Accelerating Discovery

A Strategic Plan for Accelerator R&D in the U.S.

Report of the Accelerator Research and Development Subpanel

April 2015

Executive Summary

The dramatic success of beam-based particle physics research has been the result of the development of ever-more powerful accelerators over the past seventy-five years. During this period, accelerator science and technology has contributed to research that led to twenty-five Nobel Prizes in Physics, most recently for the theory leading to the Higgs boson preceding its discovery at the Large Hadron Collider (LHC) in 2012. Accelerator research and development provided the breakthroughs throughout the years that have made these successes possible. Renewed investment is essential to developing the future accelerators that will provide the field of particle physics with opportunities for new discoveries, as well as to maintaining U.S. world leadership in accelerator research and development.

The recent Particle Physics Project Prioritization Panel (P5)[1] recommended a number of future accelerator facilities in its strategic plan for U.S. particle physics. Each of these Projects has benefited from investments in U.S. generic accelerator R&D. The LHC, P5's highest-priority near-term large project, is about to begin operations at 13 TeV, or nearly twice its operating energy employed during previous runs. The high luminosity upgrade of the LHC (HL-LHC), which is expected to begin operations in 2025, will further increase the potential for new physics discoveries. The productive operating period for discoveries from the HL-LHC will extend to 2035. In the U.S., particle physics research facilities are focusing on long-baseline neutrino oscillations using upgrades of the Fermilab proton complex for higher beam power. Recent proton improvements are now providing neutrino beams of increased intensity to experiments in Minnesota. Further improvement with the Proton Improvement Plan II (PIP-II) will supply one megawatt (MW) beam power for the start of the Long-Baseline Neutrino Facility (LBNF) operations in the 2025–2030 timeframe. P5 identified LBNF as the highest-priority large project in its timeframe. The improvements will provide unprecedented neutrino beam fluxes to detectors located in the Sanford Underground Research Facility (SURF) in South Dakota for the study of the neutrino sector in detail. In Japan there are plans to construct the 500 GeV International Linear Collider (ILC) e^+e^- collider to provide complementary physics capabilities to the HL-LHC and to start operations in the 2030 timeframe. P5 also identified future-generation accelerators that are likely to be demanded when results from the current generation of experiments are known.

The Subpanel examined the accelerator R&D that is required to prepare for the future-generation accelerators envisioned by P5. Short-term R&D to optimize planned new facilities, HL-LHC, PIP-II, and ILC, was excluded from our analysis. The Subpanel examined medium-term R&D to bring new concepts to practice so that they can be considered for the design of new facilities: *e.g.* a very high-energy proton-proton collider to explore particle physics beyond the reach of the HL-LHC program; a multi-MW proton beam and target system for next-generation neutrino experiments; and an energy upgrade of the ILC to an energy of approximately 1 TeV for precision experiments in the ½ TeV to 1 TeV range. The Subpanel refers to these facilities as the *Next Steps*. The Subpanel also examined long-term R&D of exploratory nature aimed at developing new concepts for what the Subpanel referred to as *Further Future* accelerators identified by P5, a multi-TeV e^+e^- collider for energy frontier research complementary to research at hadron colliders, and a neutrino factory for the further study of the neutrino sector. These facilities will ultimately be constructed if the R&D is successful, and if demanded by the future physics research program, informed by interim results.

The General Accelerator Research and Development (GARD) program in the Department of Energy Office of High Energy Physics (HEP) provides most of the funds for accelerator R&D in the U.S. and is currently funded at a level of $68 M for FY 2015. GARD supports medium- and long-term accelerator R&D, and the facilities to support that R&D. This past year the Physics Division of the National Science Foundation initiated a new program for accelerator science with an annual funding level of $10 M, which provides significant accelerator R&D support complementary to GARD. Near-term accelerator R&D is supported through DOE directed R&D projects. Examples of such projects include LARP (LHC Accelerator Research Program) for the U.S. contribution to the LHC upgrades and the PIP-II superconducting linear accelerator (linac) project for proton intensity improvements at Fermilab. The Subpanel assessed and analyzed the requisite evolution of the GARD program to provide the essential R&D for the Next Steps and Further Future accelerators. It also considered funding for fundamental accelerator research and for workforce development of accelerator scientists and engineers.

[1] The May 2014 report of the Particle Physics Project Prioritization Panel (P5) is available at:
http://science.energy.gov/~/media/hep/hepap/pdf/May%202014/FINAL_P5_Report_Interactive_060214.pdf

The Subpanel found GARD to be a highly productive program in accelerator R&D, and reasonably well aligned with both the general goals of the national accelerator R&D program, as defined in this report, and the strategic vision for accelerator-based particle physics, as defined in the P5 report. Nonetheless, better alignment could be achieved by rebalancing investment. For example, by increasing emphasis on accelerator R&D for proton beams with respect to R&D for electron beams, and by increasing emphasis on medium-term R&D with respect to long-term R&D. Specific investments in Accelerator Physics and Technology, in Particle Sources and Targetry, and in Superconducting Magnets and Materials are recommended that will serve to rebalance the GARD portfolio in proton beam research and in the medium-term R&D.

With the restricted budgets that particle physics faces, the accelerator research community must adopt common goals that are aligned with the field's strategic vision. The community should define and develop a coordinated, coherent R&D program that will achieve those goals. Collaboration and coordination with programs in the rest of the world are necessary. With this planned and coordinated approach, the facilities for future accelerator-based particle research can be available much sooner.

To guide the R&D programs for the Next Steps and Further Future accelerators in a cost-effective manner, the particle physics community should establish early in the accelerators' conceptual design an agreed-upon specification of the physics parameters for the research programs on these accelerators. Energy and luminosity, for instance, are key cost drivers for future colliders, for both construction and operating costs.

The first budget scenario analyzed by the Subpanel (referred to as Scenario A) assumed constant funding for the future GARD program at the FY 2015 funding level of $68 M. In this scenario, some areas of accelerator R&D must shrink to fund the areas that demand more support to carry out the R&D needed by P5 goals. As a high profile example, some funding for long-term R&D on advanced acceleration techniques for multi-TeV e^+e^- colliders must be redirected towards R&D for a very high-energy proton-proton collider. When the FACET R&D facility stops operations at the end of 2016 because of the construction of the LCLS-II free electron laser at SLAC National Accelerator Laboratory, the operating budget of FACET would become available temporarily for the support of the high priority R&D items needed to realize the Next Step accelerators. The funding for the beam-driven plasma wakefield acceleration (PWFA) science program now at FACET could support continuing this research at other facilities; however, funding for a needed follow-on R&D facility for PWFA research is not available in the GARD base budget without severe dislocation of the other priority programs in the GARD portfolio. Although a follow-on R&D facility is needed to continue the full spectrum of PWFA research, it could only be funded in Scenario C described below.

There are opportunities for increased investment that would significantly advance the prospects for realizing the accelerators needed in the future. A modest increase, 10% to 20%, in the overall GARD budget (referred to as Scenario B) would open numerous critical R&D activities that do not fit in the current base. Among these are specific items in Accelerator Physics and Technology (supporting simulation), in Particle Sources and Targetry (radiation damage studies), RF Acceleration (higher gradients and efficient sources), Superconducting Magnets and Materials (development of dipoles using new materials), and Advanced Acceleration (opening the BELLA facility to outside users).

Beyond Scenario B, there are two major program areas that are key to hastening the development of a very high-energy proton-proton collider and of a multi-TeV e^+e^- collider. In a budget scenario referred to as Scenario C, R&D in each area could be supported via a sequence of targeted R&D projects with definite goals and finite lifetimes. With these additional funds, future accelerator facilities needed for scientific discovery could be realizable on a timescale that would continue to attract the brightest young researchers to particle physics and to accelerator physics research, and the U.S. accelerator R&D program would continue to be world leading.

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1: Introduction

Today's particle accelerators and storage rings have evolved from the early transformer rectifier devices, which reached ~100 keV beam energies in the 1930s, to the LHC, which will begin operations at a center of mass energy of 13 TeV in spring of 2015. In parallel, the field has been propelled by an enormous increase in beam current, where next-generation high-current facilities will accelerate grams of matter to near the speed of light. This dramatic increase in beam energy and power has come about through continuous devotion of some fraction of the particle physics research budget to accelerator research and development. Opportunities for significant new increases in energy and power exist today, notably in the key areas of superconducting magnet and superconducting radio frequency (SRF) development. New cavity configurations and efficient energy recovery for high frequency normal conducting RF acceleration systems may also provide a path to higher energy future e^+e^- linear colliders. While at a very early stage of development, advanced accelerators such as plasma and dielectric wakefield acceleration systems may offer the increased energy range in much more compact systems. It is with this background that our Subpanel has worked to develop this report as a guide to most effectively use the scarce resources available in the current funding climate.

During late 2013, the Committee of Visitors for the Office of High Energy Physics recommended an evaluation of the HEP General Accelerator Research and Development (GARD) program, in order to identify and prioritize components that are central to the evolving HEP mission. The recent Particle Physics Project Prioritization Panel (P5) report[2] also endorsed forming a HEPAP subpanel to look at the GARD program portfolio with an emphasis on assuring alignment with the P5 priorities for the particle physics program in the U.S. over the next decade and beyond.

The present HEPAP Accelerator R&D Subpanel was formed in early summer 2014 and held its first face-to-face meeting at SLAC National Accelerator Laboratory on July 6 and 7, 2014. The charge to the Subpanel is given in Appendix A, and the membership of the Subpanel is listed in Appendix B. A public website[3] was set up, and a request for input from the community in the form of white papers was sent out to the membership of both the Division of Physics of Beams and the Division of Particles and Fields of the American Physical Society. During the last week of August 2014, a week-long road trip was undertaken by the members of the Subpanel along with members of the Office of High Energy Physics. Brookhaven National Laboratory (BNL) was the first stop, and high-energy collider options were the main topics discussed. Accelerator R&D in other regions of the world were also summarized. The next stop was at Fermi National Accelerator Laboratory (Fermilab) with a side trip to Argonne National Laboratory (ANL) that evening to tour their facilities. The emphasis in this stop was on the Intensity Frontier. At SLAC and then Lawrence Berkeley National Laboratory (LBNL) the following day, the focus moved to advanced acceleration technologies. Town Hall sessions were held at BNL, Fermilab, and SLAC to gather additional community input. Agendas for the road trip meetings are available in Appendix C.

Consideration of the HL-LHC, the ILC, and PIP-II was not included in the charge to the Subpanel, and they were not part of the Subpanel's analyses.

A second face-to-face meeting was held in conjunction with the U.S. Particle Accelerator School at Newport Beach, CA on November 6 and 7, 2014. The third face-to-face meeting took place in Chicago on December 2 and 3, 2014. The foci of these two meetings were on examining the budget of the current program, along with possible future needs and a first discussion of the drivers for the possible medium-term program in particle physics. There was considerable discussion of a possible very high-energy proton-proton collider and its implications for a U.S. program in superconducting magnet technology. The dipole magnets for such a machine are its main cost driver, and offer the biggest cost saving potential in the construction of the collider.

The fourth face-to-face meeting at the University of California, Los Angeles (UCLA), on January 9 and 10, 2015, began with an extended discussion of possible budget scenarios and the balance among the areas supported by the GARD program. It became clear that the current balance would need to be changed to increase support in the priority R&D areas for the next-generation accelerators put forward in the P5 report, namely superconducting magnet research along with superconducting RF and areas related to the Intensity Frontier. It also became clear that a short-term increase in funding would be necessary to construct a new R&D facility essential for beam-driven plasma wakefield

[2] The May 2014 report of the Particle Physics Project Prioritization Panel (P5) is available at: http://science.energy.gov/~/media/hep/hepap/pdf/May%202014/FINAL_P5_Report_Interactive_060214.pdf

[3] The HEPAP Accelerator R&D Subpanel website is available at: http://www.usparticlephysics.org/p5/ards

acceleration research when the current FACET facility at SLAC ceases operations at the end of 2016. This investment would also be needed in order to maintain the world-leading position of the U.S. in advanced acceleration research.

A fifth face-to-face meeting was held at SLAC from February 27 to March 1, 2015, to finalize the report and its recommendations before its presentation at the HEPAP meeting on April 6 and 7, 2015.

