

Secretary of Energy Advisory Board

Report of the Task Force on Technology Development for Environmental Management

December 2014



U.S. Department of Energy

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Preface

One of the major missions of the Department of Energy (“DOE”) is the cleanup of the legacy of nuclear weapons production and Government-sponsored nuclear research. Beginning with the Manhattan Project and extending over the course of the Cold War, DOE operated a massive industrial complex to produce and test nuclear weapons and to pursue nuclear technology more generally. When the Cold War ended, most of this complex was shut down and DOE began the costly long-term effort to clean up the materials, waste, and environmental contamination resulting from the operations. The scope of the cleanup work is staggering. Over \$150B has been spent to date in pursuit of this mission, with the result that cleanup of 91 of the 107 major sites has been completed.¹ But many of these sites were small and slightly contaminated; the cleanup of 16 difficult and high-risk sites remains ahead. The remaining work represents some of the most complex and technically challenging cleanup efforts anywhere in the world.

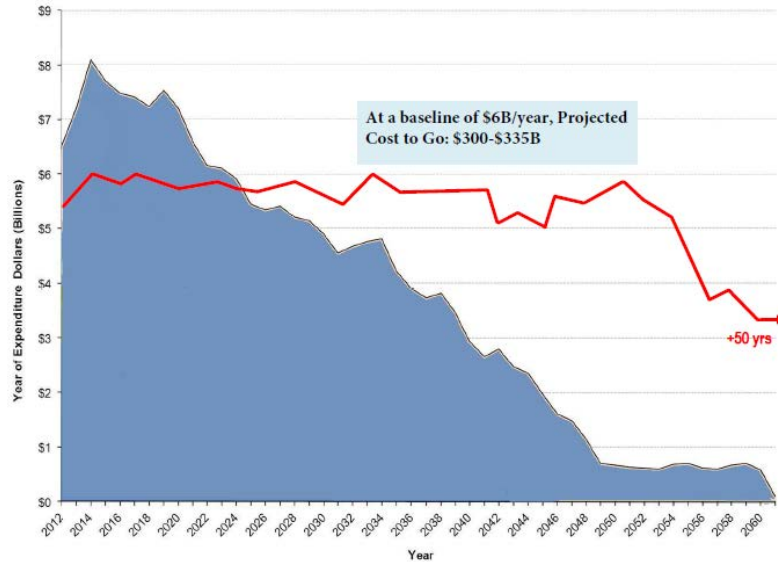
DOE estimates that the cost to complete the work could range from \$187-223B, assuming expenditures consistent with a “compliance budget” over the next ten years or so.² As shown by the figure, if funding at the full level were available, the cleanup is projected to be completed around 2060. But the “compliance budget” is well above the \$5-6B/year allocated to Environmental Management (“EM”) in recent years and, in the current budget circumstances, it is unlikely that increased funding will be available. If EM expenditures are limited to \$5-6B/year, there will likely be non-compliance with some consent orders and agreements governing cleanup, a delay in completion of cleanup, and a significant increase in the lifecycle cost. The total cost increases because as the cleanup schedule is extended, maintaining the sites and the facilities used for cleanup (including the steadily increasing maintenance cost of aging infrastructure) consume an increasing fraction of the available funds, leaving less for the conduct of the cleanup work. With a baseline budget of \$6B/year, the incremental cost for completion of the work might require cumulative expenditures in future years on the order of \$300-335B and that the cleanup would extend for many decades after 2060. Indeed, the technical issues

¹ Presentation to the Task Force by the DOE Office of Environmental Management (July 15, 2014).

² *Id.*

associated with the successful completion of several large projects suggest that these baseline costs may be underestimates.

- **The cumulative future cost with unconstrained annual budget allocations is estimated at approximately \$200B.**



Source: DOE Laboratory Directors’ presentation to SEAB (Mar.27, 2014).

Finding ways to reduce the aggregate cost, to do the job more effectively and safely, and to speed up the work will clearly serve the American public. Technology offers that opportunity. Moreover, new technology is necessary because there are significant challenges associated with the cleanup work ahead. In fact, without the development of new technology, it is not clear that the cleanup can be completed satisfactorily or at any reasonable cost.

It is in this context that the Secretary charged the Secretary of Energy Advisory Board (“SEAB”) to provide advice as to how the Department could more effectively assure the development of technology for the cleanup effort. SEAB formed a Task Force to examine the issue and this document constitutes the Task Force’s report to SEAB on its work. The charge from the Secretary is set out in Appendix 1, and the membership of the Task Force is identified in Appendix 2.

Executive Summary

The current EM budget for technology development is only \$13 million – about 0.2% of the EM budget. But the successful completion of the cleanup of the sites likely will require significant new technology. Indeed, advances in science and technology can provide the means for completing the EM mission more swiftly, more inexpensively, more safely, and more effectively. The current investment in science and technology is far too low.

We recommend that DOE increase its investments in science and technology for the EM cleanup program to about 3% of the annual EM budget. We further recommend that these investments be focused on three strategic elements:

- *An incremental technology development program* focused on improving the efficiency and effectiveness of existing cleanup processes. This effort should grow from the existing \$13M/year investment to \$30-50M/year and should remain a separately identified part of the EM budget.
- *A high-impact technology development program* that pursues technologies that are outside the day-to-day program, that target big challenges, and that hold the promise of breakthrough improvements. It is our judgment that the program should have ultimate funding of \$75-100M/year if the initial results are promising. This program should commence with a workshop to identify specific challenges to be targeted.
- *A fundamental research program* focused on developing new knowledge and capabilities that bear on the EM challenges. This program should be tailored to the EM mission, should have a budget of approximately \$25M/year, and should be managed by the Office of Science in close coordination with EM. This program should commence with a workshop involving all potential participants to develop a strategic research plan to inform requests for proposals.

