SANDIA REPORT
SAND2012-3622
Unlimited Release
Printed May 2012

Retrospective on the Seniors' Council Tier 1 LDRD Portfolio

William P. Ballard

Prepared by
Sandia National Laboratories
Albuquerque, New Mexico 87185 and Livermore, California 94550

Sandia National Laboratories is a multi-program laboratory managed and operated by Sandia Corporation, a wholly owned subsidiary of Lockheed Martin Corporation, for the U.S. Department of Energy's National Nuclear Security Administration under contract DE-AC04-94AL85000.

Sandia National Laboratories

Issued by Sandia National Laboratories, operated for the United States Department of Energy by Sandia Corporation.

Printed in the United States of America. This report has been reproduced directly from the best available copy.

Available to DOE and DOE contractors from
U.S. Department of Energy
Office of Scientific and Technical Information
P.O. Box 62
Oak Ridge, TN 37831

Telephone: (865) 576-8401
Facsimile: (865) 576-5728
E-Mail: reports@adonis.osti.gov
Online ordering: http://www.osti.gov/bridge

Available to the public from
U.S. Department of Commerce
National Technical Information Service
5285 Port Royal Rd.
Springfield, VA 22161

Telephone: (800) 553-6847
Facsimile: (703) 605-6900
E-Mail: orders@ntis.fedworld.gov
Online order: http://www.ntis.gov/help/ordermethods.asp?loc=7-4-0#online

SAND2012-3622
Unlimited Release
Printed May 2012

Retrospective on the Seniors' Council Tier 1 LDRD Portfolio

William Ballard
Center for California Weapons Systems Engineering
Sandia National Laboratories
P.O. Box 969
Livermore, CA 94551

Abstract

This report describes the Tier 1 LDRD portfolio, administered by the Seniors' Council between 2003 and 2011. 73 projects were sponsored over the 9 years of the portfolio at a cost of $10.5 million which includes $1.9M of a special effort in directed innovation targeted at climate change and cyber security. Two of these Tier 1 efforts were the seeds for the Grand Challenge LDRDs in Quantum Computing and Next Generation Photovoltaic conversion. A few LDRDs were terminated early when it appeared clear that the research was not going to succeed. A great many more were successful and led to full Tier 2 LDRDs or direct customer sponsorship. Over a dozen patents are in various stages of prosecution from this work, and one project is being submitted for an R and D 100 award.

Acknowledgements

The Council would like to thank the LDRD office for years of support in processing these Tier 1 LDRDs. Hank Westrich, Keith Ortiz, Paul Rockett and Sheri Martinez were all extremely helpful, and the support of the LDRD budget analysts and others in the office was also outstanding.

Council Members

Over the years, the council has evolved, but the following were members at one time or another, and participated in the Tier 1 process: Dick Damerow, Lyndon Pierson, John Emerson, Dana Powers, E. Jones, Arian Pregenzer, Lawrence Larsen, Dan Tichenor, Clif Olson, Bill Ballard, Kevin Linker, Pat Brady, John Naegle, Tan Thai, Joe Michael, Joe Schoeniger, Ed Cole, and Kurt Lanes. Many of these members have since retired.

Table of Contents

Table of Contents

Introduction

In 2001 the Seniors' Council, which advises the Chief Technology Officer (VP of R&D), was asked to review the Laboratory Directed Research and Development (LDRD) program. Their report is attached in Appendix A. One of their recommendations was that the LDRD office establish what became known as the Tier 1 LDRD Investment Area. Tier 1 LDRDs were designed to be small (~$100k), short term (12-24 month duration), feasibility studies that would resolve some key aspect of a research area. One important attribute of the portfolio was that the proposal process was asynchronous with the regular LDRD program – acknowledging that good ideas can happen at any time. A primary criterion of the portfolio was that the research would resolve some key unknown that would allow the work to continue under the regular LDRD program or under direct customer sponsorship. In either case, the research risk was significantly lower after the Tier 1 project was completed. We also attempted to accept substantially more risk than the regular LDRD program.

Evaluation Criteria

Proposals were generally solicited informally (Daily News announcements, word of mouth) although there was a formal call on the LDRD web site describing our research area. Once a Principal Investigator (PI) contacted any member of the Council, we assigned a cognizant senior from the Council to work with the PI on preparing a proposal. We attempted to have the cognizant senior be competent in the research area, but this was not always possible.

The cognizant senior would work with the PI to craft a proposal suitable for Tier 1 funding. The key unknown must be carefully identified, a clear attack plan described for resolving the issue, and a success metric carefully designed. The cognizant would often have to iterate multiple times with the PI to obtain a cogent proposal. Once the proposal was complete, the cognizant would find several technical reviewers, sometimes from outside Sandia, to evaluate the proposal. The entire Council was given the opportunity to read the proposal, and at one of our monthly meetings, we would discuss the proposal, get a recommendation from the cognizant, and vote on whether to fund or not. On multiple occasions, there was sufficient interest or confusion that we asked the PI to come and present to the Council. We could then ask more detailed questions and come to a better funding decision.

We only funded about half of the ideas that were brought forth for a variety of reasons; the most common being that the PI could not bring sufficient clarity to the key unknown to convince us of its importance. Some of the proposed ideas were rejects from the regular LDRD process. Those that were considered too risky or involved multiple investment areas we considered. Those that were correctly rejected by regular LDRD process (Tier 2) we generally did not consider further.