In addition to the face-to-face meetings, many subgroup meetings were held by teleconference and email. After the second face-to-face meeting, a teleconference of the entire Subpanel was initiated on a weekly basis, which changed to twice a week during the last two months as the report was being put together. The entire process was very much a team effort with every member contributing. Without this, we would not have been able to produce the report that you see here.

The remainder of this section of the report introduces the field's vision of future accelerator needs and the goals of the national accelerator R&D program, and it provides the Subpanel's assessment of the current General Accelerator R&D program. Sections 2-4 summarize the Subpanel's specific recommendations for three budget scenarios. Scenario A assumed constant funding for the future GARD program at the FY 2015 funding level. Scenario B assumed a modest budget increase (10% to 20%). Scenario C, which responds to the Subpanel's charge, identifies investment opportunities for transformative progress. The remainder of the report discusses each of the five accelerator R&D thrusts considered by the Subpanel. The comments and recommendations represent the consensus view of the Subpanel.

1.1: The Vision for Future Accelerators and for Accelerator R&D in the P5 Strategic Plan

The P5 report presented a prioritized list of future-generation accelerators based on the current understanding of their potential to address the science drivers[4] and enable discovery. It concluded that upgrading the Fermilab proton beam to the multi-MW level along with associated improvements in targets would produce the needed increase in neutrino flux to further exploit the planned Long-Baseline Neutrino Facility (LBNF). The most powerful future tool for direct discovery of new particles and interactions within the time window considered by P5 is a very high-energy proton-proton collider. Such a collider would be capable of directly producing new particles at mass scales approaching 10 TeV/c^2 to 15 TeV/c^2, further push the frontier of direct dark matter production, and provide an enhanced means to leverage the Higgs boson as a tool for discovery. An energy upgrade for the ILC to the 1 TeV regime would enable more detailed studies of the Higgs boson along with potential discoveries of physics beyond the standard model. A multi-TeV e^+e^- collider follows in priority, complementing a very high-energy proton-proton collider with the capability to increase measurement precision and further extend discovery for those same science drivers. Finally, a neutrino factory based on a muon storage ring would provide the capability to achieve a more precise and complete understanding of neutrino physics beyond the planned LBNF. Physics results in the coming years will provide further guidance regarding the scientific promise of each of these possible future accelerators.

1.2: National Goals of HEP Program of Accelerator Science and Technology R&D

General Goals and Characteristics

The program of accelerator science and technology R&D of the Office of High Energy Physics should have the following general goals and characteristics:

- *Program balance:* Enable discovery science on all future time scales with:
    - *Short-term R&D:* Develop via directed R&D the techniques and technologies required for optimization of operating accelerators or approved new accelerators that will enable future discoveries in particle physics in the near term.
    - *Medium-term R&D:* Perform the accelerator R&D necessary to bring new concepts to practice in order that they can be considered for the design of a new accelerator that will enable the experimental capabilities for future discoveries in particle physics in the medium term.
    - *Long-term R&D:* Perform the exploratory accelerator research aimed at developing new concepts for acceleration and new technologies that will lead to the accelerators that will enable discoveries in particle physics in the long term.
- *Cost-effectiveness:* Long-term and medium-term R&D should yield scientific and technological breakthroughs that will enable future accelerators of higher intensity and higher energies at realizable costs.

[4] P5 identified five science drivers, compelling lines of inquiry that show great promise for discovery over the next ten to twenty years.

- *Coherence:* Define a coherent national R&D program that, in a coordinated fashion, draws upon the respective strengths of the various SC/HEP laboratories and of universities in order to achieve the complete goals of the program without duplication.

- *Alignment:* Define a national R&D program that targets future accelerators that are aligned with the strategic plan of particle physics (the "P5 plan").

- *International context:* Define the national program so as to complement and reinforce the accelerator R&D activities of international partners, thus to enable realization of future large international accelerator projects, be they hosted in the U.S. or in other regions of the world.

- *R&D facilities:* Develop and build the accelerator R&D facilities that are required to study, develop, test, and demonstrate fundamental accelerator science and technologies for future accelerator facilities. Redundancies in large facilities domestically and internationally should be avoided.

- *Workforce development:* Develop the workforce necessary to operate and maintain current accelerators, to develop and design the next generation of accelerators for particle physics, and to perform the accelerator technology R&D and fundamental accelerator science research required for the medium- and long-term.

- *Synergistic R&D:* To the extent allowed by funding, perform accelerator R&D and fundamental accelerator research that are synergistic between HEP and other sciences and that will lead to new research capabilities and discoveries.

1.3: Specific Goals in Alignment with the P5 Strategic Plan

Alignment with the current strategic plan of particle physics demands that the national program address accelerator technology R&D and accelerator science research targeting the following planned and foreseen future accelerator facilities, while sustaining the other goals and characteristics of the program:

- *Short-term:* Develop the following planned accelerators (short-term R&D directed at these accelerators is not included in the charge to this Subpanel):

  - *Proton Improvement Plan II (PIP-II)*
    for research at the Intensity Frontier, particularly to pursue the physics associated with neutrino mass, and also to explore the unknown (new particles, interactions, and physical principles).

  - *High-Luminosity Large Hadron Collider (HL-LHC)*
    for research at the Energy Frontier with colliding proton beams, in order to use the Higgs boson as a new tool for discovery, to identify the new physics of dark matter, as well as to explore the unknown (new particles, interactions, and physical principles).

  - *International Linear Collider (ILC)*
    for research at the Energy Frontier with colliding electron-positron beams complementary to the colliding proton beams of the HL-LHC, in order to use the Higgs boson as a new tool for discovery, to identify the new physics of dark matter, as well as to explore the unknown (new particles, interactions, and physical principles).

- *Medium-term:* Perform the accelerator R&D required to bring new concepts to practice for the following medium-term accelerators of the next-generation ("Next Step") of each of the three complementary types of accelerator facilities:

  - *Multi-MW proton accelerator*
    the Next Step accelerator for research at the Intensity Frontier, including neutrino physics.

  - *Very high-energy proton-proton collider*
    the Next Step accelerator for research at the Energy Frontier with colliding proton beams.

  - *TeV-scale ILC upgrade*
    the possible Next Step accelerator for research at the Energy Frontier with colliding electron-positron beams.

- *Long-term:* Perform the exploratory research aimed at developing new concepts that will make possible the complementary accelerator facilities of possible science interest after the Next Step ("Further Future"):

  - *Multi-TeV e^+e^- collider*
    the Further Future accelerator for research at the Energy Frontier with colliding electron-positron beams.

  - *Neutrino factory*
    the Further Future accelerator for Intensity Frontier neutrino physics research.

The scientific motivation for the above medium-term and long-term accelerator facility targets was discussed more in Section 1.1 on the P5 vision for future accelerators and for accelerator R&D. The challenges with respect to accelerator science and technology are discussed in Section 1.4 below.

1.4: Scientific and Technological Challenges of Future Accelerators Identified in the Strategic Plan

The physics parameters for the research programs for future accelerators shape the scientific and technical challenges facing their design. The required luminosity, as well as the energy, for both proton-proton and e^+e^- colliders are key cost drivers. The size and operating energy of the accelerators determine not only their initial construction cost but also directly influence their operating cost and their reliability. The particle physics community must come together and agree on the physics parameters in order to define the challenges and to guide the accelerator R&D necessary to realize the Next Steps and Further Future accelerators

Realizing a multi-MW proton source to provide neutrino beam intensities at Fermilab beyond those of the PIP-II project will require significant further R&D on targets and focusing systems, concentrated on tolerance of materials to radiation effects of intense beams. (See Section 6.) Studies of space charge effects are also important, because space charge effects at injection energies of the chain of synchrotrons can limit the ultimate beam intensity. (See Section 5.)

The greatest challenges of a very high-energy proton-proton collider are the performance and cost of its dipole bending magnets. Superconducting magnet and materials R&D, with an emphasis on conductor development along with simplified magnet manufacturing technology, is key to making such a collider realizable. (See Section 8.) Accelerator physics studies will be needed to optimize the collider design, including experiments with beams that could have a significant impact on the design of the injector complex. R&D will also be needed on a variety of accelerator technology issues as the conceptual design develops, including collimation and beam abort, cryogenic, and vacuum systems. (See Section 5.)

The greatest challenges to the design of a high-energy e^+e^- collider based on RF acceleration are high accelerating gradient and low power consumption. R&D is needed to increase RF accelerating gradient and to develop more efficient RF sources that incorporate energy recovery. (See Section 7.)

R&D on advanced acceleration techniques has made considerable, eye-catching, progress in recent years, yet the challenges facing these techniques to accelerate beams of sufficient quality for eventual use in a high-energy e^+e^- collider are very large. The open technical issues essential for collider applications include simultaneously meeting requirements for beam stability and control, narrow beam energy spread, emittance and brightness preservation, repetition rate, efficiency, and reliability as well as the ability to accelerate positrons. (See Section 9.)

1.5: Overview of the Current GARD Program

DOE supports medium-term and long-term accelerator R&D in the U.S. through their General Accelerator R&D (GARD) program ($68 M in FY 2015) in the Office of High Energy Physics, and through the smaller new Accelerator R&D Stewardship program ($10 M in FY 2015) which is also managed by the Office of High Energy Physics. DOE supports short-term accelerator R&D through "directed R&D" projects, which currently include the LARP program for the HL-LHC final focus components and crab RF cavities, the PIP-II linac at Fermilab, the ILC, and until recently the Muon Accelerator Program (MAP). In FY 2014, the National Science Foundation initiated a new program in Basic Accelerator Science in its Division of Physics, which provides a nice complement to the research activities of the DOE GARD program. The total funding for this new program is $10 M in FY 2015 distributed over 13 awards. This new program is a very welcome addition to the support of accelerator R&D in the U.S.

Accelerator R&D supported by the GARD program is categorized in seven thrusts. These thrusts[5] are listed in **Figure 1**. They were the basis of our budget discussions when assessing changes that may need to be made to achieve the P5 goals. The activities within each GARD thrust are described in subsequent sections of this report. Also included in the figure are the operating expenses that support the SRF, Superconducting Magnet, and Advanced Acceleration thrusts.

Accelerator R&D facility operating costs total $28.6 M, accounting for 42% of the total GARD program budget of $68 M. The budget remaining for accelerator research is $39.4 M, 58% of the total. The two largest components of the research budget are New Acceleration Concepts and Superconducting Magnets and Materials, which together account for 50%.

For purposes of this report, the Subpanel grouped certain GARD thrust areas together. Accelerator and Beam Physics

[5] Also included in the chart are the operating expenses in support of the SRF, Superconducting Magnet, and Advanced Acceleration thrusts.

and Beam Instrumentation and Control thrusts were combined into Accelerator Physics and Technology. Particle Sources was expanded to include Targetry. The Superconducting and Normal Conducting RF thrusts were combined into a single RF Acceleration area. New Acceleration Concepts was renamed as Advanced Acceleration. The allocation of funding corresponding to the Subpanel's grouping is displayed in **Figure 2**, again including facility operations costs.

1.6: Assessment of the Current GARD Program

This subsection presents a summary assessment of the current GARD program as a whole. In the sections for each of the thrust groupings later in the report, specific assessments of individual R&D thrusts and groupings are given, as well as specific discussion of alignment of R&D thrusts with the field's strategic plan as outlined in the P5 report.