All three of these programs should:

- Utilize rigorous peer review;
- Encourage broad participation by universities, national laboratories, and industry; and

- Include periodic assessments of program effectiveness to guide adjustments.

We further recommend that DOE create an *EM university program* to engage faculty, postdocs, and graduate students in the pursuit of the EM mission in order to provide a pipeline of new ideas, to access advances in engineering and science, and to provide a cadre of educated personnel for participation in the EM program in the decades ahead. This program should be modeled on the Nuclear Energy University Program and should be funded at a level of \$10M/year.

Finally, we recommend that DOE engage all stakeholders – program offices, contractors, universities, national laboratories, regulators, concerned citizens, and political figures -- in the consideration of new technologies in order to build a foundation for the acceptance of new approaches. There needs to be a common appreciation that new ways of doing business are necessary to catalyze success.

The various recommendations, if pursued at the levels we recommend as eventual targets, require budgetary allocations as follows:

Incremental Technology Development	\$30-50M/year
High Impact Technology Development	\$75-100M/year
Fundamental Research Program	\$25M/year
EM University Program	\$10M/year

The total cost of the recommendations is thus in the range of \$140-185M/year. We readily admit that the estimate of the appropriate funding is a judgment by the Task Force (several of whom have extensive experience with the cleanup program) and that monitoring and adjustment of the investment over time is appropriate. We also recognize the difficulty of an investment at this scale in the near term. But, these funds constitute only about 3% of the EM budget and this investment need not arise from new budget outlays, but should come in large part from the existing EM budget. We believe that the savings that are likely to arise from an investment along the lines we propose will amply repay the costs over time. Moreover, the usage of incentivized contracts -- that is, sharing the savings arising from new technology with the contractors -- may facilitate the application of new technology in ways that reduce the budget impact.

Although technology should be an essential element of a revitalized EM program, we believe that a comprehensive reexamination of the EM program is necessary. There are fundamental conflicts among the deadlines in compliance agreements, the regulatory requirements, the availability of technology that can enable the successful completion of the work in a timely fashion, and current and future budget realities. There need to be adjustments in the program that allow science and technology to provide a higher probability of long-term success, as well as the possibility of earlier completion of the cleanups, significant cost savings, and risk reduction. In the long term, the public will be served by a fundamental realignment of the cleanup program to one that is based on the achievement of a balance of cost and risk and that is built on a foundation of scientific and technical advance.

Technology Development for Environmental Management

In section 1, we discuss the need to pursue science and technology in a variety of different ways in order to facilitate the cleanup effort. In section 2, we discuss the special need to engage universities. Then, in section 3, we discuss funding. Section 4 describes some management issues that are critical for success.

1. Science and Technology for Cleanup

The EM budget over the years has included varying levels of funding for technology development (“TD”). In the early years through about FY2000, the annual TD funding was over 4% of the EM budget and exceeded 6% in the period from FY90 to FY96. At the time, EM did not have a good understanding of the cleanup challenges that it faced, with the result that its R&D expenditures were not well focused and, as a general matter, were not seen as fully effective. The EM Science Program is viewed as having been successful in attacking the “R” part of the R&D effort, but it was handed off to the Office of Science (“SC”) in the early 2000s and over time it became less and less directed at meeting EM’s needs. After FY2003, the funding for TD plummeted in large part because of an assumption that the cleanup could be accomplished quickly and that new technology was not needed. Then, in the years after FY06, the funding for TD has typically been less than 0.5% of the EM budget. The level of funding in recent years reflects the low priority given to the development of technology in a period of constrained budgets and the need to direct funds to comply with cleanup deadlines required by consent decrees and agreements. In fact, in FY14 the TD budget is only \$13 million – only about 0.2% of the EM budget.³

The TD funds do not represent the entirety of the efforts to advance and apply technology to the cleanup effort. The contractors who perform the cleanup and the field offices have pursued technology development using program funds to deal with specific challenges that must be overcome to complete their work. Technology inevitably marches forward over time on many fronts and the cleanup methods reflect that reality. The extent of the investment by DOE

³ A fuller history of TD funding is described in Appendix 3.

and the contractors using program funds for technology development is uncertain. But the focus of the programmatic efforts to apply new technology is on near-term (less than 3-5 years), readily solvable, and local (site-specific) challenges, not on long-term, difficult, or cross-cutting challenges that extend across the entire complex. It must be recognized that DOE has responsibility for the full mission life-cycle (50+ years), whereas the contractors typically have a short-term responsibility. Thus, the technology developed and applied in the course of operations does not represent a strategic approach to the deployment of technology across the full scope and life cycle of the EM program. Investments are not being made in science and technology that could have high impact but that require sustained research and development.

The determination of the appropriate allocation for advancing technology must, of course, be governed by the nature of the challenges, the opportunities that technology development provides to meet those challenges, and, ultimately, by an assessment of the return on investment. Because the challenges that confront EM are profound and difficult and the anticipated time frame for the completion of cleanup is long, we are confident that there are significant opportunities to exploit technology to speed cleanup, to increase its effectiveness, to reduce worker risk, and/or to reduce cost. (In many cases, we expect that these benefits may occur together.) We conclude that the support for the development of technology for the cleanup effort has fallen far too low. We note that a committee of the National Academies reached a similar conclusion after an extensive study of the opportunities to apply technology in a 2009 report: *“Observation 1: the complexity and enormity of EM’s cleanup task require the results from a significant, ongoing R&D program so that EM can complete its cleanup mission safely, cost-effectively, and expeditiously.”*⁴

The program should include three different components: (1) incremental technology development; (2) high-impact technology development; and (3) advancement of fundamental research related to the cleanup challenge. In order for these efforts to yield their full benefits, there are also a number of other matters deserving attention that are discussed in Section 4.