Funding Profile

The Tier 1 effort was originally targeted at $1M per year

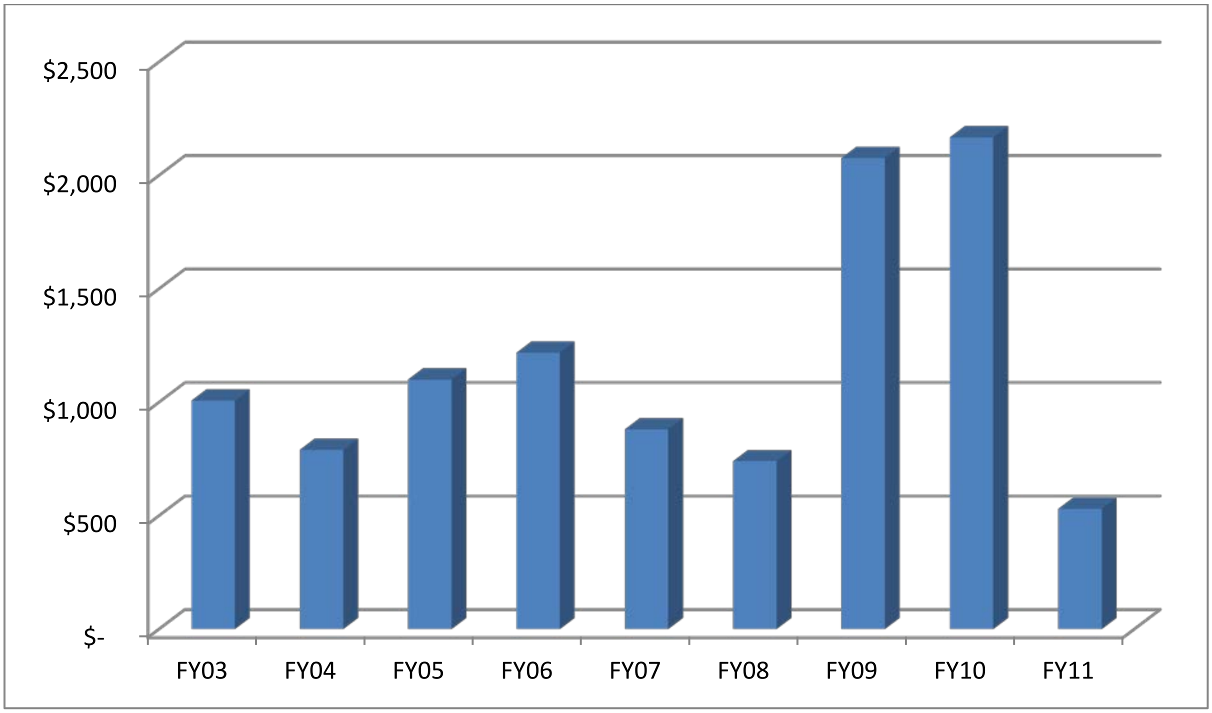

Figure 1 Tier 1 LDRD funding profile by fiscal year

As can be seen in Figure 1, the funding applied varied significantly from the nominal $1M over the years. In some years, good, creative proposals simply did not show up, and we did not fully use the money. In FY11, the remainder of the funds was redirected to the Early Career LDRD pool.

It is worth noting that in several cases Tier 1 LDRDs were augmented by Tier 2 late start money but this appears as Tier 1 funds. Finally, the impact of an additional $1M per year in FY09 and FY10 is evident. These additional funds were applied equally to Climate Change and Cyber Security in an attempt to get more innovative ideas started. The Cyber Security LDRDs have formed much of the basis for the current Cyber Investment Area research. One of the Climate Change investigations on the unintended consequences of climate mitigation turned out to be very useful when the GAO started investigating this area, and it turned out that we had already considered the impacts of upper atmosphere aerosol injection.

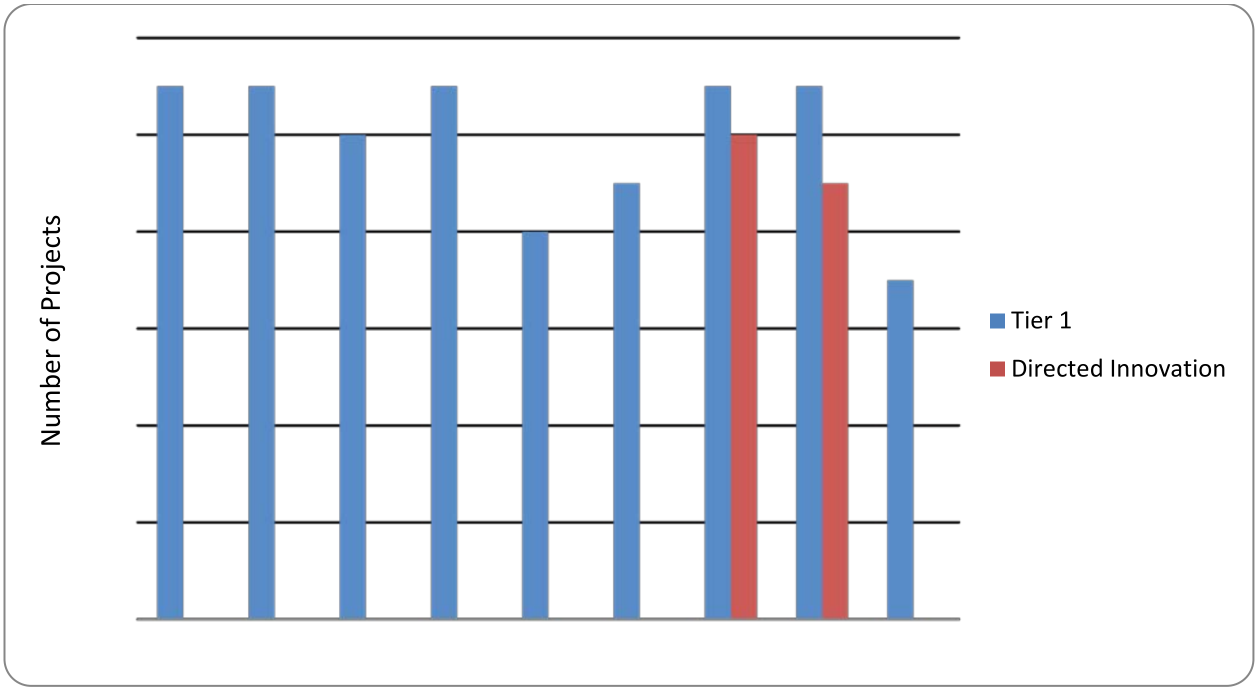

Figure 2 Number of LDRDs for Tier 1 and Directed Innovation

Figure 2 shows that we generally funded each LDRD at a ~$100k level, though some were larger and some smaller. Many of these projects spanned two fiscal years due to the asynchronous nature of our process.