The Subpanel found that, overall, the activities of the GARD

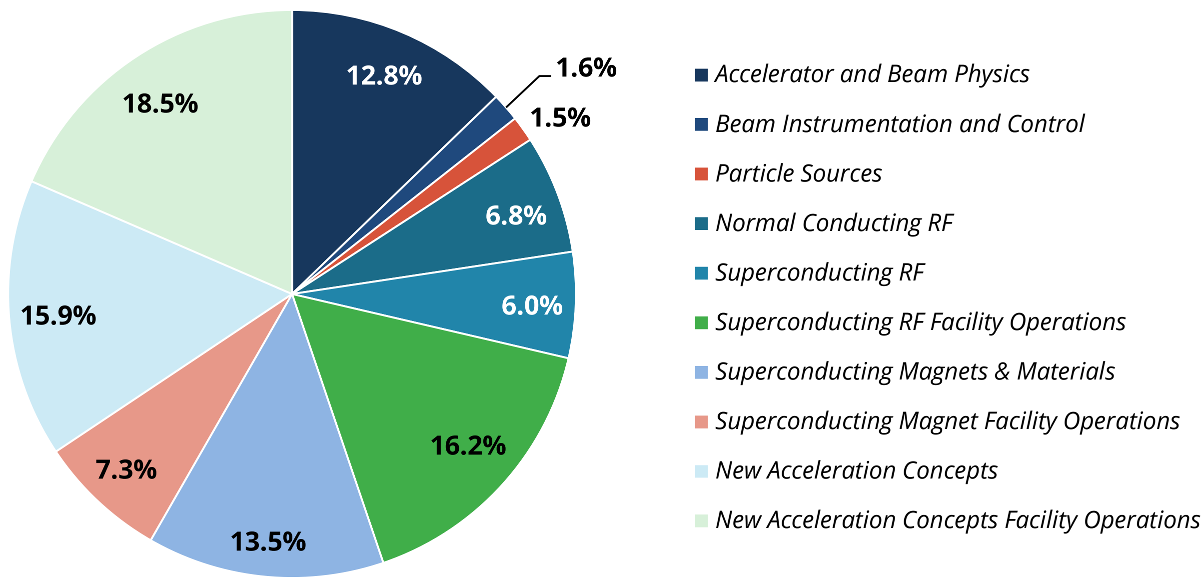

Figure 1: Accelerator R&D thrusts supported by the GARD program.

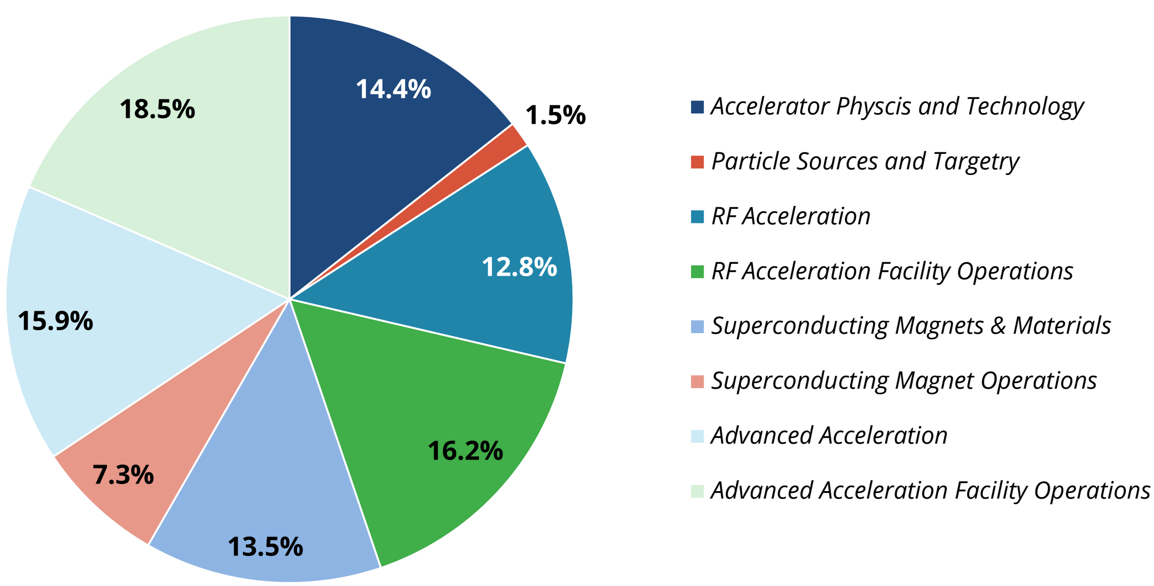

Figure 2: Accelerator R&D thrusts within the GARD program grouped as considered by the HEPAP Accelerator R&D Subpanel.

program are reasonably well aligned with the general goals outlined in Section 1.2 and with the strategic vision of the field. Neither duplication of effort nor R&D that was not focused on the general goals was identified. The GARD program can however be brought into better alignment with the program goals and vision. For instance, the Subpanel found that the GARD program is under-invested in R&D for future accelerators based on proton beams relative to the investment in R&D for accelerators based on electron beams, particularly considering the emphasis placed by P5 on the importance of high-intensity proton beams and of a very high-energy proton-proton collider in the strategic plan. Similarly, the Subpanel found that the GARD program is somewhat under-invested in medium-term R&D relative to long-term R&D, due in large part to investments made in advanced acceleration techniques in recent years, given that these techniques are targeted at electron-positron colliders of potential interest in the further future. Specific increases in investment in Accelerator Physics and Technology, in Particle Sources and Targetry, and in Superconducting Magnets and Materials outlined in this report will rebalance the GARD portfolio with appropriate weighting of R&D on proton beams and on the medium term priorities.

The current GARD program does not properly reflect several of the general goals that should characterize the program. In particular, the current program of activities has emphasized curiosity-driven rather than goal-driven research and development. A well-balanced GARD program should have aspects of both. To focus further on goal-driven activities within the R&D thrusts, the accelerator research community should adopt common goals, which should be aligned with the strategic vision of the field, and define a coordinated, coherent program for that thrust designed to achieve those goals. These R&D thrust areas should be complementary to, and performed in collaboration and coordination with, programs elsewhere in the world with the same thrust. Such collaboration and coordination will bring the accelerator facilities that are needed for future discoveries to the particle physics community sooner. Development of these goals and plans, at national and international levels, will be best accomplished as a community-driven, as opposed to a DOE-defined, process. The DOE should encourage and foster this process.

Cost-effectiveness considerations do not currently enter into the definition of R&D activities at a sufficiently early stage of the R&D process. R&D that provides higher performance at lower cost should be more heavily emphasized in the definition of the R&D programs.

The GARD program currently has R&D facilities that adequately support its R&D program. These facilities, which were largely funded by the Recovery Act, include the superconducting magnet test facilities at LBNL, BNL, and Fermilab, the SRF test facilities at Fermilab, the BELLA facility at LBNL, and the FACET test facility at SLAC. To experimentally test the effectiveness of integrable non-linear lattices that hold the promise of significantly limiting the effects of space charge in low energy beams, the IOTA ring at Fermilab should be completed and operated to carry out these studies. Because of the substantial investment that is necessary to construct and operate dedicated R&D facilities, without increased investment the GARD program will be financially challenged to provide facilities capable of sustaining productive and timely R&D. As one example, the Subpanel has identified the need for a follow-on facility to FACET, which will close at the end of 2016 due to LCLS-II construction. This facility is needed to further the promising research in beam-driven plasma wakefield acceleration in a timely fashion. Nonetheless, the Subpanel has concluded that investment in such a follow-on facility is not possible within the current GARD funding envelope given other R&D priorities.

The GARD program provides workforce development both through training gained in R&D activities at laboratories and universities and via the critical role played by the U.S. Particle Accelerator School (USPAS), which is funded in part by GARD.

The current GARD budget supports a lively, productive program in accelerator R&D; however, the current GARD budget is insufficient to support a balanced program of critical R&D in all thrust areas or on all time scales or that addresses at an adequate rate of progress the strategic vision of the P5 report. Consequently, the current GARD budget cannot afford to support R&D that is not of direct benefit to HEP goals, although it can continue to support R&D that is dual-purpose or strongly synergistic with HEP. The Subpanel assumed that the budget will remain at its current level in our analysis to set the R&D priorities for the future program (defined as Scenario A).

There are opportunities for increased investment that would significantly advance the prospects for realizing needed new accelerators. A modest increase, 10% to 20%, in the overall GARD budget (defined as Scenario B) would open numerous critical R&D areas that do not fit in the base funding. There are specific items in Accelerator Physics and Technology (supporting simulation), in Particle Sources and Targetry (radiation damage studies), RF Acceleration (higher gradients and efficient sources), Superconducting Magnets and Materials (development of dipoles using new materials), and Advanced Acceleration (opening the BELLA facility to outside users) that would speed up progress to the needed accelerators.

Key to developing a credible path to a very high-energy proton-proton collider and to a multi-TeV e^+e^- collider are two larger program needs. Funding for these would be broken into two initiatives in a budget scenario (referred to as Scenario C) that can fund well-defined R&D projects. The initiatives would be targeted with definite goals and finite lifetimes. With these additional funds, the GARD program would continue to be world leading and the needed new accelerator facilities would be realizable on a timescale that would continue to attract the brightest young researchers to particle physics research.

1.7: Impediments

U.S. accelerator R&D efforts are hindered by a variety of impediments relating to, for instance, workforce, curiosity-driven research, interdisciplinary research, and test facilities. Some impediments are discussed in the following paragraphs.

Inadequate workforce development and training: Workforce development and training in accelerator science and technology is a major concern for the long-term health of accelerator-based particle physics research. The 2014 HEPAP Subcommittee on Workforce Development reported a severe shortage of accelerator scientists and technologists. Quoting from the report: [6]

> *The shortage of accelerator scientists is apparent at the national labs. FNAL reports that job openings in aspects of accelerator science typically attract two to three applicants, and most of these are foreign. At BNL, 16 searches for accelerator physicists in the last three years turned up fewer than ten qualified applicants. As at FNAL, most of these were foreign, from Europe, Russia, India, China and elsewhere. Historically, the demand for accelerator scientists has been filled by particle physicists who transitioned to accelerator science in order to further their research; however, this pool has diminished as accelerators have moved off campus, and more recently, overseas.*

Only a dozen or so U.S. universities have academic programs in accelerator science, and recently two of those no longer have programs due to the closure of accelerator facilities associated with their campuses. Roughly fifteen to twenty Ph.D. degrees are awarded in accelerator science each year in the U.S. The Subcommittee report identified the U.S. Particle Accelerator School (USPAS), an initiative of the U.S. accelerator laboratories supported in part by the HEP GARD program, as a critical resource in the training of accelerator scientists and technologists. In addition to augmenting academic programs and providing a segue for particle physicists transitioning to accelerator science and technology, USPAS plays a very important role in enabling engineers and scientists to remain current as accelerator science advances.

Risk to curiosity-driven basic research: Accelerator R&D supported by the Office of High Energy Physics in the past spawned accelerators that have been extremely useful in other fields of basic research. Current funding levels for the GARD program leave very little room for support of curiosity-driven basic research in accelerator science, putting at risk finding the breakthroughs in accelerator science that will lead, for example, to new accelerator capabilities.

Lack of interdisciplinary ties: Although accelerator science and technology is by nature multidisciplinary, establishment of closer ties between accelerator researchers in R&D programs at laboratories with university researchers in material science areas, such as superconducting materials and radiation damage in materials, offers significant potential.

Restrictive use of test facilities: Much accelerator R&D requires use of large laboratory test facilities, such as the facilities needed for superconducting magnet and superconducting RF development and those needed for wakefield acceleration research. Opening these facilities for wider use, for instance by university researchers, could allow more efficient utilization of existing facilities and have significant benefit for the accelerator R&D community.

[6] The June 2014 report of the HEPAP Subcommittee on Workforce Development is available at: http://science.energy.gov/~/media/hep/hepap/pdf/Reports/OHEP_Workforce_Letter_Report.pdf

2: Recommendations in Scenario A

To carry out the accelerator-based particle physics research program that was described in the P5 strategic plan, and outlined above, three different lines of accelerators are needed. On the Intensity Frontier, intense proton beams are needed to produce neutrinos and other types of secondary particles for neutrino science and for precision studies sensitive to new physics. On the Energy Frontier, hadron colliders provide the capability to pursue three of the five P5 science drivers: the Higgs as a tool for discovery; the new physics of dark matter; and exploring the unknown: new particles, interactions, and physical principles. High energy e^+e^- colliders provide complementary energy frontier capabilities to contribute to the study of the same three drivers.

On the Intensity Frontier, higher proton beam intensities along with improved targets and secondary beam focusing systems will be needed. The next step after the HL-LHC will be a very high-energy proton-proton collider, up to ~100 TeV, and it will need significant R&D in superconducting magnets and materials to demonstrate technical feasibility and to reduce costs. The ILC is the current effort in e^+e^- colliders and the next step here is an energy upgrade to 1 TeV. To make the upgrade cost effective, R&D on superconducting RF cavities will be needed to raise the current gradient of 31 MV/m to significantly higher levels through the use of new materials.

These accelerators are summarized in **Table 1**.

The greater demands placed on the performance of these accelerators, while at the same time reducing their costs significantly, gives rise to the challenges facing the GARD program.