⁴ National Research Council, *Advice on the Department of Energy’s Cleanup Technology Roadmap: Gaps and Bridges*, 5 (2009) (italics in original).

a. Incremental Technology Development

EM should seek to pursue incremental changes in existing processes in order to improve their effectiveness, to speed up the cleanup, and to reduce cost. Industrial experience shows that the cumulative effect of incremental changes can be very large. In fact, the continuous improvement at Rocky Flats of decontamination chemicals and techniques, of detection, assay and certification instrumentation, and of packaging materials, methods and shipping systems, reduced cost significantly and accelerated the cleanup. The instrumentation/sensors and decontamination (chemical and mechanical) technologies used in the cleanup of Rocky Flats were essentially all unknown at DOE sites when the cleanup work began in earnest seven years earlier. Most of these technology improvements were co-funded by DOE and the contractor by way of a partnership in which the contractor was allowed to retain a share of the cost savings.⁵

As noted above, some incremental development of technology is pursued today within the site-specific programs. This should continue. Moreover, because some of the cleanup challenges confronting DOE are not unique, EM should seek to engage with other agencies, such as DOD and NASA, in the pursuit of technology. There are also opportunities to harvest advances made elsewhere in DOE that would facilitate the EM mission. For example, work at the National Energy Technology Laboratory on corrosion may bear directly on EM's work. We understand that EM is now pursuing these opportunities.

There also should be a focused investment in incremental technology development as an identified and separate part of the overall EM program. The time frame and scope of the current efforts may not fully reflect consideration of opportunities that can arise across the complex or the need for a long-term perspective. Given the magnitude of the cleanup task, we judge that the target for EM investment in incremental technology development should grow from the current \$13M/year to funding on the order of \$30-50M/year.

⁵ The Government Accountability Office has pointed to the development of new technology and the incentives for the contractor as important factors for the rapid cleanup of Rocky Flats at lower cost than anticipated. GAO, *Nuclear Cleanup of Rocky Flats: DOE Can Use Lessons Learned to Improve Oversight of other Sites' Cleanup Activities*, 4-5 (2006) (hereinafter "GAO Report").

b. High-Impact Technology Development

Largely missing from the EM program is a vigorous effort to pursue and develop technologies that are outside the day-to-day program, that target big challenges, and that hold the promise of breakthrough performance. We believe that there are significant opportunities to develop game-changing technologies that could significantly speed up the cleanup, reduce cost, and/or improve the effectiveness of the effort. Given the expected duration of the work, there is ample time to develop and deploy such technologies or to adapt and improve technologies from other industries or applications. We thus conclude that there should be a substantial focused effort to pursue them.

Successful past examples of the sorts of technology development of the character that should be pursued include: the improvement of glass waste loading and the ability to accept a wider range of waste constituents, engineered groundwater remediation, and next generation solvents for cesium separation.⁶ A presentation to us by the National Laboratory Directors' Council (NLDC) shows that past advances in these areas have achieved a disproportionate return on investment.⁷ We agree with their assertion that significant gains can be achieved by a program that is focused on advancing novel ideas. Indeed, testimony to the Task Force indicates that EM believes it has a stockpile of potential game-changers that, with adequate resources, it would pursue.⁸ The NLDC presented various suggestions to us.⁹ And some thoughts by the Task Force are attached as Appendix 4.¹⁰

⁶ Presentation by Terry Michalske on behalf of National Laboratory Directors' Council to the Task Force (July 15, 2014).

⁷ *Id.* For example, funding of under \$60M to develop a “next generation solvent” for cesium extraction in salt waste processing at the Savannah River Site is claimed to have resulted in savings of over \$1B. Similarly, the expenditure of about \$1 million to develop a science-based approach for evaluating the appropriate remediation strategy for volatile contaminants in the vadose zone is estimated to have yielded potential savings of \$50M. *See also* GAO Report, *supra* note 5, discussing the cleanup of Rocky Flats.

⁸ Testimony by Mark Gilbertson, Deputy Assistant Secretary for Environmental Management, to the Task Force (October 9, 2014).

⁹ Presentation from Terry Michalske, *supra* note 6.

¹⁰ The National Academies have prepared a comprehensive analysis of the “gaps” in the cleanup program that require attention. National Research Council, *supra* note 4, Appendix C. That analysis reveals needs that should be addressed by the technology development efforts.

Given the magnitude of the cleanup task and the opportunity for cost reduction and for efficiency improvements, we believe it is appropriate to pursue a program to pursue high-impact technology with ultimate funding on the order of \$75-100M/year if the initial results are promising. We suggest that the effort start at a smaller level (say \$10-15M) to build the program and ideally to achieve some early success. For technologies showing early promise and no indicated show-stoppers, the more expensive pilot tests on actual wastes would follow.

Participation in the program should be available to all who can contribute. It should include the DOE national laboratories (who have been significant advocates of this approach), other federal laboratories (e.g., NRL, NIST), the universities, and contractors. In fact, the efforts might appropriately involve partnerships among these groups.

There should be a rigorous process to select the projects to pursue, including a careful needs definition, open competition for proposals, and independent peer review. The following characteristics are illustrative of what is expected from such technologies: the possibility of significant improvement in cost or efficiency; flexibility to adapt to circumstances, such as a range of different feeds or variability within a feed; simplicity; versatility; modularity; the promise of less operator handling; the elimination of secondary wastes; more efficient monitoring and control of processes; and fewer processing steps. Of course, not all the characteristics will be found in any given approach. Moreover, it is important to pursue technologies that do not threaten to create risk for the baseline technologies that are applied at the sites, but rather that promise less risk in their application. Indeed, the scope of the program should include consideration of cleanup challenges that will be presented in the future as operational sites are transferred to EM for decommissioning and final cleanup. We suggest that the effort commence by way of a workshop involving the prospective participants to identify some specific challenges to be targeted.