Impact

It is early to judge the impact of some of the more recent projects. Remember that these were chosen to be at the TRL 1 level – ideas that needed some fundamental study either through modeling and simulation, analysis, or experimentation to measure some fundamental unknown. It takes years for this sort of work to reach fruition.

However, our early investment in quantum computing in 2005 eventually led to the creation of a quantum computing Grand Challenge LDRD (Tier 3). Additional Tier 2 LDRD funding moved the technology from our Tier 1 level to the Grand Challenge level. Similarly, we invested early in high efficiency photovoltaic work that is just now starting as a Grand Challenge.

Early in the portfolio, some of the Tier 1 LDRDs filled the role now filled by Early Career LDRDs. A number of new staff members took advantage of the mentoring by the cognizant Senior in learning how to write successful LDRD proposals, and have gone on to further success in the R&D community at Sandia. Notable here is Chi Chi May, Alec Talin, and Susan Altman.

We don't have good statistics on the number of publications that grew out of this research.

Intellectual Property

Many of the reports generated in the LDRD process have Patent Caution limitations, and we know of others that published the report after the patent was applied for, so a reasonable number of patents have come out of the process. Eleven of the final reports had Patent Cautions, and interestingly, the Directed Innovation work in Cyber Security accounted for

We encouraged the PIs to file appropriate technical advances (TAs) and patents. A recent run of the patent database showed that 40 TAs were filed, and 13 patents are in various stages of prosecution. Thus, over half of the 73 projects generated TAs. Some of the projects generated multiple patent applications. There were numerous provisional patents also filed, some of which were abandoned and some of which were eventually filed. Also, if the patents were filed as the result of Tier 2 LDRD follow-on work, we would not find these in the database.

Note that the most recent projects probably have not yet filed and so more patents are possible. We did not track any copyrighted works which might have picked up some of the computer codes. The ongoing patent applications for the novel cooling technique in Jeff Koplow's LDRD and the Physically Unclonable Function work of Jason Hamlet promise to provide significant intellectual property from the most recent batch of LDRDs.

Summary

Overall, the Tier 1 portfolio was a great success in terms of accepting high risk ideas. Not all of the Research ultimately succeeded, but those that did have had significant impact. The Seniors' Council feels that if the Tier 1 expectations are place on the regular IATs, they should allow asynchronous submissions, and be willing to spend additional time grooming the proposals.

List of All Tier 1 Projects

Project	Proposal	Title	PI Name	Proj Tot
59034	03-1067	3-D Large Eddy Simulation of Turbulent Flow based on One-Dimensional Turbulence Modeling	SCHMIDT,RODNEY C.	$ 180
60626	03-1072	The Derivation and Implications of 'Nearly Non-informative' Priors from Metadata-conditioning	COX,ROGER GARY	$ 75
60627	03-1096	New Seismic Event Detection Technology	LARSON,KURT W.	$ 100
61045	03-1099	Testing Recent Metal/Oxide Joining Discoveries in NW Component Manufacturing	JENNISON,DWIGHT RICHARD	$ 125
61046	03-1114	Photonic Encryption using All-Optical Logic	TANG,JASON D.	$ 180
61047	03-1117	Automated Video Screening for Unattended Background Monitoring in Dynamic Environments	CARLSON,JEFFREY J.	$ 100
62269	03-1174	Nanoporous-Carbon Adsorbers for Chemical Microsensors	SIEGAL,MICHAEL P.	$ 240
63972	03-1185	Molecular Electronics Test Platform	TALIN,ALBERT ALEC	$ 112
65558	03-1188	Detection and Reconstruction of Error Control Codes for Engineered and Biological Regulatory Systems	MAY,ELEBEOBA E.	$ 75
65559	03-1187	Direct Single Ion Machining of Nanopores	ROSSI,PAOLO	$ 120
66170	03-1206	Investigation of the Effects of Intense Pulsed Particle Beams on the Durability of Metal-to-Plastic Interfaces	RENK,TIMOTHY J.	$ 75
74548	04-1529	Risk of Biological Terrorism to Water Distribution Systems	ALTMAN,SUSAN J.	$ 235
75513	04-1537	Developing Algorithms for Predicting Protein-protein Interactions from Experimental Constraints	ROE,DIANA C.	$ 155
75514	04-1535	Instrumentation Development for Real Time Brain Wave Monitoring	ANDERSON,LAWRENCE F.	$ 150
78132	04-1530	Deciphering the Genetic Regulatory Code Using an Inverse Error Control Coding Framework	MAY,ELEBEOBA E.	$ 75
80836	04-1575	Reducing the Economic Consequences of a Successful RDD Event	MARTELL,MARY-ALENA	$ 35
80838	04-1576	Molten Salt-Based Growth of Bulk GaN and InN	WALDRIP,KAREN E.	$ 100
84315	05-1286	Micropower Chemical Fuel-to-Electric Conversion: A "Regenerative Flip"; Hydrogen Concentration Cell Promising Nea	WALLY,KARL	$ 50
85513	05-1303	MEMS-based Arrays of Micro Ion Traps for Quantum Computation and Quantum Simulation Scaling	BLAIN,MATTHEW G.	$ 407
89669	05-1359	Diffusionless Fluid Transport and Routing Using Novel Microfluidic Devices	REICHMUTH,DAVID	$ 113
90498	05-1358	Emulsion Technology for Sample/Contaminant Collection	BOURDON,CHRIS J.	$ 270
90499	05-1360	Visualizing Higher Order Finite Elements	THOMPSON,DAVID C.	$ 100
90506	05-1396	Bioagent Detection Using Miniaturized NMR	ALAM,TODD M.	$ 271
102608	06-1603	Understanding the Materials Physics for an Alternative for PZT 95/5	VENTURINI,EUGENE LEO	$ 321
103004	06-1811	Modeling and Simulation of Spectra Expected from Radiation Sensors Made from Arrays of MEMS Scale Capillaries	DERZON,MARK S.	$ 198
103005	06-1812	Ultrafast NanoLaser Device for Detecting Cancer in a Single Live Cell	GOURLEY,PAUL L.	$ 335
104949	06-1837	Hollow Waveguides for Instrumentation in Intense Radiation Environments	WEISS,JONATHAN D.	$ 150
104953	06-1836	Nanoporous Films for Epitaxial Growth of Single Crystal Semiconductor Materials	FAN,HONGYOU	$ 245
104955	06-1842	A MEMS-based Thermoacoustic Engine	APBLETT,CHRISTOPHER A.	$ 200
104973	06-1841	Development of Sample Preparation Methods for ChIPMA-based Imaging Mass Spectrometry of Tissue Samples	BEHRENS JR.,RICHARD	$ 128
115235	07-1628	MEMS Solar Energy Harvesting	NIELSON,GREGORY N.	$ 75
125276	08-1341	Atomic Shadow Microscopy	FOILES,STEPHEN M.	$ 107
126795	08-1391	Use of Ceragenins To Create Novel Biofouling Resistant Water-Treatment Membranes	ALTMAN,SUSAN J.	$ 90
126796	08-1392	Plastic Optical Fiber Hydrogen Sensor	WEISS,JONATHAN D.	$ 133
129145	08-1411	Feasibility Investigation of a Quantifiable and Objective Approach to Organizational Performance Enhancement	SCHOLAND,ANDREW J.	$ 95
129298	08-1413	Synthesis of Shape- and Size-controlled Platinum and Platinum Alloy Nanostructures on Carbon with Improved Durab	SONG,YUJIANG	$ 100