2.1: Overview of Scenario A Recommendations

The current GARD budget (defined as Scenario A) supports a lively, productive program in accelerator R&D; however, it is insufficient to support a balanced program of critical R&D in all thrust areas on all relevant time scales, and that addresses at an adequate rate of progress the strategic vision of the P5 report. Consequently, the Subpanel has had to make choices regarding the priority of R&D investment. One example is that a successor to the very successful FACET facility at SLAC for research into particle-driven plasma wakefield acceleration cannot be accommodated in Scenario A. While some research in this area can move to the Accelerator Test Facility (ATF) at BNL, progress in this promising technique will be slowed and will eventually come to a virtual standstill until a next-generation PWFA research facility is constructed.

The choices for R&D investment in Scenario A are described in the following recommendations. The Subpanel checked that these recommendations are consistent with the current GARD budget by constructing model portfolios to fit within the budget. The Subpanel's recommendations are organized according to the future accelerator to which the recommended R&D applies.

2.2: "Next Step" Accelerator Facilities

2.2.1: Multi-MW Proton Beam

P5 identified the eventual need, post PIP-II, for a multi-MW

	Intensity Frontier Accelerators	Hadron Colliders	e^+e^- Colliders
Current Efforts	PIP	LHC	
	PIP-II	HL-LHC	ILC
Next Steps	Multi-MW proton beam	Very high-energy proton-proton collider	1 TeV class energy upgrade of ILC *
Further Future Goals	Neutrino factory *	Higher-energy upgrade	Multi-TeV collider *

Table 1: Particle accelerators foreseen by the P5 strategic plan to carry out future accelerator-based particle physics research. ***(* The priority and urgency of some accelerators depends upon how physics unfolds at current or Next Step accelerators.)***

proton beam in order to increase the neutrino flux for the long-baseline neutrino program at Fermilab. A multi-MW proton beam will require R&D on targets and focusing systems. It will also require a new accelerator to replace the aging Booster between the new PIP-II linac and the Main Injector. Both a superconducting linac and a rapid cycling synchrotron are candidates for the new booster, and R&D in the respective technologies is called for in order to optimize the selection.

For proton beam power beyond PIP-II, production targets and focusing systems for the secondary charged particles that decay, producing the neutrino beams, are particularly challenging. Components must be fabricated from materials that can withstand high radiation fields and thermal shocks, in addition to high temperatures, over long periods of exposure. R&D in the properties of various materials in the hostile environment of high-power beams, beyond that being performed in the context of existing projects, is necessary. Increased generic research is likely to improve the viability for running all future high-intensity neutrino programs by improving the reliability and efficacy of targets and focusing systems.

Recommendation 1. Fund generic high-power component R&D at a level necessary to carry out needed thermal shock studies and ionizing radiation damage studies on candidate materials that are not covered by project-directed research.

Space charge effects at injection energy currently limit the beam intensity of accelerator rings. A novel ring design paradigm based upon so-called integrable non-linear focusing lattices promises beam current limits that significantly exceed those of conventional lattices, by reducing space charge driven resonance effects. Higher beam currents would benefit the development of a multi-MW proton beam at Fermilab, particularly if a rapid-cycling synchrotron is implemented for this purpose. Beam experiments, which could be performed for proton beams by the proposed IOTA ring at Fermilab, are needed to study this paradigm in advance of a technology decision for the multi-MW proton beam in the 2020–2025 timeframe. Study of space charge driven effects will also benefit development of a very high-energy proton-proton collider.

Recommendation 2. Construct the IOTA ring, and conduct experimental studies of high-current beam dynamics in integrable non-linear focusing systems.

Simulations of the beam dynamics in the presence of strong space charge and of integrable nonlinear focusing lattices are also needed to complement the experimental studies.

Recommendation 3. Support a collaborative framework among laboratories and universities that assures sufficient support in beam simulations and in beam instrumentation to address beam and particle stability including strong space charge forces.

A linac based upon superconducting radio frequency (SRF) cavities is an alternative to a rapid-cycling synchrotron for a multi-MW proton beam. Advances in SRF R&D, particularly improvements in accelerating gradient, could benefit the multi-MW proton facility. Advances in both SRF and the study of integrable nonlinear systems should be fostered in time to inform the selection in the 2020–2025 timeframe of the acceleration technology for a multi-MW proton beam. The same SRF advances would also benefit progress toward a ~1 TeV upgrade of the ILC. These considerations motivate increased funding for SRF R&D.

Recommendation 4. Direct appropriate investment in superconducting RF R&D in order to inform the selection of the acceleration technology for the multi-MW proton beam at Fermilab.

2.2.2: Very High-Energy Proton-Proton Collider

P5 identified the scientific promise of a very high-energy proton-proton collider. Such a hadron collider would provide the accelerator-based opportunities in the era to follow the LHC and its upgrade, the High-Luminosity LHC (HL-LHC). P5 cited a very high-energy proton-proton collider as "the most powerful future tool for direct discovery of new particles and interactions under any scenario of physics results that can be acquired during the P5 time window."

Realization of a very high-energy proton-proton collider, from R&D through construction, will be, by necessity, a worldwide endeavor due to its scale. Conceptual design studies have recently been initiated at CERN and in China, and have been performed in the U.S. in the past[7]. Superconducting magnets are an essential enabling technology and a primary cost-driver for such a collider. Superconducting dipole magnet performance requirements are demanding, and represent a long lead-time technical challenge requiring many years of R&D. Moreover, breakthroughs are required in the cost-performance of superconducting magnet technology.

Recognizing the scientific importance of this future accelerator, and of the technical challenges of developing its su-

[7] "Design study for a staged very large hadron collider," VLHC Design Study Group Collaboration (Ambrosio, Giorgio *et al.*), SLAC-R-0591, FERMILAB-TM-2149.

perconducting magnets, P5 recommended, "Participate in global conceptual design studies and critical path R&D for future very high-energy proton-proton colliders. Continue to play a leadership role in superconducting magnet technology focused on the dual goals of increasing performance and decreasing costs." This perspective is reflected in the following Subpanel recommendation and subsequent more specific recommendations:

Recommendation 5. Participate in international design studies for a very high-energy proton-proton collider in order to realize this Next Step in hadron collider facilities for exploration of the Energy Frontier. Vigorously pursue major cost reductions by investing in magnet development and in the most promising superconducting materials, targeting potential breakthroughs in cost-performance.

Superconducting magnet R&D for a very high-energy proton-proton collider should be guided by accelerator design studies. While a large number of challenges must eventually be addressed by the design of the collider, accelerator design studies that inform the critical-path magnet R&D program should be given priority initially.

Recommendation 5a. Support accelerator design and simulation activities that guide and are informed by the superconducting magnet R&D program for a very high-energy proton-proton collider.

To maximize the progress towards realizing a very high-energy proton-proton collider, superconducting magnet R&D in the U.S. should be coherent, should be coordinated with international partners, and should be focused on simultaneous improvement of technical performance and significant reduction in cost. A figure-of-merit is the magnet cost as a function of the product of magnetic field B and dipole length L.

Recommendation 5b. Form a focused U.S. high-field magnet R&D collaboration that is coordinated with global design studies for a very high-energy proton-proton collider. The over-arching goal is a large improvement in cost-performance.

The most promising superconducting material currently known for the next generation of high-field magnets is Nb_3Sn. Needed developments, with targets, include: reducing the cost of Nb_3Sn to the same cost per kilogram as NbTi; achieving more than a factor of two in field for the equivalent amount of conductor, *e.g.*, by taking advantage of conductor grading, which is particularly effective for high fields; and finally, increasing the critical current density of Nb_3Sn by 30% relative to present Nb_3Sn R&D conductor.

Recommendation 5c. Aggressively pursue the development of Nb_3Sn magnets suitable for use in a very high-energy proton-proton collider.

High Temperature Superconductors (HTS) are needed for magnetic fields above 16 T. Substantial improvement in HTS materials (*e.g.* ReBCO and Bi-2212) has been achieved. However, these materials are still in the early stages of development, and many technical challenges remain. Presently foreseeable costs are prohibitive for use in future colliders except in limited applications.

Recommendation 5d. Establish and execute a high-temperature superconducting (HTS) material and magnet development plan with appropriate milestones to demonstrate the feasibility of cost-effective accelerator magnets using HTS.

Significant reductions in touch labor and material costs of next-generation superconducting magnets, as well as improved magnet reliability and ease of operation, are essential R&D goals. The high-field magnet program may benefit from engaging industry and advanced degree programs in engineering and manufacturing at research institutions in order to achieve optimized designs that can lead to significant cost reduction both in construction and operating costs.

Recommendation 5e. Engage industry and manufacturing engineering disciplines to explore techniques to both decrease the touch labor and increase the overall reliability of next-generation superconducting accelerator magnets.

The profound challenges of superconducting magnet development demand an adequately funded, well-coordinated national program. Given the importance of transformational improvements, a doubling of investment in magnet R&D is warranted.

Recommendation 5f. Significantly increase funding for superconducting accelerator magnet R&D in order to support aggressive development of new conductor and magnet technologies.

2.2.3: 1-TeV ILC Upgrade

An energy upgrade of the ILC, from its initial design value of 0.5 TeV center-of-mass energy to ~1 TeV, could be the Next

Step in e^+e^- colliders after construction of the ILC, if discoveries at the LHC/HL-LHC call for a 1-TeV scale e^+e^- collider.

Research and development of superconducting RF (SRF) acceleration technology could make accelerating gradients two to three times the ILC design value possible. New superconducting materials could enable gradients ~80 MV/m and lower power consumption; however, extensive R&D is necessary. Applied materials science should be directed at high-gradient SRF materials including alternate materials, thin films, and new processing techniques. Cavity geometry should also be optimized for new high-gradient structures.

Recommendation 6. Increase funding for development of superconducting RF (SRF) technology with the goal to significantly reduce the cost of a ~1 TeV energy upgrade of the ILC. Strive to achieve 80 MV/m accelerating gradients with new SRF materials on the 10-year timescale.

Development of SRF with higher gradients and lower power consumption requires investment beyond that directed to R&D associated with the LCLS-II and PIP-II projects. As previously discussed, these developments may also benefit new accelerators for high-intensity proton beams.

2.3: "Further Future" Accelerator Facilities

2.3.1: Multi-TeV e^+e^- Collider

Advanced techniques for wakefield acceleration offer the potential of dramatic reduction of the size and cost of future accelerators, and would revolutionize electron and positron acceleration for a multi-TeV e^+e^- collider. Techniques currently being investigated include: plasma wakefield acceleration driven by electron beams (particle-driven wakefield acceleration, or PWFA); plasma wakefield acceleration driven by lasers (laser-driven wakefield acceleration, LWFA); dielectric wakefield acceleration (DWFA) using an electron drive beam to create electromagnetic (Cerenkov) wakes in a dielectric structure; and direct laser acceleration (DLA) using optical scale dielectric structures to generate a longitudinal electric field driven by a laser. Development of these techniques has led to high-profile results that have captured the interest of the wider science community.

Presently, the premier R&D facility for studying particle-driven wakefield acceleration is FACET at SLAC. It is the only facility presently capable of studying positron acceleration, which is crucial for an e^+e^- collider. FACET, however, will close at the end of 2016 due to the construction of the LCLS-II facility. Consequently, it is important to study positron acceleration while FACET is still operating. After closure of FACET and prior to completion of a subsequent PWFA R&D facility, other facilities, such as ATF at BNL, can be used to continue PWFA research.

Recommendation 7. Vigorously pursue particle-driven plasma wakefield acceleration of positrons at FACET in the time remaining for the operation of the facility. Between the closing of FACET and the operation of a follow-on facility, preserve the momentum of particle-driven wakefield acceleration research using other facilities.

The Subpanel determined that construction of the proposed FACET-II, or of any successor to FACET, is not affordable in Scenario A.

Laser-driven plasma wakefield acceleration is also a promising advanced acceleration concept. The premier U.S. facility for LWFA R&D is the BELLA facility at LBNL, which has recently begun studies.

Recommendation 8. Continue to support laser-driven plasma wakefield acceleration experiments on BELLA at the current level.