The process should also incorporate rigorous periodic reviews so that continued investment in approaches that do not hold promise is avoided. It should be anticipated that some

initiatives will fail: that is the nature of investment in breakthrough approaches.¹¹ The expectation is that an aggressive effort will result in enough “winners” to more than compensate for the ideas that do not fulfill their initial promise. That is, we advocate a strategy that has long been the *modus operandi* for the venture capital community.

The design of the program should recognize and build-in the reality that the pursuit of concepts developed through the program will have to be passed to the program offices for pilot efforts as well as full-scale demonstration. The application of new approaches will require the acceptance by the full range of stakeholders, perhaps coupled with contractual or regulatory change. This aspect of the challenge is discussed in section 4.

The possible homes for the program include EM, ARPA-E, a free-standing entity, or the Office of Science (“SC”). There are advantages and disadvantages to each. The establishment of the program within EM would place the effort close to the cleanup program, but might discourage outside-the-box thinking. ARPA-E might have the right adventurous spirit, but it is a developing effort and a new EM initiative might divert it from its general energy mission. A free-standing program would require a statutory change and would present all the challenges of building a new enterprise. And placement within SC could ensure good connection to science, but might build a program that is too distant from the challenges that EM confronts. In the near term, the decision as to where to start the program should be guided by a judgment of where the program could best be nurtured in its early years in a difficult funding environment.¹²

¹¹ If an intractable problem is worthy of pursuit through the program, it may be appropriate to launch more than one initiative to solve it. The pursuit of parallel efforts provides a greater likelihood that a solution will be found.

¹² We recognize that there may be advantages, over time, in placing the various elements of science and technology advancement that we propose under single management. In that way, incremental technology development, high-impact technology development, and fundamental research could be linked to each other in way that provides a clear path to full-scale implementation. Given the need to build some of these activities from scratch, we think it best to house each of them initially in a way that allows each element individually to thrive and succeed. In order for this approach to work, however, there should be some coordination across the portfolio to assure that the boundaries of the various elements do not become barriers.

Program leadership is as important, if not more important than the home of the program. The program leadership should include technical experts (i.e., PhDs) in relevant science and engineering fields, experience in the management of technology, and field experience.

c. Fundamental Research

The pursuit of fundamental research that bears on EM challenges could advance understanding in important areas and pave the way for both incremental improvements and high-impact technology. Given the inevitable long term for the cleanup effort, there is ample time for advances in science to have significant impact on the cleanup program.

EM should seek to harvest science that bears on its task from the existing programs of the Office of Science (“SC”). But investment that bears directly on the EM mission is also appropriate. For example, research on the atomic- and molecular-scale chemistries of waste processing and contaminant immobilization in engineered and natural systems could yield many benefits. We envision a program tailored specifically to support the EM mission might justify an incremental expenditure of \$25M/year. It is appropriate that this program have a home in SC, but there should be tight coordination with EM and with the office pursuing high-impact technology development. Workshops involving potential participants should be held at an early stage to lay the groundwork for an EM-related fundamental research program. Some preliminary thoughts by the Task Force for appropriate points of focus are set out in Appendix 5. The program, like other efforts pursued by SC, should be subject to stringent independent peer review.

* * *

In summary, we advocate a comprehensive program that includes incremental technology development, the pursuit of game-changing technology, and advancement of the scientific foundations for the EM work. Indeed, work that bears strongly on the EM mission could serve to meet broader needs in the Department. For example, the initiative on subsurface science could not only serve EM’s needs, but also facilitate the work of the Offices of Fossil Energy and Nuclear Energy.

In order to build a foundation for a program that is sustained, it is necessary to build the case for the work's value within DOE, the Office of Management and Budget, and the Congress. We urge the periodic assessment of the programs' effectiveness and a willingness to make adjustments as the programs mature. Metrics of effectiveness may be essential for this purpose, supplemented by anecdotal information of significant and/or interesting program successes. The Department has done a poor job in the past in explaining how its investment in technology has facilitated its cleanup efforts.

Although this report focuses on the need to apply technology more effectively, the Task Force is of the view that a difficult but comprehensive restructuring of the cleanup program is necessary. DOE, the regulators, and the various other stakeholders should confront the fundamental conflicts that arise among the deadlines in the compliance agreements, the regulatory requirements, the availability of technology for the successful completion of the work in a timely fashion, and current and future budget realities. There need to be adjustments in the program that will provide a higher probability of long-term success, as well as the possibility of earlier completion of the program, significant cost savings, and risk reduction. In the long term, the public will be served by a fundamental realignment of the cleanup program to one that is based on the achievement of a balance of risk and cost and that is built on a foundation of scientific and technical advance. We acknowledge the difficulty in achieving consensus on a new approach among all the stakeholders with interests in the cleanup program. But change is necessary.

2. University Engagement

The national laboratories should be an essential participant in the efforts to develop technology to address the EM mission. But there also should be a focused effort to involve universities. We give special mention to universities because the EM program will proceed for decades and there is a need to maintain a close connection with universities in order to provide a pipeline of fresh ideas, to access advances in engineering and science, and, most importantly, to provide highly educated personnel for participation in the EM program in the decades ahead. Given the scope of its challenges, EM needs to develop a pipeline of technically sophisticated individuals through connection with universities.