129299	08-1414	Laser Detection	THORNE,LAWRENCE R.	$	216
129970	08-1421	Small Space Object Imaging	KEARNEY,SEAN P.	$	100
130419	08-1430	Land-surface Studies w ith an Imaging Neutron Detector	MASCARENHAS,NICHOLAS	$	120
130420	08-1431	Plasmonic Enhanced Ultrafast Photoconductive Sw itch	SHANER,ERIC A.	$	110
135039	09-1356	Unintended Consequences of Climate Mitigation	BRADY,PATRICK V.	$	200
135040	09-1344	Authentication for High Exposure Cyber Systems	PIERSON,LYNDON G.	$	100
135041	09-1332	Molecule-Based Approach for Computing Chemical-Reaction Rates in Upper-Atmosphere Hypersonic Flow s	GALLIS,MICHAIL A.	$	125
135042	09-1331	Technologies for Autonomous Satellite Capture	WILSON,DAVID G.	$	125
135192	09-1333	A Fundamentally New Approach to Air Cooling	KOPLOW,JEFFREY P.	$	172
135802	09-1337	Molecular Fountain Based on Kinematic Cooling	CHANDLER,DAVID W.	$	100
137804	09-1342	Designer Catalysts for Next Generation Fuel Synthesis	THOMA,STEVEN G.	$	126
137807	09-1346	Reduced Order Models for Thermal Analysis	GARTLING,DAVID K.	$	193
139146	09-1401	Infrastructure for Nondestructive, Real-Time Fingerprinting of Integrated Circuits	BAUER,TODD M.	$	100
139352	09-1402	The Theory of Diversity and Redundancy in Information System Security	TORGERSON,MARK D.	$	235
139353	09-1400	High-Efficiency (>50%) PV Cells	NIELSON,GREGORY N.	$	100
139867	09-1403	Uncertainty Quantification for Large-Scale Ocean Circulation Predictions	SAFTA,COSMIN	$	169
140640	09-1411	Automated Verification of Concurrent Systems	BUENO,DENIS	$	100
140641	09-1408	Uncertainty Quantification of US Southw est Climate From IPCC Projections	BOSLOUGH,MARK B. E.	$	205
140764	09-1409	Quantitative Laboratory Measurements of Biogeochemical Processes Controlling Biogenic Calcite Carbon Sequestrat	LANE,TODD W.	$	215
140766	09-1410	Developing a Cyber Security Systems Methodology for Analysis of Life Cycle Protections	RICHTER,GARY W.	$	100
141507	09-1417	Development, Sensitivity Analysis and Uncertainty Quantification of High-Fidelity Arctic Sea-Ice Models	BOCHEV,PAVEL B.	$	182
143418	10-1138	Minority Carrier Recombination in III-Nitride Heterostructure Bipolar Transistors	WIERER,JONATHAN	$	125
145281	10-1152	Paradigms for Skill Assessments	AKHADOV,ELSHAN	$	99
145282	10-1148	Development Tow ard a Nano-Thermal Interface Material	SIEGAL,MICHAEL P.	$	99
145283	10-1151	Physically Unclonable Function (PUF) Based Softw are Authentication and Component Binding	HAMLET,JASON	$	160
145284	10-1150	System Metrics for Comparative Analysis of Cyber Security Systems	ROBERTSON,PERRY J.	$	100
145835	10-1149	Understanding the Physics of a Possible Non-Abelian Fractional Quantum Hall Effect State	PAN,WEI	$	175
145998	10-1153	Modeling Attacker-Defender Interactions in Information Netw orks	COLLINS,MICHAEL J.	$	170
147486	10-1172	Laser Wafering - Accelerating Moore's Law for Silicon Solar	FRIEDMANN,THOMAS A.	$	65
147940	10-1174	Molecular-Scale Measurements of Electric Fields at Electrochemical Interfaces	FARROW,ROGER L.	$	128
148067	10-1171	Nanoparticle Modification of Photodefined Nanostructures for Sensor and Energy Applications	POLSKY,RONEN	$	107
149404	10-1256	Room Temperature Detector Array Technology for the Terahertz to Far-infrared	SHANER,ERIC A.	$	125
150115	10-1275	Attosat Lorentz Augmented Orbit (LAO) Flight Dynamics	PALMER,JEREMY A.	$	100
153888	11-1067	BEAM ADAPT SONAR SYST	SHEAFFER JR.,DONALD A.	$	98
154764	11-1168	HI RES 3D IN SITU NANOFAB	BOGART,GREGORY ROBERT	$	123
155092	11-1202	APPL-SPEC MICRO-ION TRAP	MANGAN,MICHAEL	$	123
155326	11-1204	REALIZ OF PRACT ULTR LASR	SOH,BEOM SOO	$	98

