in the oil industry.

Another driver for a structured geophysical demonstration process or competition is the need to "weed out" charlatans or pseudoscience technology claims. While a structured demonstration has been used occasionally in the past (Tuley et al., 2001), a formal process of "double blind" demonstration to verify, refute, or debunk questionable geophysical technology claims is needed in both government and private sectors. The U.S. government in general and the Department of Defense in particular is plagued continually by bogus geophysical technology proposals to officials and managers, who have no capability to assess the merits of the technology presented. Recent increases in such bogus concepts and proposals are prompted by the very real needs posed by intrusion tunnels (both domestic and foreign) and underground facilities, land mines, improvised explosive devices (IEDs), and UXO cleanup. The greatest challenge to developing a formal and reliable geophysical demonstration process is a mechanism for triggering the process.

There will undoubtedly be resistance and a "competition" between the old and new paradigms of R&D funding and execution. This battle between old and new R&D paradigms will occur simultaneously with a struggle over a balance between "small" and "big" science program funding (AGU, 2004). Scientists and their professional societies must stay involved in the process of shaping the R&D paradigm and many will and should make the laudable "personal sacrifice" of entering the political arena to directly influence the process.

Suggested reading. "DARPA's debacle in the desert" by J. Hooper (*Popular Science*, 2004). "Big science: Atomic bomb research and the beginnings of high-energy physics" by S. Goldberg (*Topical Essays for Teachers*, History of Science Society, 1995). "A dim view of management" by J. F. Waymouth (*C2C: Connecting Classroom and Community*, 2001). "Report to the President on federal energy research and development for the challenges of the twenty-first century" (Office of Science and Technology Policy, 1997). "Report of the Defense Science Board Task Force on Unexploded Ordnance" (Department of Defense, 2003). "Seeing into the Earth" by National Research Council (National Academy of Sciences, 2000). "Report on a workshop on electromagnetic induction methods for UXO detection and discrimination" by D. K. Butler (*TLE*, 2004). "Optimal design of research contests" by Y-K. Che and I. Gale (*American Economic Review*, 2002). "Do we need new competition policy in the 'new economy'?" by M. Stenborg (*The Finnish Economy and Society*, 2002). *Longitude: The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time* by D. Sobel (Walker Publishing, 1995). "Flights of fantasy: A triumph of ingenuity, persistence, and the scientific method" by D. K. Butler (*TLE*, 2002). "AGU position statement on the need for balance between small and large science" by AGU (*EOS*, 2004). Fred Aminzadeh (personal communication, 2005). "Evaluation of EarthRadar UXO testing at Fort A.P. Hill," by Tuley et al. (Institute for Defense Analyses, Report D-2625, 2001). TLE

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Corresponding author: Dwain.K.Butler@erdc.usace.army.mil