The potential advantages of structure-based direct laser accelerators (DLA) have not been demonstrated. Opportunities for DLA testing with beam at the SLAC NLCTA will end in the near future; however, the ATF at BNL could provide beams with higher energy and better emittance, as well as a more accommodating laser wavelength. The Subpanel found that direct laser acceleration (DLA) is less likely than other techniques to be the technology of choice for e^+e^- colliders, and recommends reducing DLA funding.

Recommendation 9. Reduce funding for direct laser acceleration research activities.

Funding should be provided for viable options towards a multi-TeV e^+e^- collider; however, budget constraints demand that down-selection of advanced acceleration techniques be performed before extensive further investments are made. The down-selection process will need well-defined selection criteria related to suitability towards a multi-TeV e^+e^- collider, and will need to occur at an appropriate time in research and development of the techniques. For each advanced acceleration technique, and for normal conducting RF, an R&D roadmap should be established, with suitable milestones towards achieving required performance parameters.

Recommendation 10. Convene the university and laboratory proponents of advanced acceleration concepts to

develop R&D roadmaps with a series of milestones and common down-selection criteria towards the goal of constructing a multi-TeV e^+e^- collider.

Continued funding of advanced acceleration concepts from the Office of High Energy Physics should be conditional upon significant progress in achieving the experimental milestones appropriate to particle physics.

Recent developments in normal conducting radio frequency (NCRF) technology indicate that this may also be a candidate technology for a multi-TeV e^+e^- collider. Novel cavity geometries have produced impressive gains in accelerating gradients at room temperature. These investigations merit continued investment.

Recommendation 11. Continue research on high-efficiency power sources and high-gradient normal conducting RF structures.

The RF power and beam sources of the Next Linear Collider Test Accelerator (NLCTA) can be useful for testing NCRF structures.

Recommendation 12. Make NLCTA available for RF structure tests using its RF power and beam sources.

NCRF R&D activities should be consolidated and focused on the next major step in this approach, a multistage prototype accelerator.

Recommendation 13. Focus normal conducting RF R&D on developing a multistage prototype based on high-gradient normal conducting RF structures and high-efficiency RF power sources to demonstrate the effectiveness of the technology for a multi-TeV e^+e^- collider.

2.3.2: Neutrino Factory

Physics results from long-baseline neutrino oscillation studies using the multi-MW proton beam of the Next Steps could call in the further future for more neutrino oscillation studies using a beam from a neutrino factory based upon a muon storage ring, rather than a high-intensity proton "super-beam." The recommendation of P5 concerning the Muon Accelerator Program (MAP) and the Muon Ionization Cooling Experiment (MICE) led to the termination of MAP and scheduling of earlier than planned completion of MICE. P5 also recommended incorporating into the GARD program MAP activities that are of general importance to accelerator R&D. Under this guidance, fundamental aspects of muon beam dynamics R&D could be funded on a competitive basis against other activities of general interest.

2.4: Accelerator and Beam Physics – Support for Next Steps and Further Future Goals

Accelerator and beam physics, as well as beam instrumentation and control, are vital for the Next Steps and Further Future Goals, and should support the national priorities in a coordinated fashion.

Recommendation 14. Continue accelerator and beam physics activities and beam instrumentation and control R&D aimed at developing the accelerators defined in the Next Steps and the Further Future Goals. Develop coordination strategies, both nationally and internationally, to carry out these studies in an efficient manner.

A balanced accelerator R&D program must support fundamental accelerator physics research as well as encourage novel ideas beyond R&D directly related to the Next Steps and Further Future goals. Without new ideas entering the field, and without understanding limitations at a fundamental level, accelerator science will stagnate in the long run. University programs are well suited for this type of research, particularly given the NSF basic accelerator science program, with its emphasis on innovative accelerator science.

Recommendation 15. To ensure a healthy, broad program in accelerator research, allocate a fraction of the budget of the Accelerator Physics and Technology thrust to pursue fundamental accelerator research outside of the specific goals of the Next Steps and Further Future Goals. Research activities at universities should play a particularly important role.

The above recommendations can be accommodated within the current funding level of the GARD program (Scenario A). The goals and characteristics of the GARD program that led to this set of recommendations are presented in later sections. While the present GARD program funds world-leading R&D in many areas of accelerator science and technology, additional funding is needed to ensure the R&D breakthroughs necessary to realize Next Steps and Further Future facilities on the timescales that they will be called for by the particle physics research program. R&D opportunities that could be opened by a modest increase in GARD funding (defined as Scenario B) and that could yield new breakthroughs are discussed in Section 3. A roadmap for transformational accelerator R&D enabled by targeted increased funding is presented in Section 4 on Scenario C.

3: Scenario B

Whereas the current GARD budget (Scenario A) is insufficient to satisfy the expectations of P5, a modest rise in base funding for GARD research (defined as Scenario B: an increase of 10% to 20% of GARD research, 1% to 2% of HEP) would open numerous critical R&D opportunities that do not fit in the current base, as well as invigorate fundamental accelerator science research. Important opportunities are outlined below as examples.

The R&D of acceleration techniques, SRF, NCRF, and wakefield acceleration, could be enhanced in crucial ways with incremented Scenario B funding. Supplemental funding for SRF R&D would facilitate achieving the target of 80 MV/m acceleration gradients with new superconducting materials on timescales that might be used for multi-MW proton beams for neutrino science and that could be implemented for the ~1 TeV upgrade of the ILC.

Research and development of NCRF, with its broad applications, including use for a multi-GeV drive beam for PWFA, as a candidate acceleration technology for electron-positron colliders with energy greater than 1 TeV, and more efficient sources of RF for superconducting linacs, could be augmented. The research program in novel advanced acceleration techniques at dedicated R&D and user facilities could be expanded, improving the likelihood of a transformational breakthrough. As an example, the mission of the BELLA facility could be extended to give access to external users.

With Scenario B funding, an ambitious computational accelerator science program could be initiated to develop new algorithms, techniques, and generic simulation code with the goal of end-to-end simulations of complex accelerators that will guide the design, and improve the operations, of future accelerators of all types. Advancing the capabilities of accelerator simulation codes to capitalize on the drive toward exascale computing would have large benefits in improving accelerator design and performance. New computational algorithms coupled with the latest computer architectures are likely to reduce execution times for many classes of simulation code by several orders of magnitude, thereby making practical end-to-end simulations of complex accelerator systems. Such capabilities will enable cost-effective optimization of wakefield accelerators, as well as near-real-time simulations of large operational machines such as megawatt proton accelerators or a very high-energy proton-proton collider. In the near term, advanced simulation tools will maximize the productivity of R&D for all future accelerators.

With the base GARD budget, a healthy portfolio of fundamental accelerator science research is difficult to maintain along with crucial medium- and long-term R&D targeted at the future accelerators identified by P5. Supplemental funding for accelerator and beam physics could restore good balance between targeted R&D and fundamental research.

Supplemental funding would also allow investment in new university initiatives for the purpose of development of the national accelerator workforce. Initiatives could include incentives for new university programs or faculty positions in accelerator science, training programs for graduate students, and postdoctoral positions for scientists transitioning from experimental or theoretical particle physics to accelerator physics.

Recommendation B1. Increase base GARD funding modestly in order to open numerous critical R&D opportunities that do not fit in the current base, as well as to invigorate fundamental accelerator science research, and to step up development of the national accelerator workforce.

4: Scenario C

The P5 report, in its Scenario C, called for a roadmap for the U.S. to "move boldly toward development of transformational accelerator R&D ... with an aggressive, sustained, and imaginative R&D program ... changing the capability-cost curve of accelerators." Motivated by the P5 science drivers, the goal of increased investment in accelerator R&D is to "make these further-future accelerators technically and financially feasible on much shorter timescales." Accordingly, the Subpanel recommends, for P5's Scenario C, investment in medium-term R&D for a very high-energy proton-proton collider and in long-term R&D for a multi-TeV e^+e^- collider.

Recommendation C1. Hasten the realization of the accelerator of P5's medium-term vision for discovery: a very high-energy proton-proton collider, and the realization of the accelerator of P5's long-term vision for discovery: a multi-TeV e^+e^- collider.

The Subpanel envisions realizing these goals by supplementing the base accelerator R&D program of Scenario A or B with a sequence of R&D initiatives directed along the path to the accelerators of P5's vision. The Subpanel identified two urgent, high-priority accelerator R&D initiatives for immediate investment in order to propel particle physics forward. In order to hasten a very high-energy proton-proton collider, it is necessary to ramp up research and development of superconducting magnets, targeted primarily for a very high-energy proton-proton collider, to a level that permits a multi-faceted program to explore possible avenues of breakthrough in parallel. For the multi-TeV e^+e^- collider, the beam-driven plasma wakefield acceleration concept needs a follow-on R&D facility to FACET, while a number of candidate technologies, both RF-based and based on wakefield acceleration, are pursued to a technology down-selection.

Recommendation C1a. Ramp up research and development of superconducting magnets, targeted primarily for a very high-energy proton-proton collider, to a level that permits a multi-faceted program to explore possible avenues of breakthrough in parallel. Investigate additional magnet configurations, fabricate multi-meter prototypes, and explore low-cost manufacturing techniques and industrial scale-up of conductors. Increase support for high-temperature superconducting (HTS) materials and magnet development to demonstrate the viability of accelerator-quality HTS magnets for a very high-energy proton-proton collider.

Recommendation C1b. Develop, construct, and operate a next-generation facility for particle-driven plasma wakefield acceleration research and development, targeting a multi-TeV e^+e^- collider, in order to sustain this promising and synergistic line of research after the closure of the FACET facility.

The R&D path to the medium-term goal of a very high-energy proton-proton collider and the R&D path to the long-term goal of a multi-TeV e^+e^- collider are outlined in later sections on Superconducting Magnets and Materials (Section 8) and on Advanced Acceleration (Section 9). These sections also sketch subsequent R&D steps beyond the two R&D projects identified here for immediate investment.

Scenario C funding would enable the U.S. accelerator R&D program to "move boldly toward development of transformational accelerator R&D ... with an aggressive, sustained, and imaginative R&D program", as called for by the P5 strategic plan. By funding R&D projects that would hasten the development of a very high-energy proton-proton collider and of a multi-TeV e^+e^- collider, Scenario C funding would consolidate R&D areas in which the U.S. already has significant strengths and leadership positions. With this additional funding, the U.S. could maintain its traditional leadership in accelerator R&D. The R&D projects chosen would significantly enhance the state-of-the-art; consequently, they can be expected to generate exciting results that will draw new practitioners into the accelerator R&D enterprise, and that can be applied across the Office of Science. Scenario C funding would energize a vibrant accelerator-based U.S. particle physics program.

5: Accelerator Physics and Technology

The performance of modern accelerators and storage rings relies on strong expertise in accelerator physics, careful design and simulation, and high quality beam instrumentation and control. The potential Next Step accelerators, a multi-MW proton accelerator, a very high-energy proton-proton collider, and an ILC energy upgrade will be even more dependent on these research areas.

In the current GARD program, theory and computer modeling of accelerators and beams, as well as accelerator design, is contained in the Accelerator and Beam Physics thrust area. These activities will be crucial in making progress towards the realization of the Next Step accelerators. At the same time, pursuit of general accelerator science needs to be supported. The fundamental limitations in the current approaches to accelerators must be continually challenged if accelerator-based high energy particle physics is to remain a vibrant field in the long term.

5.1: Current GARD Program

The current GARD thrusts in the area defined by the Subpanel as Accelerator Physics and Technology are the Accelerator and Beam Physics thrust and the Beam Instrumentation and Controls thrust.

Accelerator and Beam Physics Thrust

The Accelerator and Beam Physics thrust has been funded at the level of ~10% of the total GARD budget during the last two fiscal years. Approximately half of this effort is at Fermilab. The other half is divided among SLAC, LBNL, and the University programs.

At Fermilab this area includes support for PIP-II, IOTA, and their beam computation effort. At SLAC, the largest effort is in beam physics and in accelerator design and computation. There is also work on SuperKEKB, the ILC final focus and machine detector interface (MDI), low level RF (LLRF), and feedback. At LBNL's Center for Beam Physics (CBP), the main effort is in advanced acceleration computation and advanced accelerator modeling.

The funds at the universities support a wide variety of general accelerator physics topics including space charge, producing and maintaining low emittance beams, evaluating a variety of advanced beam manipulation processes, and computing accelerator beam properties through advanced simulations. The work typically proceeds through many smaller-scale grants of finite duration with only a few investigators and includes graduate student support.