Appendix A: Senior Scientists and Engineers Review Of the LDRD Program at Sandia National Laboratories

December 2001

Seniors' Council

Preface

The Senior Scientists and Engineers were asked to review Sandia National Laboratories' Laboratory Directed Research and Development (LDRD) program. This report constitutes Phase 1 of this effort and was developed by:

R. Damerow, 2561	L. Pierson, 9336
J. Emerson, 14172	D. Powers, 6400
E. Jones, 1123	A. Pregenzer, 5320
L. Larsen, 15300	D. Tichenor, 8732
C. Olson, 1600	

with the valuable assistance of Bryon Cloer and Chuck Myers, 1030.

Introduction

The Laboratory Directed Research and Development (LDRD) program was established at the DOE national laboratories to help maintain their technical vitality. It is critical to attracting and retaining the highest quality researchers. Reviews of the LDRD program at Sandia National Laboratories by external review groups have found the program to be. For example, a recent report by the Government Accounting Office found that LDRD programs at the national laboratories ". . .met DOE's guidelines" and "Adequate management controls exist to reasonably ensure compliance with LDRD's Guidelines" (GAO-01-927, September 2001).

However, within Sandia there is dissatisfaction with the LDRD program. Management is concerned about whether the LDRD program is fulfilling its promise. For example, is the program yielding truly innovative changes or are its contributions incremental in nature? Is the administrative overhead of the program excessive? Is it diluted by being spread over too many technical disciplines? Among staff, there is dissatisfaction with the program even among those who have successfully competed for funding, (as documented by the Summer 2001 Science and Technology Employee Satisfaction Survey). Many feel that it is not worth the effort to develop an LDRD proposal.

As part of its effort to address these dissatisfactions, the Mission Council asked the Senior Scientists and Engineers to review the LDRD program over the next year. This document represents Phase 1 of that review. In Phase 1 we identify issues associated with the LDRD program, suggest preliminary ideas for addressing selected issues, including the use of external review panels, and propose steps for Phase 2 of the review.

Issues

In Phase 1 we identified issues in four categories: program objectives, program strategy, program design, and the review process. These are summarized in Table 1 and discussed in the following subsections.

Table 1. LDRD Program Issues

1. *Objectives of LDRD Program are Unclear*
   - Maintenance of capabilities versus development of new capabilities?
   - Fundamental research or applied research?
2. *Strategies or Roadmaps for Investment are Lacking or Ignored*
   - How can LDRD results be linked to ongoing work more effectively?
   - Could follow-up to LDRD projects be improved?
   - Does growth in the number of investment areas spread resources too thin?
   - Do we have the right mix of sizes for LDRD projects?
   - Does the shrinking research time horizon result in more incremental projects?
3. *The Design of the LDRD Program May Exacerbate Problems*
   - Should there be an explicit process to link projects at different scales i.e., development of a bright idea, regular LDRD, and grand challenges?
   - Can we establish a more flexible schedule for proposals to increase creativity and to avoid hastily-written last-minute proposals?
   - Should we establish guidelines for market and competition analysis as part of the LDRD proposal process?
   - Are existing success metrics adequate?
4. *The Review and Approval Process is Often Inadequate*
   - Does management inappropriately pre-determine the outcome of the selection process?
   - Can we select reviewers based on technical qualifications rather than on availability?
   - Are internal reviewers capable of assessing proposals that lie outside traditional Sandia strengths?
   - How do we know that investigators are qualified to perform proposed work?
   - Do external review panels function as advocates rather than as objective reviewers?

A. Issues: Objectives of the LDRD Program

The LDRD programs at the DOE national laboratories have five objectives:

- Maintain the scientific and technical vitality of the laboratories,
- Enhance the laboratories' ability to address future DOE missions
- Foster creativity and stimulate exploration of forefront science and technology
- Serve as a proving ground for new research
- Support high-risk, potentially high-value R&D.

Clarifying these objectives and defining an appropriate balance among them is needed to guide LDRD program strategy and design. There is tension between the focus on "maintaining capabilities" as identified in the first objective and "developing new capabilities" as implied by the latter two. Also, the desired balance between "applied research" and "fundamental research" is not clear.

Articulating the balance between "maintaining existing capabilities" and "developing new capabilities" is particularly important as the funding level for Defense Programs Research Foundations (DP RF) declines (from >$100M in the 1980s, to ~$70M in the 1990s). LDRD activities now play a much larger role in "maintenance of existing capabilities," thereby decreasing the investment in more forward-looking objectives. Extrapolation of trends suggests that LDRD could shift to a predominantly "maintenance" role in the future. Indeed, changes mandated by Congress to the DP RF funding levels have already resulted in LDRD becoming an important source of funding for the maintenance of some weapons systems capabilities at Sandia.

Preparation to meet future mission needs of the Department of Energy (the second objective) is addressed through the LDRD Mission Technologies and Science and Technology Investment Areas. The structure of these investment areas is very complex, and is difficult for the uninitiated to navigate. Indeed, this labyrinthine structure may itself be a problem. These activity areas address both the DOE weapons mission and a broader set of possible future missions.

The third objective of the LDRD program is to stimulate exploration at the forefront of science and technology, and is potentially much broader than "future DOE missions." It also implies a need to use LDRD to fund fundamental or basic science research. Currently, fundamental research appears to receive less LDRD funding than do projects that address the first two objectives.