The Nuclear Energy University Program provides a good model for executing a university program that is directed at EM needs. \$10 million/year would be an appropriate target funding level. Like the NE effort, the program might support individual research projects at \$200-300K/year, with forward funding of 3-year awards and an option for an extension of 2 years. Doctoral and post-doctoral fellowships might appropriately be part of the program.

Moreover, EM might consider other efforts to build the skilled workforce it will need on into the future. For example, EM might engage undergraduates in EM projects over the summer (with an eye to possible future employment), pursue workshops involving academia and industry concerning EM challenges and solutions, and promote programs to encourage current employees to pursue advanced degrees. The aim is to build a community to help EM meet its long-term needs.

3. Funding

The various recommendations, if pursued at the levels we recommend as eventual targets, require budgetary allocations as follows:

Incremental Technology Development	\$30-50M/year
High Impact Technology Development	\$75-100M/year
Fundamental Research Program	\$25M/year
EM University Program	\$10M/year

The total cost of the recommendations is thus in the range of \$140-185M/year. We readily admit that the estimate of the appropriate funding is a judgment by the Task Force (several of whom have extensive experience with the cleanup program) and that monitoring and adjustment of the investment over time is appropriate. We also recognize the difficulty of an investment at this scale in the near term. But, these funds constitute only about 3% of the EM budget and this investment need not arise from new budget outlays, but could come in large part from the existing EM budget. We believe that the savings that are likely to arise from an investment along the lines we propose will amply repay the costs over time. Moreover, the usage of incentivized contracts -- that is, sharing the savings arising from new technology with the

contractors -- may facilitate the application of new technology in ways that reduce the budget impact.

Although we recognize that budgetary support at the proposed levels may be difficult to achieve in the near term, we believe it is essential that the support for the program be sustained and predictable, regardless of the level, in order to allow time for meaningful advances to be made and to attract top researchers to participate. The development of relevant science and technology for the EM program needs to be an ongoing priority.

4. Management

As noted above, the development and deployment of improved technology is an important ingredient to the fulfillment of the EM mission. Technology is necessary for success, but by itself is insufficient. What is needed is the application of system integration in the broadest sense of the term.

The aim should be to develop approaches that optimize the entire system, not just one stage of the cleanup. It serves little purpose to develop a technology that saves cost at one stage, but increases costs in other stages. By way of example, processing technology must be developed with an eye to its implications for the disposal of any resulting waste. Similarly, advances in disposal technology should be evaluated with an eye to their implications for the processing stages of cleanup. Consideration of the entire system is a necessary perspective in decisions as to the investment in technology.

There is another broader aspect of system integration that requires attention. The cleanups must take place in a complex policy, regulatory, and political context. In order for technology development to be effective, it must fit into a policy and regulatory regime. Any constraints must be identified early and addressed. For example, in order to facilitate the application of technology, it may be necessary to obtain changes in regulatory requirements. For example, the adoption of new cleanup approaches may require modification of regulatory requirements from technology-based standards to performance-based standards that provide necessary protection of workers, public health, and the environment.

Moreover, other relevant stakeholders must be engaged in the technology decision-making at an early stage so there is a foundation for the acceptance of new approaches. For example, the program offices and the contractors need to be intimately involved because the application of a new approach requires their willingness to invest in it. Technology development will be ineffective unless it addresses “customer” needs. In fact, incentives might be considered in order to encourage the application of new technologies; contractors might be allowed to reap some of the savings and be protected from some of the risks that derive from the implementation of new technology.¹³ In short, technology “push” arising from the development of technical advances should be coupled with technology “pull” by those who could beneficially apply the technology.¹⁴

The scope of engagement must even extend beyond regulators, program offices, and the contractors. The relevant stakeholders in this context also include citizen groups, and often others, such as governors and other political figures interested in the affected sites. In order for a new approach to succeed, there needs to be an awareness of and a capacity to accommodate the full range of factors that can impact the implementation of new technology. The recognition of the need for “buy-in” argues for early and extensive interaction with all relevant stakeholders in order to build a willingness to apply new approaches. Regular workshops involving the relevant stakeholders can serve to build a foundation for acceptance of new ways of working.

In this context, DOE needs to be sophisticated in its communications and completely candid. The technology program should establish realistic expectations and make sure that the stakeholders understand not only the benefits of new approaches, but also the costs. Beyond that, DOE should make sure that stakeholders are aware that surprises can and will occur. Given the complexity of the technology that is required for the cleanups, there will be large uncertainties and risks. It is important for DOE to avoid creating false expectations.

¹³ As discussed above, the cleanup of Rocky Flats was facilitated by contractor incentives. *See* GAO Report, *supra* note 5.

¹⁴ Of course, any such incentives will require effective contract management. Contractors should be held accountable for resolving issues that arise and not deflecting accountability back to DOE.

We recognize that the need for broad system integration places difficult demands on the relevant leadership. Although the EM leadership should involve individuals with scientific sophistication and experience in the relevant core disciplines, a broad range of skills is necessary.

Conclusion


DOE is obligated to complete the cleanup of the legacy of past operations. The costs will be astronomical and the effort will continue for many decades unless ways are found to do the job faster, cheaper, and better through technology. In fact, the current program is unlikely to achieve its goals and stay within its current cost baseline unless significant changes are made. In these circumstances, there is no real choice but to make investments in technology that can enable success. In recent years, those investments have fallen too low. In addition to the need to advance new technology, there is a need to address the impediments to its successful implementation that can arise because of the wide array of groups that have stakes in the cleanup effort. Aggressive efforts to engage all stakeholders in a revitalized cleanup program are necessary.