In general, the efforts at the laboratories tend to naturally focus on the current accelerator priorities at each of the laboratories. At universities, there is a wider variety of topics covered in accelerator science.

One area in which there has been some recent movement towards a nationally unified effort is in accelerator-related computation. Effort in the area has been boosted by funding from the SciDAC (Scientific Discovery through Advanced Computing) program jointly funded by ASCR (DOE Office of Advanced Scientific Computational Research) and HEP. One of the outgrowths of this effort is the CAMPA (Consortium for Advanced Modeling of Particle Accelerators) initiative from LBNL, SLAC, and Fermilab to establish a national program in advanced modeling of accelerators. There are, however, still many isolated simulation efforts within the program.

Beam Instrumentation and Controls Thrust

Presently, approximately 3% of GARD funding is directed towards topics in beam instrumentation and controls. The support is directed towards laboratory activities at ANL, LBNL, and SLAC. The topics supported at ANL are developing standard beam diagnostics for the Argonne Wakefield Accelerator and for characterizing its beam. At LBNL, funding is for the CBP group activity developing beam diagnostics. The CBP group works actively on: precision timing; beam controls and feedback systems; BPM (beam position monitor) development and longitudinal phase space measurement; and beam manipulations for advanced accelerator concepts. At SLAC the work is aimed at developing LLRF and feedback systems mainly for LHC.

5.2: Accelerator and Beam Physics

The effort in accelerator and beam physics, as in other areas, should, in the first instance, serve the accelerators for HEP. It should primarily advance developments towards multi-MW proton accelerators, very high-energy proton-proton colliders or TeV-scale ILC upgrades as the Next

Steps, and for the Further Future, multi-TeV e^+e^- colliders and neutrino factories.

The overarching goal for the effort towards a multi-MW proton accelerator is to understand, and to the extent possible, overcome, space charge limitations at injection energies for high-intensity proton synchrotrons. This understanding is also relevant to space charge considerations in the architecture of the booster complex of a next-generation proton-proton collider. The work on integrable non-linear optics to develop a novel ring design paradigm is an appealing area of investigation in this area. The IOTA ring at Fermilab would be able to experimentally test these ideas as, to a lesser extent, would the UMER ring at Maryland.

The study of accelerator physics issues pertaining to a very high-energy proton-proton collider currently has no significant effort in the U.S., with the last work in this area being the 2001 VLHC study.[8] Among the challenges for such an accelerator is the handling of the synchrotron radiation from protons, as well as safely managing the extremely high stored energy in the beams and the efficient operation of the overall facility, with filling times that must be significantly shorter than the average storage times for physics.

It is likely that there will be significant developments in accelerating technologies, both conventional (NCRF, SRF) and advanced (DWFA, PWFA, LWFA) technologies, in the coming decades. Any of these technologies could possibly provide a path towards a multi-TeV e^+e^- collider. Also, the anticipated upgrade of ILC to a TeV-class collider may be able to take advantage of some of these developments. Accelerator physics and simulation support in these areas are crucial for making progress.

Besides these efforts towards specific goals, it is important to maintain effort in general accelerator physics R&D. Without new ideas entering the field, accelerator science will stagnate in the long run. University programs are well suited for this type of research, particularly given the NSF Accelerator Science program with its emphasis on innovative accelerator science.

Research into muon accelerators deals with a broad range of advanced accelerator concepts including high-power targetry, muon cooling, and fast acceleration, which would lead to neutrino factories and muon colliders. While muon colliders can reach higher energies than electron-positron colliders, they would require extensive R&D. Following the recommendation of P5, the Muon Accelerator Program (MAP) is being phased out, and the muon cooling experiment, MICE, is being brought to an expedited conclusion.

5.3: Recommendations - Accelerator and Beam Physics

Recommendation 14. Continue accelerator and beam physics activities and beam instrumentation and control R&D aimed at developing the accelerators defined in the Next Steps and the Further Future Goals. Develop coordination strategies, both nationally and internationally, to carry out these studies in an efficient manner.

Recommendation 15. To ensure a healthy, broad program in accelerator research, allocate a fraction of the budget of the Accelerator Physics and Technology thrust to pursue fundamental accelerator research outside of the specific goals of the Next Steps and Further Future Goals. Research activities at universities should play a particularly important role.

5.4: Computation and Simulation

Computer simulations play an indispensable role in all accelerator areas. Currently, there are many simulation programs used for accelerator physics. There is, however, very little coordination and cooperation among the developers of these codes. Moreover there is very little effort currently being made to make these codes generally available to the accelerator community and to support the users of these codes. The CAMPA framework is an exception, and such activities should be encouraged.

The direction of development in computer technologies makes it mandatory that the accelerator simulation codes (as well as all other HEP-related codes) adapt to modern computer architectures. High performance computers are another resource that HEP has not yet sufficiently exploited. The effort to coordinate such advanced computational activities for HEP is taking place within the Forum for Computing Excellence (FCE). Accelerator simulation effort in the direction of advanced computing should also be an integral part of the FCE, as are the other areas of HEP computation. An overall goal of this coordinated effort is to maintain and update main-line accelerator computer codes to take advantage of the most modern computer architectures.

Advances in simulations, as well as in computational capabilities, raise the exciting possibility of making a coherent set of comprehensive numerical tools available to enable virtual prototyping of accelerator components as well as virtual end-to-end accelerator modeling of beam dynamics. It should be possible to construct real-time simulations to support accelerator operations and experiments, allowing

[8] "Design study for a staged very large hadron collider," VLHC Design Study Group Collaboration (Ambrosio, Giorgio *et al.*), SLAC-0591, FERMILAB-TM-2149.

more rapid and detailed progress to be made in understanding accelerator performance.

Simulation efforts are vital for new accelerator development and supporting experimental accelerator R&D studies. Such coherent efforts could be tailored after the successful LARP model that identified mutual study goals for assuring success of a given project (HL-LHC in the case of LARP) and supported collaboration among various university and laboratory partners.

5.5: Beam Instrumentation and Controls

Beam diagnostics and control systems are usually developed within the context of a specific project, where the performance and requirements of the systems are derived from the overall project requirements. This means that diagnostics system developments tend to be pursued after a specific project is approved and during the actual commissioning and operation of the project.

In the context of the Next Step accelerators, several beam diagnostic/control topics have been identified that need to be undertaken to support the development of new accelerators. These topics include synchronization and laser timing systems, proton beam halo monitoring, safe disposal of very high-energy beams, safe disposal of stored energy in large rings, and diagnostics at the targets of intensity frontier accelerators.

For example, compared to the current LHC experience, one anticipates factors of 100–1000 higher stored beam energy in a very high-energy proton-proton collider. Suitably reliable beam abort systems must be developed, along with beam dumps that will safely absorb all the beam energy. As part of this effort, work on beam collimation systems will be needed. The HL-LHC collimators have to deal with kW beam power deposition. A future large collider might have to deal with several tens or hundreds of kW beam power deposition in individual collimators. Further understanding of this issue would be appropriate.

In another example, the Fermilab neutrino source facility is developing a method to accurately determine the position of the incident proton beam on the target. The relevant beam diagnostics will have to form an integrated package with the target itself. Likewise, the target development process in future intensity frontier accelerators will need to address diagnostics issues in an integrated way.

Beam instrumentation and diagnostic tools are vital ingredients for supporting experimental accelerator R&D studies and assuring the successful commissioning and operation of an accelerator facility. Here too, a coherent effort for new beam instrumentation and diagnostics developments could be tailored after the successful LARP model.

5.6: Recommendation – Simulation and Beam Instrumentation and Controls

Recommendation 3. Support a collaborative framework among laboratories and universities that assures sufficient support in beam simulations and in beam instrumentation to address beam and particle stability including strong space charge forces.

5.7: Accelerator Experiments

Good facilities presently exist and more are in the planning stage for studying multiple aspects of e^+e^- acceleration. It is expected that these facilities will be instrumental in driving progress in this field. At present similar dedicated infrastructure is lacking for studying proton beam acceleration, even though some of the problems, especially at the intensity limit, require complete understanding of complicated phenomena in order that they be addressed and ameliorated.

There has been considerable work and enthusiasm developing around the idea that a novel ring design paradigm may lead to accelerators that have intensity performance beyond that possible in the more traditional renderings of ring accelerators. The new paradigm has one main element in contrast to working with highly linear transverse beam focusing systems, it is based on designing and building rings with specially designed non-linear beam focusing systems where in the presence of the non-linearity, a separating integral surface exists for the transverse dynamics that provides suitable transverse beam confinement. It is expected that the beam-current limits in a ring designed with a so-called integrable non-linear focusing lattice can significantly exceed that possible in rings designed and laid out in the more conventional way.

The University of Maryland has recently been funded to work on a scaled integrable optics experiment on the University of Maryland Electron Ring through the new NSF Accelerator Science Program. Fermilab, through IOTA, will build a ring where this idea can eventually be tested with protons. Such experimental programs will be an important training ground for the new generation of accelerator physicists, a pre-requisite for the successful implementation of any future large-scale accelerator project. Evaluating space charge effects in new parameter regimes should stimulate development of new ideas in beam physics and beam diagnostics. Understanding and establishing the limits of deploying integrable non-linear focusing systems is particularly important to the Next Step multi-MW proton accelerator where, the roadmap includes a technical down-select between a Rapid Cycling Synchrotron (RCS) operating with very high space charge and a high-power 6 GeV to 8 GeV superconducting RF (SRF) linac. The down-select between the RCS and SRF linac must occur in the 2020–2025 timeframe in order to provide multi-MW beams to drive the long-baseline neutrino program by 2030.

5.8: Recommendation – Accelerator Experiments

Recommendation 2. Construct the IOTA ring, and conduct experimental studies of high-current beam dynamics in integrable non-linear focusing systems.

6: Particle Sources and Targetry

Intense particle beams are an essential element of the P5 plan for future particle physics research in the U.S. The future physics program will require intense particle beams for both very high-energy colliders and for long-baseline neutrino experiments. To establish and maintain reliable operations of high-power beams will require continuous improvements in the design of accelerator and beam components. Production targets and focusing systems for the secondary charged particles that decay into the neutrino beams are particularly challenging. Components must be fabricated from materials that can withstand high radiation fields and thermal shock in addition to high temperatures over long periods of exposure. To enable the design of the future needed components, generic R&D in the properties of various materials in the hostile environment of high-power beams is necessary.

6.1: Current GARD Program

GARD has supported high-power component R&D to enable high intensity beams for the past three years through the RaDIATE collaboration, which was formed to provide a forum to develop a more coherent focus on the problem of radiation damage and thermal shock in materials. RaDIATE is an excellent start in developing a program of generic studies in the most promising materials for these high-power beam components. This research will provide important information to many areas of physics research that use high-power beams.

In addition to the GARD-funded effort, target and component research is also carried out by existing projects such as NOvA, T2K, and LBNF. However, the budget and time constraints of projects force relatively short-term, focused research that will lead to quick, workable solutions. It is not clear that these solutions will enable robust beams for reliable long-term operations.

The GARD program also supports research for the development of primary beam sources. This effort is small and should continue at the present level.

6.2: Future Needs

The currently running NOvA neutrino experiment at Fermilab plans to increase the beam power on target from 350 kW to 700 kW using the existing target and focusing system. Operational experience to date indicates that this plan is likely to be adequate. In the future LBNF plans to run with 1 MW to 2 MW of beam on the target. For this experiment and future experiments, or upgrades, it is not clear that the target and focusing systems being planned are adequate for reliable operations. Project-supported R&D is expected to improve targets and beam components for running at the higher power levels. For example, a focusing system with a much higher duty cycle than that of the present system is one of the key issues for future high-intensity experiments. But without generic R&D to investigate materials, the reliability of operations could be inadequate even for the planned experiments. Increased generic research is likely to improve the viability for running all future high-intensity neutrino programs by improving the reliability and efficacy of target/focusing systems. In addition, beam components such as collimators, beam dumps, and magnets for the very high-energy proton-proton collider are likely to be improved by these studies.

Radiation damage studies with high-energy proton beams is very expensive, while similar doses to materials of interest can be obtained using low-energy ion beams, which are readily available and at much lower cost. To use the data from these low-energy beam exposures, an experiment using both types of beams on a candidate material as a calibration has to be carried out.