Senior management needs to address explicitly the appropriate balance among the objectives of the LDRD program.

B. Issues: Strategy of the LDRD Program

Without a clear articulation of the relative balance of the objectives, the strategy of the LDRD program has become unfocused. Strategic issues that should be addressed include the lack of technical roadmaps for many Sandia programs, reduced average funding per LDRD, and the need for appropriate metrics to monitor success of the LDRD program.

Clear strategies or roadmaps for investment are lacking or ignored in many program areas at Sandia. Good technical roadmaps identify critical hurdles and technical challenges that would be natural targets for LDRD proposals. Developing, communicating, and updating technical roadmaps could be an important step in increasing the relevance of LDRD projects. At Sandia, technical roadmaps are often formulated at too high a level and with insufficient detail to be effective in attracting LDRD proposals. In addition, investment areas often do not have lifetimes consistent with the progression of LDRD research. Qualitative technical calls are often reformulated dramatically on an annual basis, whereas a typical LDRD project lasts up to three years.

Effective technical roadmaps would also provide a framework for addressing other sources of dissatisfaction with the LDRD program at Sandia: they could improve linkages between LDRD projects and the technical strategies adopted by each of the investment areas, and linkages between LDRD efforts with ongoing, funded work. They would also provide a framework for assessing the implications and follow-on opportunities of LDRD projects after they are complete.

Another strategic issue is the growing number of LDRD investment areas. The number of investment areas supported by the LDRD program has grown by more than 50% in the last three years (from 11 in 1999 to 17 in 2002) while overall funding has remained around $80 million (See Attachment 1 for more details). The consequence is less LDRD support for any single investment area, which may contribute to smaller LDRD projects. For example, the average size of an LDRD project for FY1996 through FY1998 was $295K, whereas in FY2001 the average size is $252K. At other NNSA laboratories the average FY2001 project sizes were $345K for LANL and $283K for LLNL. Figure 1 shows the distribution of LDRD projects in FY2001 as a function of size.

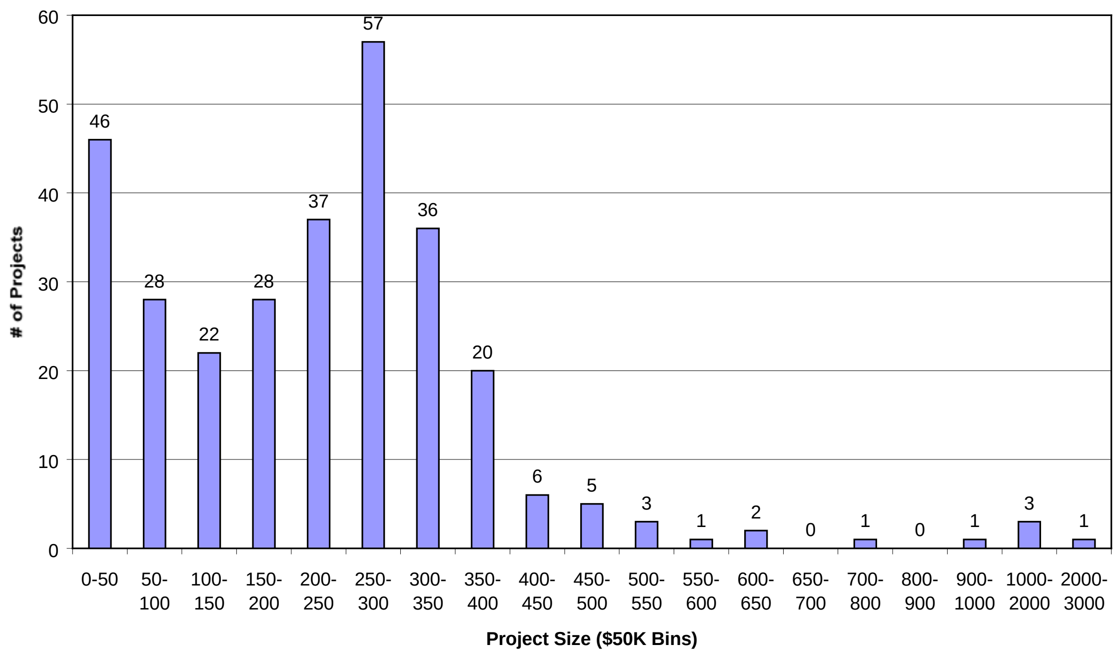

Figure 3 FY 2001 LDRD Project Distribution by Size

A review of the Sandia patent database shows that Grand Challenge LDRD projects generate about more patent disclosures as do normal LDRD projects, and are much more productive than externally funded Sandia projects. This may indicate that intellectual property generation scales with the size of the investment, or that multidisciplinary research of the type done in Grand Challenge LDRD projects is especially fertile. Understanding the connection between project size and productivity clearly deserves further attention.

To summarize the strategic issues, a lack of well-defined technical roadmaps, combined with shrinking research time horizons and growth in the number of investment areas may underlie trends toward less relevant LDRD proposals that are incremental in nature. This results in incremental improvements in maintenance of existing programs, and relatively small advances in future programs and innovation.

C. Issues: Design of the LDRD Program

Funds available for the LDRD program at Sandia are apportioned by the Mission Council between Grand Challenge LDRDs for large multidisciplinary research efforts (up to several million dollars) and smaller, regular LDRDs (on the order of $250K). There is no small-scale category of LDRD to develop “bright ideas.” Nor is there an explicit process to link projects funded at different scales, i.e., provisions are not made for a natural evolution from “bright idea” to regular LDRD to Grand Challenge. This may limit the productivity of the LDRD program.

The inflexibility of the schedule for submitting proposals is another potential problem. Whereas a fixed schedule may be required for regular and Grand Challenge LDRD proposals, innovative ideas often surface throughout the year. Establishing an ongoing process during the year for funding the development of new ideas could enhance innovation and creativity.