Appendix 1 – Task Force Charge



The Secretary of Energy
Washington, D.C. 20585
May 15, 2014

MEMORANDUM FOR THE CO-CHAIRS
SECRETARY OF ENERGY ADVISORY BOARD

FROM: ERNEST J. MONIZ 

SUBJECT: Establishing a Task Force on Technology Development for Environmental Management

I request that you form a Secretary of Energy Advisory Board (SEAB) Task Force on Technology Development for Environmental Management (EM). While the Department's Office of Environmental Management has made significant progress in closing a number of projects, many of the most challenging projects remain and will for decades to come. Technology development for environmental management holds promise for accelerating cleanup and reducing cost. The Task Force will assess the value of a renewed EM science and technology development effort and how such a program would be structured.

Purpose of the Task Force: The Task Force on Technology Development for Environmental Management should examine and report on the following:

- The opportunities and barriers to science and technology development for cleanup.
- The means to implement a program to develop such technology.
- The funding of the program.

Designated Federal Official: Corey Williams-Allen, Deputy Director, Office of Secretarial Boards and Councils

Schedule: The Task Force will provide a brief progress report to SEAB in September 2014. The Task Force will complete its work, submit a report of its activities to SEAB, and make a presentation at SEAB's December 2014 meeting. The Task Force is expected to carry out most of its work in sessions open to the public.

Appendix 2 – Biographies of Task Force Members (*indicates a SEAB Member)

Richard A. Meserve* (Chairman) is Senior Of Counsel with Covington & Burling LLP, a Washington-based law firm and President Emeritus of the Carnegie Institution for Science. His legal practice involves matters at the intersection of science, law, and policy. He served as Chairman of the Nuclear Regulatory Commission from 1999 to 2003. He is a member of the Secretary of Energy Advisory Board and of the National Academy of Engineering and a Fellow of the American Academy of Arts and Sciences, the American Philosophical Society, and the American Physical Society. He has chaired or served on many committees producing reports bearing on science and technology, including many undertaken by the National Academies of Sciences and Engineering. He has a J.D. from Harvard Law School and a Ph.D. in applied physics from Stanford University.

Gerald Boyd is Vice President of Southeast Operations for The S.M. Stoller Corporation, a wholly owned subsidiary of Huntington Ingalls Industries. Gerald has over 30 years of Federal and State Government experience in the environmental management (EM), science, and emergency management arenas. His experience includes 21 years with the Department of Energy, during which he served 8 years as Manager of the Oak Ridge Office and was responsible for an annual \$2B federal investment in environmental management, science, nuclear fuel supply, and national security programs. Prior to moving to Oak Ridge, Gerald served as Deputy Assistant Secretary for Science and Technology in the Office of Environmental Management at DOE Headquarters. He has extensive training and experience in project and program Management, safety and health, and nuclear and radiological management, as well as in depth knowledge and understanding of the overall EM program.

Rafael L. Bras* is the provost and executive vice president for Academic Affairs at the Georgia Institute of Technology. He is a professor in the School of Civil and Environmental Engineering and School of Earth and Atmospheric Sciences. Prior to becoming provost, Dr. Bras was Distinguished Professor and Dean of the Henry Samueli School of Engineering of the University of California, Irvine (UCI). For 32 years prior to joining UCI he was a professor in the departments of Civil and Environmental Engineering and Earth, Atmospheric and Planetary

Sciences at MIT. He is past Chair of the MIT Faculty, former head of the Civil and Environmental Engineering department and Director of the Ralph M. Parsons Laboratory at MIT. He has served as advisor to many government and private institutions.

Thomas O. Hunter retired in July 2010 as President and Laboratories Director of Sandia National Laboratories, a multi-program laboratory operated by Sandia Corporation for the U.S. Department of Energy's National Nuclear Security Administration. In May, 2010, Dr. Hunter led the federal government's scientific team that worked with BP officials to develop and analyze solutions to the BP oil spill. Early in 2011, he was appointed Chairman of the Department of Interior's Ocean Energy Safety Advisory Committee (OESAC), a committee charged with identifying future technology needs for offshore oil and gas development. Dr. Hunter serves on many advisory boards for universities and government entities and is the author of numerous technical papers and presentations. He earned a B.S. in mechanical engineering from the University of Florida, an M.S. in mechanical engineering from the University of New Mexico, an M.S. in nuclear engineering from the University of Wisconsin, and a Ph.D. in nuclear engineering from the University of Wisconsin.

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David Kosson is the Cornelius Vanderbilt Professor of Engineering and Professor of Civil and Environmental Engineering at Vanderbilt University, where he also has joint appointments as Professor of Chemical Engineering and Professor of Earth and Environmental Sciences. Professor Kosson also is the Principal Investigator for the multi-university Consortium for Risk Evaluation with Stakeholder Participation (CRESP). Professor Kosson's research

focuses on management of nuclear and chemical wastes, including process development and contaminant mass transfer applied to groundwater, soil, sediment, and waste systems. Professor Kosson has participated in or led many external technical reviews on nuclear waste processing for the Department of Energy including for tank wastes and a range of technology approaches at Hanford, Savannah River and Idaho sites. He received his Ph.D. in Chemical and Biochemical Engineering from Rutgers University, where he subsequently was Professor of Chemical and Biochemical Engineering.