Further improvements in the operations of high-power secondary beams can be obtained through detailed computer modeling of targets and focusing systems. When coupled with radiation damage and thermal shock tolerance, such studies have the potential for significantly increasing the integrated neutrino flux at a remote detector through increased efficiency and reliability.

6.3: Recommendation

Recommendation 1. Fund generic high-power component R&D at a level necessary to carry out needed thermal shock studies and ionizing radiation damage studies on candidate materials that are not covered by project-directed research.

7: RF Acceleration

Most present day particle accelerators require radio frequency (RF) accelerating cavities to supply the electric field utilized to increase the energy of the charged particles in the beam. The accelerating gradient (energy gain per unit length) is one of the key factors determining the cost of linear colliders. For circular accelerators, higher accelerating gradients potentially enable shorter acceleration cycles. RF cavities have evolved from the D-shaped cavities in the earliest cyclotrons to the modern elliptical shaped superconducting cavities made from pure niobium. Recent advances in cavity geometries and copper alloys have also enabled significantly higher gradients in high-frequency cavities.

Normal conducting RF (NCRF) acceleration has the widest range of applications of all acceleration technologies, ranging from small (< 10 MeV), low average-power commercial linacs (*e.g.*, for external beam radiation medical procedures, industrial ion implantation, and non-destructive testing) to high peak-energy accelerators (*e.g.*, the 50-GeV Stanford Linear Collider) and high average-power accelerators (*e.g.*, the megawatt-class, 800-MeV LANSCE accelerator operating at 0.1% duty factor) for discovery science. This technology dates back to its first demonstration in 1946 and the U.S. is still recognized as world-leading in this field.

Superconducting radio-frequency (SRF) acceleration systems are today one of the critical technologies for the majority of operating and future accelerators around the world, including linear and circular e^+e^- colliders, high-power proton accelerators, neutron spallation sources, and linear and circular light sources. The U.S. has been among the leaders in SRF development and applications since its inception. SRF technology, along with fundamental understanding of the science of RF superconductivity, is making great strides in improving the performance of SRF cavities in terms of increased accelerating gradient and reduced losses.

7.1: Normal Conducting RF Acceleration

7.1.1: Current GARD Program in NCRF Acceleration

At SLAC there is an active program to improve the accelerating gradient of high frequency (X-band) normal conducting cavities. Significant progress has been made in understanding the breakdown process as being due to peak magnetic fields. A program is under way using novel cavity geometries that increase the shunt impedance and a higher strength copper alloy to limit breakdown damage. This program has produced cavities with an accelerating gradient of more than 180 MV/m at room temperature. The development of novel RF source architecture (including multi-beam, sheet-beam, and radial-beam variations) is under way at SLAC and elsewhere and is supported by the GARD program.

7.1.2: Opportunities and Challenges in NCRF Acceleration

Recent progress of NCRF has generated interest in possible application of this technology for a multi-TeV e^+e^- collider. One of the fundamental problems with normal conducting linacs is the short pulse length associated with high frequency to avoid breakdown. The short pulses require pulse compression techniques that result in reduced power efficiency. A key performance goal needed for a viable multi-TeV future collider is to increase the wall-plug efficiency (increase efficiency in order to reduce energy consumption).

Recent progress indicates that high shunt-impedance, high-gradient NCRF cavities and efficient high-power short pulse RF sources can result in systems that are modular and more efficient. In particular, new sheet-beam or multi-beam RF sources, or smaller parallel distributed sources can generate extremely high peak-power with reasonable rise and fall times without the need for RF pulse compression. Therefore the sources can be well matched to ultra-high-gradient structures, and the wall-plug to beam power efficiency can be dramatically improved. In addition, new accelerator structure topologies can provide a much higher efficiency structure with higher beam loading. Gradients up to 300 MV/m now seem feasible. Therefore, more efficient and more cost-effective NCRF colliders may become possible.

7.1.3: R&D and Recommendations on NCRF Acceleration

In order for high-gradient NCRF technology to be considered as a viable option for a multi-TeV collider, the following R&D elements must be demonstrated:

Integrated RF sources: Integrated RF sources from the A.C. line to the accelerator structure are a key component of this novel NCRF architecture. RF sources need to be developed followed by a system demonstration. An appropriate R&D

target is a full module with wall plug to RF efficiency exceeding 50%, and with more than 200 MW of peak RF power.

Accelerator structures: Copper alloy structures operating at above 150 MV/m (and short structures at ~180 MV/m) have already been demonstrated while a full-length accelerator structure has not been demonstrated. NCRF structures typically need multi-bunch trains for reasonable efficiency, and so require HOM damping. A higher order mode (HOM) damped structure operating at high gradient will need to be demonstrated using photonic-band gap or other novel wakefield suppression techniques, and it is not clear whether high efficiency can be maintained. A possible alternative is single-bunch (or few-bunch) train structures that do not require damping but need effective energy recovery. Cooled structures have been experimentally demonstrated to operate above 300 MV/m, with similar values expected theoretically for multi-frequency structures. Going from 150-MV/m multi-bunch structures to 300-MV/m single bunch structures will require serious development efforts to achieve the required amount of energy recovery for high-efficiency operation and for optimizing collider designs based on these structures.

To demonstrate the feasibility of this technology for a multi-TeV e^+e^- collider, a multi-stage prototype will need to be constructed. Such a prototype should integrate high-efficiency RF sources with accelerating structures and should be tested with beam to confirm loaded gradient, emittance control, and wall plug to beam efficiency. In addition, it should provide useful information on costs/MeV. The infrastructure of the NLCTA facility at SLAC (RF power and beam sources) would be useful for testing these novel structures.

Recommendation 11. Continue research on high-efficiency power sources and high-gradient normal conducting RF structures.

Recommendation 12. Make NLCTA available for RF structure tests using its RF power and beam sources.

Recommendation 13. Focus normal conducting RF R&D on developing a multistage prototype based on high-gradient normal conducting RF structures and high-efficiency RF power sources to demonstrate the effectiveness of the technology for a multi-TeV e^+e^- collider.

7.2: Superconducting RF

7.2.1: Current GARD Program in SRF Acceleration

At Fermilab, “nitrogen doping,” a surface treatment technique for superconducting cavities in a nitrogen atmosphere, has led to dramatic increase of Q_0, the cavity quality factor. Nitrogen doping of 1,300-MHz niobium cavities can reliably increase the practical medium-field Q_0 by about a factor of three above standards from a few years ago. Reliable production procedures, suitable cool-down procedures, and corresponding cryomodule designs are being developed for LCLS-II, the planned SRF linac upgrade of SLAC’s LCLS X-ray laser. In addition, a full ILC cryomodule, housing eight cavities, has been assembled and tested with an accelerating gradient exceeding the ILC specification of 31.5 MV/m. The PIP-II linac upgrade project at Fermilab is developing high-performance lower-frequency SRF cavities. The R&D for these cavities was originally supported by GARD but has now been transferred to the PIP-II project.

At Cornell, GARD has supported SRF research on highest-gradient ILC and other cavities, highest Q_0 cavities, novel SRF materials, especially Nb_3Sn, and research on fundamental SRF field limits. Significant achievements include world-record gradient and the first Nb_3Sn cavity exceeding specifications of bare niobium in its parameter range.

GARD also supports SRF activities at ANL and several universities. The focus of these programs is on ILC geometry cavities.

7.2.2: SRF R&D Outside the GARD Program

The GARD SRF program is complemented and significantly leveraged by developments that take place outside GARD. These include developments in the U.S. pursued by universities supported by the NSF, other offices of the Office of Science, and by a vibrant international program in SRF R&D motivated by a number of current and future projects.

Cornell University is contributing to high-gradient SRF cavities by studying cost-efficient cavity preparation. Medium gradients (15 MV/m) and high Q_0 were achieved at 4.2 K, opening the way to much simpler refrigeration systems and much cheaper cavity operation. Old Dominion University (ODU) has developed superconducting “spoke cavities”, which are non-elliptical structures typically used in proton and heavy-ion linacs, and have potential applications for the low-frequency portions of PIP-II. ODU has an ongoing program in SRF theory.

There are also ongoing SRF activities outside the HEP program. Funded by the Office of Basic Energy Science, LCLS-II will have a cryogenic system whose size depends strongly on the surface resistivity of the nitrogen-doped Nb cavities. Surface preparations for low surface resistivity and high Q_0 are being developed. Funded by the Office of Nuclear Physics, FRIB requires SRF for low-beta ($\beta = v/c < 1$) cavities that accelerate ions. These cavities have synergies with the low-energy end of PIP-II.

7.2.3: Opportunities and Challenges in SRF Acceleration

The subjects of SRF R&D that are relevant to HEP and that should be targeted by GARD funding include:

- Developments targeted at specific future accelerators:
  - High gradient R&D, targeting 80 MV/m and exploring new materials and structures for ILC upgrade;
  - HOM-damped cavities and "crab" cavity development for future very high-energy proton-proton colliders;
  - High Q R&D, aiming for 4×10^{10} to 5×10^{10} at high gradient for ILC upgrade and future proton linacs for high-power proton beams;
- Broadly applicable developments that are needed for a healthy program:
  - Overall efficiency optimization and cost reduction for both capital and operating expenditures;
  - SRF test facilities with beam for R&D and training.

Gradients close to 80 MV/m are feasible by using new SRF materials and would provide an excellent option for an energy upgrade of the ILC to ~1 TeV. In order to use the ILC linac, the new cavities must be compatible with the ILC repetition rate and bunch and pulse structure. This upgrade is only possible with high-gradient SRF, not with NCRF or plasma wakefield acceleration. With further R&D, the cost of a TeV-scale SRF-based collider can be reduced.

The focus of SRF R&D for a very high-energy proton-proton collider is on efficient and highly HOM-damped SRF multi-cell accelerating cavities and "crab cavities," transversely deflecting cavities installed near the interaction point for the purpose of increasing the luminosity of the collider.

A continuous wave (CW) SRF linac is one of two candidate technologies for a new accelerator that will replace the aging Fermilab Booster and enable multi-MW proton beams for the long-baseline neutrino program. Advances in SRF, particularly improvements in accelerating gradient, could substantially benefit the multi-MW proton facility. They should be pursued in time to inform the technology selection in the 2020–2025 timeframe.

New SRF materials and structures could dramatically improve the accelerator gradient and lower power consumption. Applied materials science should be directed at high-gradient SRF materials including alternate SRF materials, thin films (superconducting coatings for SRF cavities, which promise to lead to acceleration gradients higher than those obtained by bulk niobium), and new processing techniques. There should also be an effort to optimize the cavity geometry for new high-gradient structures.

The availability of SRF test facilities, including some with beam, for R&D and training is key to progress in SRF R&D. SRF R&D is complex and expensive, and well-equipped test facilities with trained service personnel are essential for further progress. While vertical cavity testing facilities are common, currently only FNAL, Cornell, and JLab are equipped to test cavities horizontally, with Cornell's setup being close to a realistic accelerator environment. Cornell's test setup has also been used with an electron beam.

7.2.4: Recommendations on SRF Acceleration

Recommendation 4. Direct appropriate investment in superconducting RF R&D in order to inform the selection of the acceleration technology for the multi-MW proton beam at Fermilab.

Recommendation 6. Increase funding for development of superconducting RF (SRF) technology with the goal to significantly reduce the cost of a ~1 TeV energy upgrade of the ILC. Strive to achieve 80 MV/m accelerating gradients with new SRF materials on the 10-year timescale.

7.3: NCRF and SRF R&D in Scenario C

R&D in NCRF and SRF acceleration that could be enabled by Scenario C funding for application to a multi-TeV e^+e^- collider is discussed in Section 9.6.

8: Superconducting Magnets and Materials

The P5 report states, "A very high-energy proton-proton collider is the most powerful future tool for direct discovery of new particles and interactions under any scenario of physics results that can be acquired in the P5 time window." The report also states, "The U.S. is the world leader in R&D on high-field superconducting magnet technology, which will be a critical enabling technology for such a collider." In light of these observations, the P5 strategic plan endorses medium-term R&D on high-field magnets and materials in the context of its Recommendation 24: "Participate in global conceptual design studies and critical path R&D for future very high-energy proton-proton colliders. Continue to play a leadership role in superconducting magnet technology focused on the dual goals of increasing performance and decreasing costs." Conceptual design studies aimed toward future very high-energy proton-proton colliders are now being organized in Europe and in China. In order for the U.S. to maintain a leadership role in magnet technology and to provide a basis for a unique and significant intellectual contribution to a future very high-energy proton-proton collider, it is imperative that the U.S. superconducting magnet and materials R&D program be adequately funded and effectively coordinated.