Another problem with the current LDRD program is that requirements for proposals and reports do not meet minimum scientific standards. For proposals, there are no requirements to analyze the potential market for a proposed technology or to demonstrate an understanding of the state-of-the-art outside Sandia. Not only does this discourage investigators from doing adequate “homework” prior to submitting a proposal, it also limits the information available to reviewers. LDRD reports suffer from similar lack of standards: many are received by the LDRD office without peer review. Poor reporting contributes to poor follow-on; and failure to publish results in peer-reviewed journals significantly reduces the value of LDRD research to the larger scientific community. Adopting standards for proposals and reports more consistent with the mainstream scientific community could significantly improve the quality of the LDRD program.

Several metrics have been used for evaluating the LDRD program, including the number of publications, number of citations, and the generation of patent disclosures. Sandia’s LDRD activities score highly using such metrics. However, whether they are the right metrics to assess all efforts is an open question. Their applicability to classified activities is also an issue. Examination of the metrics used to evaluate the LDRD program deserves further consideration.

D. Issues: The Review and Approval of LDRD Projects

The process that is used to solicit LDRD proposals and to review those proposals causes great frustration and dissatisfaction among the technical staff. On the highest level, many are concerned that the outcome of the selection process is predetermined by management. This leads to cynicism and a reluctance to make an effort to develop innovative proposals.

A second concern is that reviewers frequently do not have adequate technical expertise, even in the initial stages of review. Often reviewers are selected based on their availability, rather than on their technical qualifications. The result could be that speculative ideas do not get a fair hearing. This is compounded by a perceived bias toward proposals that provide strong evidence of eventual success.

Developing a review process that includes technical peers in addition to reviewers with more general expertise could also address the issue of identifying the need for review by external experts. External reviewers are critical to evaluating ideas that lie outside traditional Sandia strengths. For example, as we move into assessing opportunities for involvement in biotechnology or in applying technology to social problems, external review will be essential to assure our ideas are scientifically valid and relevant. A more complete discussion of the issues associated with external review is provided in the next section.

Changes in the review process alone are insufficient. As noted in the previous section, there is a significant disparity in the standards for proposals in Sandia's LDRD program and standards for proposals in the greater scientific community. Minimum standards would include demonstration of the scientific credibility of the proposed work, a discussion of the state-of-the-art in the research area, the requirements of the investigators to perform the work, and analysis of the competition and of the market. Many Grand Challenge LDRD proposals at Sandia do not meet these standards, some do not even include a bibliography. Even routine searches of patent literature and technology surveys are rarely conducted. These shortcomings in Sandia's program surely contribute to incrementalism and irrelevance.

Preliminary Ideas and Phase 2 Activities

This Phase 1 report has focused primarily on identifying issues with the existing LDRD program. Phase 2 will focus on developing ideas for resolving these issues. Even at this stage we have developed some preliminary ideas concerning the need for technical roadmaps, a possible alternative structure for the LDRD program, and the use of external review panels.

A. Phase 2: Objectives of the LDRD Program

In Phase 2 we will develop ideas for clarifying and balancing potentially competing objectives.

B. Phase 2: Strategy of the LDRD Program

The development and communication of effective technology roadmaps will be considered in Phase 2. If done effectively, such roadmaps can be a useful tool in aligning the resources of an organization to address the most difficult technical challenges. Their development and communication can help establish consensus among staff and management about critical technical challenges and realistic timelines. Table 2 includes some preliminary thoughts on these issues. (An example of an effective technology roadmap at Sandia is the *Robotics and Intelligent Machines – Critical Technology Roadmap; October, 1998*).

Evidence also suggests that larger projects may be more productive as measured by standard metrics. However, funding fewer projects at higher levels means that fewer ideas will be pursued. Is there a better mix of sizes for LDRD proposals that will address these issues simultaneously? Can better scientific metrics be proposed to measure the success of the LDRD program? These issues will be addressed in Phase 2.

Table 2. Development And Communication Of Technology Roadmaps
Management Responsibility – Develop and publish program goals (out at least 5 years) that are specific enough to be connected to particular projects. – Work with key staff to develop and publish a common understanding of specific scientific and technical hurdles (gaps) that must be overcome to achieve these goals. – Identify which hurdles are likely not to be addressed with programmatic funding and which are therefore good candidates for LDRDs. – Consistently communicate with staff (and get their feedback) about the goals and critical gaps. Use goals/gaps as criteria in requesting LDRD proposals. If goals/gaps become obsolete, update. – After the LDRD is complete, review its success in the context of goals/gaps. Follow-up with every LDRD project to determine how its results address key programmatic issues and what additional work should be undertaken.
LDRD Principle Investigator – Understand goals and gaps of SBU who is target for proposal. – For "idea proposals" that are unrelated to any existing SBU goals/gaps articulate why the idea could be important to Sandia's future mission. Regular LDRD and Grand Challenge proposals should be aligned closely with gaps/goals. – **In SAND report, address the contribution LDRD made to goals/gaps.**
Reviewer – Explicitly take into account published goal/gap analysis when evaluating proposals.

C. Phase 2: Design of the LDRD Program

The LDRD program involves a variety of objectives, one of which can be to initiate and develop new programs. For this objective, it is useful to look at program models developed elsewhere. In industry, one example is the "stage-gate R&D model" for commercial development, as shown in Figure 2. New ideas enter on the left side, and must pass though a series of decision gates to proceed on toward a final, useful, commercial product. A staged approach may have useful implications for the LDRD program.