M. David Maloney is Emeritus Technology Fellow at CH2M HILL, and serves on the firm's Technology Leadership Board and Sustainability Leadership Board. He was the Technology Director 1997-2011 for CH2M Hill's cleanup work at DOE nuclear sites. For the Rocky Flats closure project, Dr. Maloney partnered with the EM-50 Science and Technology Program to create a risk/cost-shared approach that became a model and a Congressional Line Item for the weapons complex that saved over \$350M. Prior to CH2M Hill, his career focused on development of sustainable infrastructure (waste, energy, water systems) in 25 countries, where he worked in the roles of financial due diligence, investor-owner-operator, performance standards, facility regulations and licensing, and design-build-operate. He also served as Assistant to the General Manager, Energy and Environment Programs, at Argonne National Laboratory where he focused on technology transfer to industry. He has participated in several National Academies of Science study panels from 1997 to date supporting DOE EM inquiries. Dr. Maloney has a Ph.D. in Physics from Brown University and was Research Associate at the Nuclear Research Center, Karlsruhe, Germany.

Appendix 3 – The History of EM Investment in Technology

The pursuit of technology in the EM program has varied significantly over the years.

PHASE I: CENTRALIZED STRUCTURE (1989- mid-1990s)-Technology budget went from \$50M to \$400M/Yr

The EM Technology Development Program began in 1989 as a centralized Headquarters-driven program. The basis for the program was a comprehensive plan developed internally to guide the program by identifying technology needs, approaches to be used in developing, testing and evaluating technologies, and mechanisms for deployment of technologies.

The Program focused on late stage technology development and demonstration with a 2-5 year Return on Investment. Very little fundamental research or science was done.

Peer Review was largely internal and user, stakeholder, and regulator involvement was limited.

The advantage to this management approach was a very quick start to a complicated function. The program yielded many early successes that have been fundamental to environmental cleanup activities over the years, such as a better understanding of fate and transport of contaminants, improved waste forms, alternative treatment methods, robotics capability, and improved modeling systems.

There was a view by some that no technology development was needed. Some felt that the contractor community had all it needed to do the job and funds for new technologies would better be spent on ongoing cleanup activities.

PHASE II: DECENTRALIZED STRUCTURE (Mid- to Late-1990s)-Budget went from \$400M-\$240M/Yr

Pressures from DOE management, field elements, end users, regulators, OMB, and Congress forced a major change in the Program in the mid-1990s. The pressures came from criticism that the program did not involve the field elements, end users and regulators thus hampering or even preventing effective deployment of technologies. There was a view that the return on investment was not being realized as previously promised and that the program was not addressing some of the more fundamental issues related to waste forms. The Office of Science did little research in this arena although it did have a subsurface science program that greatly informed the soils and groundwater remediation efforts of EM.

The Program changed its title from the EM Technology Development Program to the EM Science and Technology Program. The Program was completely reorganized to establish Focus Teams around major waste streams. These teams were decentralized and managed at the field level. Representative from all sites having these waste streams were members of the focus teams.

Several Crosscutting Programs, such as "Characterization", "Robotics" and "Separations" remained centralized at HQ since they supported each of the field based Focus Teams. A few special initiatives were established such as the Rocky Flats Initiative (RFI) to focus on high priority/high visibility projects to bring as much technology as possible to assure their success.

The EM Science Program was established at HQ with a \$50M/Yr budget to address the longer term and more fundamental concerns across the EM Complex.

A robust external independent review program was established with the National Academy of Sciences.

Technology Roadmaps were established to map technologies from development to deployment.

The Interstate Technology Regulatory Council (ITRC) was established to involve State regulators early in the technology permitting process.

During these years the budget became constrained and State compliance agreements took precedence. As a result, the budget began to decline.

The decentralized Field Focus Team approach brought much more involvement with the users and the deployment of technologies into cleanup project increased substantially. Regulators were much more involved early in the permitting process, which enhanced the use of innovative technologies. The improved peer review process added much needed credibility to the way S&T was being managed by EM.

PHASE III: TECHNICAL ASSISTANCE AND SUPPORT (Early 2000s)-Budget went from \$240M to \$60M/Yr

In early 2000 EM Management again asked for a change in the program. The EM Program shifted to a "Closure" mode with the view that a robust technology program was not necessary.

The EM Science Program was transferred to the Office of Science with accompanying EM budget and selected EM personnel. EM funding for the program ceased within a couple of budget cycles because SC had no funding source for it. As a result, the science component was abolished.

The budget for the remaining Technology Development Program was reduced dramatically and the Focus Teams and Crosscut Programs ceased to exist.

The Program shifted to more of a technical assistance and support program for special needs as they arose.

PHASE IV: CURRENT STRUCTURE (Mid-200s to Today)-\$24M/Yr to \$13M/Yr

The current program has a technology development budget that is far below the historical levels.

The EM Program through the earlier phases changed strategies in moving from a 30-year plan to a 10-year plan and then to a 5-year plan. These ever shifting horizons affected the technology development program. Indeed, any investment could be questioned with such short plan horizons given the time that would be needed from bench to deployment.

There now is recognition that the EM Program will have a much longer and expensive tail than previously anticipated, allowing for a robust technology program to bring great value. But regulatory compliance issues have placed greater pressure on the ever decreasing EM budget, driving down technology investments.

Appendix 4 – Game-changing Concepts for EM

The successful development and delivery of truly game-changing concepts will require a strong connection to the EM program in order to identify and prioritize specific opportunities with a potential for high return. But, as discussed in the text, the identification and pursuit of such technologies will require engagement with the wide range of stakeholders. Some suggestions for areas possibly worthy of pursuit include the following:

- **In-situ treatment and stabilization**

Background: High costs of groundwater/soil remediation and facility D&D are largely driven by efforts to remove, treat and dispose of contaminants. These approaches are energy-intensive, manpower-intensive, often inefficient (requiring years of operation in some cases), and can create difficult-to-dispose of secondary waste streams with associated transport costs. There are opportunities to reduce cost as well as to provide long-term protection for the public more efficiently and effectively.