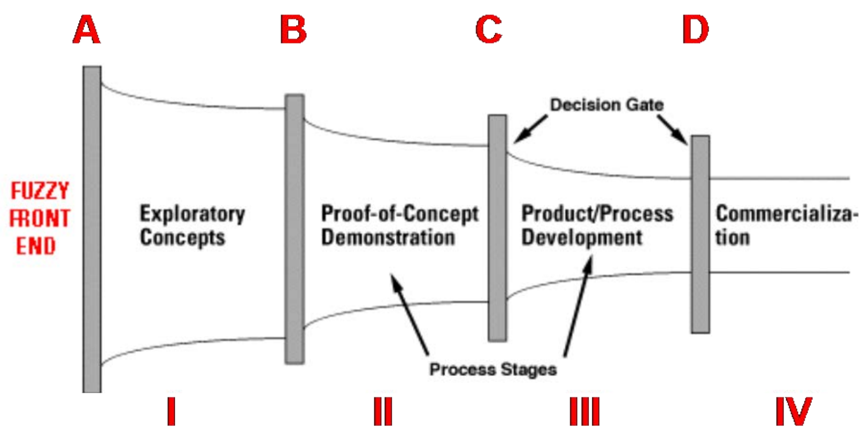

Figure 4 Example Commercial Stage-Gate R&D Model.

Our initial thoughts for the LDRD program plan are represented in a "strawman" tiered structure, as shown in Table 3. This is a three-tier structure, with distinctly different characteristics for each of the three tiers. Tier 1 is designed to encourage innovation and support new ideas that may or may not be related to existing SNL programs. To insure high scientific quality and encourage fresh ideas, Tier 1 proposals would be reviewed by a neutral, knowledgeable group (e.g., Senior. Scientists). Tier 2 is designed to contribute to existing program areas or newly-endorsed areas. Tier 2 proposals would be reviewed by investment area teams for programmatic considerations. Tier 3 (more like the current Grand Challenge LDRD) would lead to major multi-discipline programs. Tier 3 proposals would involve expert external review groups. The funding, start times, duration and reporting requirements would all be appropriately scaled for each tier, as shown in Table 3. This potential tiered structure for the LDRD program will be more fully developed in Phase 2.

D. Phase 2: Review and Approval of LDRD projects

In Phase 2 we will develop ideas for improving the review and approval process. The major issues are: which elements of the LDRD program will benefit most from external review? How can the costs and administrative overhead of external review be minimized? How can we select external reviewers that provide an objective evaluation, rather than act as champions or advocates of a particular technology? And finally, can we provide better guidance to external review panels that will enhance the value of their evaluations? Preliminary thoughts about the major issues associated with the use of external review in the LDRD program are outlined in Table 4.

Table 3. Potential Tiered Structure for the LDRD Program			
	Tier 1 Projects	Tier 2 Projects	Tier 3 Projects
Area	Any new concept	Concept related to existing programs or a new endorsed area	Major new program development
Funding	$25 – 100K	$100 - 500K	$500K - 3000K
Review	Review by neutral knowledgeable party (e.g., Sr. Sci. group)	Review by appropriate area investment team for programmatic considerations	Review by external review group for technical justification as well as internal review for programmatic issues
Start time	Any time	Beginning of FY	Beginning of FY
Minimum reporting requirement (peer reviewed)	Internal Memo	SAND Report	Peer reviewed article
Duration	Up to 24 months	Up to 36 months	Up to 48 months
Number	~ 200	~ 50	~ 20
Distribution	200@$75K	50@$300K	20@$2000K
Total $	~$15,000K	~$15,000K	~$40,000K

Table 4. Issues Concerning External Review
1. Role of External Review Review of LDRD proposals for an entire investment area? Review of Grand Challenge proposals? Review of all proposals that lie outside Sandia's recognized strengths? Review of performance and accomplishments in addition to proposals?
2. Costs and Administrative Issues To control costs should external review required solely for Grand Challenge proposals and performance? Can non-disclosure and non-competition agreements effectively protect against loss of intellectual property?
3. Selection of External Reviewers How to assure their objectivity rather than their advocacy? Who should select the reviewers? Should they be paid?
4. Mandate of External Review Can the review process be structured to provide more useful information, i.e., rank ordering proposals? Explicit discussion of the external state-of-the-art?

Comparison of Areas Supported by the LDRD Programs at Sandia in FY2002 and FY1999

FY 2002 total program = $81,000,000			FY 1999 total program = $80,000,000	
Area	**%**		**Area**	**%**
Computational and Information Sciences	4.8		Computer Sciences	10.5
Electronics and Photonics	10.1		Electronics and Photonics	13.8
Engineering Sciences	4.9		Engineering Sciences	7.7
Materials Science and Technology	9.4		Materials Science and Technology	10.4
Advanced Manufacturing	4.7		Manufacturing and Process Sciences	6.3
Pulsed Power	1.7		Sensing and Intelligent Controls	6.9
MESA Technologies	1.4		Surety Science	6.1
Biotechnologies	2.8		Information Systems and Technology	7.7
S&T/NW Strategic Objectives	4.9		Directed Energy	1.1
Emerging Threats	12.0		Environmental Sciences	2.9
Energy and Critical Infrastructure	12.0		Corporate Objectives	7.4
Nonproliferation and Materials Control	12.0			
Differentiating Technologies	1.2			
Grand Challenges	11.6		Grand Challenges	16.8
Advanced Concepts Group	1.9			
University Collaborations	1.5		University Collaborations	0.9
Special projects	0.7			
PECASE	0.4			
LDRD Program Management	1.9		LDRD Program Management	1.5

Concluding Comments

The LDRD program at the DOE national laboratories is essential to ensuring that the United States continues as a world leader in high technology and scientific research. At Sandia, it is essential to assuring our continued technical vitality and to our ability to attract and retain the highest quality researchers. The Phase 1 review of the LDRD program has identified areas in where changes could enhance the ability of the program to achieve its promise. In Phase 2 we will continue to explore these issues and develop ideas to address them.

Distribution

1	MS0351	Steven Rottler, 1000
1	MS0351	Julia Phillips, 1900
1	MS0110	Wendy Cieslak, 1200
1	MS0351	Dennis Croessman, 1910
1	MS0351	Hank Westrich, 1911
1	MS9001	Rick Stulen, 8000
1	MS9154	William Ballard, 8200
2	MS0899	Technical Library, 9536

Sandia National Laboratories