Game-changing opportunities: Use chemical and physical properties to “fix” or attenuate contaminant migration. Such an approach must be coupled with good characterization of the contamination and environmental conditions, fundamental understanding of chemical and physical environment, and models that can accurately portray and assess contaminant behaviour.

Examples:

- Groundwater and soil remediation – replace energy-intensive treatment systems (such as pump & treat) with lower-energy and/or natural attenuation approaches. Examples: Replacement of SRS pump and treat systems with ‘funnel & gate’ for metals and radioactive contaminants, “edible oil” systems for chlorinated solvents, and air stripping systems for mercury contamination.
- Facility D&D – shift from decontamination and demolition and physical dismantlement/removal approach to an *in situ* decommissioning approach where residual contaminants are safely entombed in intact facility. Examples: In situ decommissioning of P and R reactors at SRS.
- Residual tank waste – currently, after tanks are emptied, extensive (and multiple batch) chemical cleaning is performed to reduce tank contaminant inventories to the lowest possible level before the tanks undergo closure by filling with specialized grouts. Multiple cleanings results in hundreds of thousands of gallons of additional waste that must be processed for appropriate disposal. New approaches that can fix or stabilize these contaminants in place, coupled with models that can demonstrate long-term immobility would save time and money.

- **New approaches to subsurface assessment and monitoring**

Background: Long-term monitoring of remedy effectiveness at cleanup sites is traditionally accomplished through the use of large numbers of groundwater wells that

are routinely sampled and analysed for certain contaminants. Hundreds of locations and thousands of wells will require assessment and monitoring for decades.

Game-changing opportunities: Couple use of “marker” or indicator species analyses with advanced contaminant transport models to enable better understanding of contaminant migration, improving remedy selection and reducing need for wells, sampling and associated analyses. Use advanced remote sensors and wireless communications technologies to reduce manpower and laboratory efforts for analysis.

- **Reduced complexity in waste treatment and waste form processes**

Background: DOE has ~80M gallons of liquid radioactive waste in underground tanks that must be processed into a stable long-term waste form. Standard waste treatment and waste form production processes for liquid radioactive waste depend on large, expensive, dedicated waste processing facilities that take years (even decades) to design and build. Moreover, the processes that are used in these facilities often require extensive support, including feed and product removal infrastructure that must be maintained during the life of the facility and decontaminated upon completion.

Game-changing opportunities: Use concepts of process intensification/advanced manufacturing to simplify the processing approach, eliminating the need for the time and cost of huge dedicated facilities, while reducing complexity and improving efficiency. Shift the processing focus from expensive dedicated facilities to “right-sized” modular at- or in-tank approaches that provide flexibility. Develop acceptable simpler waste forms that can be manufactured in near-tank environments. At the same time, there are opportunities that can arise from a search for more versatile chemistries and processes so that fewer separations are needed. That is, separations (pretreatment) and treatment could be integrated.

Example applications:

- The potential for combining Small Column Ion Exchange (SCIX) and modular Interim Salt Processing (ARP/MCU) capability at SRS to eliminate the need to construct the full-scale Salt Waste Processing Facility (SWPF). Using advances in separations chemistry and filtration technology, SCIX and ARP/MCU capabilities can be combined to equal or surpass the processing throughput of the larger SWPF facility.
- Multi-element ion exchange (Advanced Liquid Processing Systems) operated at the Fukushima Daiichi site show application of high throughput systems for at-tank cleanup of contaminated liquid waste. Advances in resins could broaden consideration for non-elutable resins as a final waste form.
- Research into new nano- or bio-materials could enable the durable sequestration of hazardous or radioactive constituents.

Appendix 5 – Potential Areas for Scientific Pursuit

Scientific advances that could have significant impact on the cleanup program include the following:

Rapid Characterization of Chemical and Radioactive species (In process and in-situ)

This work would support the development and understanding of processing, waste forms, and subsurface contamination. It would include advanced analytical techniques, improved sensors, instrumentation systems, and other enabling technologies for remote sensing. A general objective would be the development of new methods to quickly identify species and establish relative compositions in challenging environments, including high radiation fields and subsurface disposition.

Chemical Sciences for Advanced Chemical Separations and Alternative Waste Forms

This work would include development of scalable new methods to perform separations in the complex environments of waste tanks and waste processing facilities. Reacting flows of many species and phases must be understood to better resolve which techniques can more efficiently be applied to specific waste streams. A general objective would be an understood suite of basic methods for separations or more efficient and durable waste forms that could be further developed into full scale if needed.

Fate and Transport in Geologic Media

This work would allow a better understanding and more efficient prediction of contaminant behavior in the subsurface. It would include models and methodologies for understanding geochemical interactions, hydrogeologic flow, and human uptake. It also would include development of remediation strategies and validation of models. A general objective would be a set of models that are validated and are accepted and consistent with regulatory guidelines and a set of proven remediation techniques which could be verified at larger scale in the field.

Advanced Computing and Information Systems

This work would include tailoring the fundamental of advanced computing to the problems and issues in waste processing and remediation. Enhancement of specific architectures, development of solution-focused algorithms, and tailored applications would all be investigated. A general objective would be a computing environment and information management framework that could be deployed across EM, efficient to operate, and accepted in the regulatory environment.

Regulatory-related Research

The various categories of radioactive waste are characterized by source rather than risk. Research could provide a technical foundation for change that could result in significant savings in cost and time. For example, the calcine waste streams at the Idaho site are defined as HLW, as are many other waste streams at other sites that have comparatively low activity. Redefinition could open options for safe, but lower cost, disposal pathways. Similarly, research could support conversion from technology-based standards requiring usage of borosilicate glass to performance-based standards could allow usage of other materials that provide equal or better durability and resistance to leaching.