8.1: Current GARD Program

The primary superconducting magnet development activity within the GARD program is at Fermilab (~57%), with a smaller design and test program at LBNL (~20%) and a small conductor development and test program at BNL (~5%). The university-based program (~18%), primarily at Florida State University, focuses on conductor development and is about the same size as the LBNL program. The combined superconducting magnet and materials programs represent about 20% of the total GARD budget. The LARP program to develop the Nb_3Sn quadrupoles for the HL-LHC, which was previously supported by the GARD program, has now been separated off as a directed R&D project.

The programs at each institution are generally complementary. BNL provides infrastructure for fabrication and testing that is critical for the success of the HL-LHC project and leadership in LARP/HL-LHC conductor development. The BNL magnet R&D program is also supported through a variety of funding sources outside of GARD. FNAL provides extensive fabrication and testing facilities, and is primarily focused on accelerator-quality magnet development with a small R&D component. R&D at LBNL, on the other hand, concentrates on novel approaches to technology development. This level of cooperation has served the community well. Strengthening coordination and developing unified goals are important for the next steps forward.

8.2: Challenges of a Very High-Energy Proton-Proton Collider

The P5 mandate for accelerator R&D is clearly driven by cost considerations of future colliders, such as a very high-energy proton-proton collider, because of their increased scale and complexity as their energy and/or intensity increases. Important accelerator physics challenges of next-generation proton-proton colliders are described in Section 5 on Accelerator Physics and Technology, most notably the path from luminosities of ~0.7×10^{34} cm^{-2}s^{-1}, already achieved at the LHC, to luminosities exceeding 10^{35} cm^{-2}s^{-1}, presently beyond our grasp. Intriguingly, there are no in-principle physics or technical barriers today to increasing the energy of a next-generation proton-proton collider by an order of magnitude. The leading challenge to eventually realizing such a facility is reducing the cost of technical components throughout the multi-ring complex, which is dominated by dipole magnets.

The LHC dipole magnets and present cost models for dipole magnets are based on superconducting technology now four decades old. Based upon presentations to the Subpanel, cost models point to a broad cost optimum with dipole fields between 5 T and 12 T when collider size constraints are not a factor. Site geography constraints that limit the ring circumference can drive the desired dipole field up to 20 T. This range of values for optimal field motivates developing a basis for a variety of dipole options in order to minimize the overall cost of a future collider, which must balance technical trade-offs as well as geographical and political constraints. Designs based on smaller rings with high-field magnets will need to cope with high injection rates and dramatically higher synchrotron radiation, through for example, larger bores and beam screens or open mid-plane designs. Designs based on larger rings with low-field magnets can likely mitigate synchrotron radiation with techniques evolved from the LHC but will need to deal with much higher stored beam power that scales with ring circumference.

8.3: Opportunities and Comments

Transformational technology, targeting a cost-performance improvement, measured in units of Tesla-meter, of a factor of three relative to the LHC dipoles, is clearly very desirable for a next-generation proton-proton collider. Such cost-performance improvement should be the primary goal for the superconducting magnet and materials thrust, and can only be achieved by introducing new paradigms and aggressively pushing technology beyond established limits. Modest improvements of the status quo will not be adequate. Potential for cost reduction exists in several areas: eliminate training of the superconducting matrix; decrease the required operating margin; improve mechanical stress mitigation to allow optimal grading and to improve conductor performance; and automated assembly techniques to reduce touch-labor. The tools and techniques that have been developed in the last decade through the GARD program and the U.S. LHC Accelerator Research Program (LARP) now make success in reaching the above cost-reduction goal feasible. Possibilities for increasing performance and/or reducing cost should be prioritized, and pursued in parallel to the extent possible. The U.S. High Field Magnet (HFM) community started to organize and prioritize activities in a white paper submitted to the Subpanel. This white paper can serve as a basis for formulating U.S. R&D goals and program in coordination with international partners.

Reducing magnet cost relative to LHC dipoles by a factor of three per Tesla-meter is a plausible goal for superconducting magnet R&D. As a point of reference, the cost of the present state-of-the-art LHC NbTi dipoles can be considered to have roughly three equal components: superconductor, other materials, and labor. Achieving an overall factor of three cost reduction requires targeting a large reduction in each component, for example:

- For the superconductor, a factor ~3 by: reducing the cost of Nb_3Sn to the same cost per kg as NbTi; achieving more than a factor of two improvement in field for the equivalent amount of conductor by grading, which is particularly effective for high fields; and finally, by targeting a 30% increase in the critical current density of Nb_3Sn.

- For the other materials, factor of ~2 cost reduction, plausible by: reducing or eliminating end-parts and wedges; considering iron only as shielding, not for flux return; and incorporating advanced manufacturing techniques.

- For labor, factor of ~4 cost reduction, plausible through automated manufacturing techniques in conjunction with simplicity of design, magnet length, operating temperature, installation and quench protection.

Elements of an aggressive and robust program to realize these cost-performance goals should include:

1. Development of R&D platforms that reduce turn-around time for model construction and testing, presently more than a year per test structure. Platform development includes development of test facilities and advanced diagnostics.

2. Creation of a suite of design tools that, combined with sophisticated diagnostics, will allow accurate prediction of magnet performance.

3. Engagement of universities and industry to develop manufacturing techniques to reduce touch labor.

4. Focus on magnet geometries that take into account synchrotron radiation heat loads.

Superconducting materials research is of great interest to U.S. and international researchers and is the foundation of recent successes in magnet development. Dipole magnets based on Nb_3Sn are capable of producing dipole fields between 10 T and 16 T at present performance levels but are currently substantially higher cost than NbTi per Tesla-meter. High Temperature Superconductors (HTS) are needed for fields above 16 T. Substantial improvement in HTS conductors (*e.g.* ReBCO and Bi-2212), has been achieved; however, HTS conductors are still in the early stages of development, and many technical challenges remain. Presently, foreseeable costs are prohibitive for use in future colliders except in limited applications such as interaction region (IR) magnets and separation dipoles, where HTS could be enabling. The U.S. currently leads in HTS conductor R&D, and a significantly enhanced program of HTS materials research and conductor development would ensure continued U.S. leadership, while presenting opportunities for substantial collaboration and synergy with materials research across the Office of Science and for creating new paradigms for both accelerator magnets and applications beyond HEP.

The need for capabilities in designing and constructing high-field solenoids for a variety of applications such as spectrometer magnets has been demonstrated. The recent NRC MagSci report[9] describes extensive future needs in addition

[9] "High Magnetic Field Science and Its Application in the United States: Current Status and Future Directions (2013)", National Academies Press

to those in the Office of Science and opens the possibility of intra-agency collaboration.

Currently, no significant market driver exists that would lead to significant cost reduction for HTS materials (especially for Bi-2212). Identifying synergies with other potential applications in order to develop markets could be extremely beneficial.

The modestly funded HEP Conductor Development Program along with HEP-funded laboratory and university materials and conductor programs have made excellent progress in improving the properties of Nb_3Sn, and have led to a number of successful high-field dipole test structures and adoption of Nb_3Sn for quadrupole magnets critical to the HL-LHC. Goals for further performance improvement include increasing the critical current (J_c) and reducing magnetization while maintaining an adequate Residual Resistivity Ratio (RRR).

Capital investment with vendors to produce conductor for large-scale accelerator-quality prototype magnets would energize the present small and effective program and would contribute to American market competitiveness.

Substantially reducing the "touch labor" and material costs of next-generation collider magnets, while increasing magnet reliability and ease of operation, will be important factors toward collider affordability.

A healthy program of superconducting magnet and materials R&D is necessary to meet the ambitious goals demanded by future very high-energy proton-proton colliders, as well as to ensure an adequate resource pool for the success of the LARP program. Currently, GARD funding allocated for superconducting magnet R&D (excluding materials) is just over $5 M per year, and is barely sufficient for a viable program at a single laboratory. U.S. leadership in superconducting magnets and materials was established by past R&D investment at higher levels. Given the need for and challenge of transformational improvement, an adequately funded, well-coordinated national program is required. A reasonable funding level in the base budget would be about $10 M per year, not including facilities support. The materials program, including the Conductor Development Program, is now about $2.7 M and, if directed appropriately, is adequate in the constrained base budget where the priority focus should be improving both the technical and cost performance of Nb_3Sn based dipoles.

8.4: Recommendations

Recommendation 5. Participate in international design studies for a very high-energy proton-proton collider in order to realize this Next Step in hadron collider facilities for exploration of the Energy Frontier. Vigorously pursue major cost reductions by investing in magnet development and in the most promising superconducting materials, targeting potential breakthroughs in cost-performance.

Recommendation 5a. Support accelerator design and simulation activities that guide and are informed by the superconducting magnet R&D program for a very high-energy proton-proton collider.

Recommendation 5b. Form a focused U.S. high-field magnet R&D collaboration that is coordinated with global design studies for a very high-energy proton-proton collider. The over-arching goal is a large improvement in cost-performance.

Recommendation 5c. Aggressively pursue the development of Nb_3Sn magnets suitable for use in a very high-energy proton-proton collider.

Recommendation 5d. Establish and execute a high-temperature superconducting (HTS) material and magnet development plan with appropriate milestones to demonstrate the feasibility of cost-effective accelerator magnets using HTS.

Recommendation 5e. Engage industry and manufacturing engineering disciplines to explore techniques to both decrease the touch labor and increase the overall reliability of next-generation superconducting accelerator magnets.

Recommendation 5f. Significantly increase funding for superconducting accelerator magnet R&D in order to support aggressive development of new conductor and magnet technologies.

8.5: Scenario C – Roadmap for Superconducting Magnets and Materials for a Very High Energy Proton-Proton Collider

The P5 report called for a roadmap for the U.S. to "move boldly toward development of transformational accelerator R&D … with an aggressive, sustained, and imaginative R&D program … changing the capability-cost curve of accelerators" in Scenario C. Motivated by the P5 science drivers, the goal is to "make these further-future accelerators technically and financially feasible on much shorter timescales." Investment in the R&D necessary for the realization of P5's strategic vision of the very high-energy proton-proton collider in the medium-term, especially investment in superconducting magnets and materials R&D, is one of two investments that the Subpanel identified for Scenario C.

Recommendation C1. Hasten the realization of the accelerator of P5's medium-term vision for discovery: a very high-energy proton-proton collider, and the realization of the accelerator of P5's long-term vision for discovery: a multi-TeV e^+e^- collider.

In order to hasten the very high-energy proton-proton collider, and thus to propel particle physics forward, it is necessary to ramp up research and development of superconducting magnets to a level that permits a multi-faceted program to explore possible avenues of breakthrough in parallel. As explained in Section 4, the Subpanel envisions realizing this "fast-track" program by supplementing the base accelerator R&D program of Scenario A or B with a sequence of R&D projects directed along the path to the very high-energy proton-proton collider.

The path to a very high-energy proton-proton collider

While realization of a very high-energy proton-proton collider is confronted with many technical challenges, arising from a range of issues spanning accelerator physics and technology, the leading challenge is the technical performance and cost reduction of the superconducting dipole magnets. Development of the high-field superconducting dipole magnets is a long lead-time technical challenge that will require many years of R&D. Breakthroughs are required in the cost-performance of superconducting magnet technology. For this reason, the path to this accelerator's realization starts with the magnets.

The initial phase of superconducting magnet and materials R&D will help to establish a foundation for further development in later phases. It will produce short model magnets that reach the desired field, satisfy the necessity for manufacturability, and are compatible with handling the high synchrotron radiation heat loads and the large stored energy. It will include continued R&D on superconductor performance and cost reduction. The initial phase will also be an opportunity to demonstrate the potential of HTS for accelerator magnets by building short demonstrator magnets and high-field inserts.

The increased investment possible in Scenario C will enable a ramp-up in superconducting magnet and material R&D in its initial phase to a level that permits a multi-faceted program to explore possible avenues of breakthrough in parallel. It will enable investigation of additional magnet configurations, fabrication of multi-meter prototypes, and exploration of low-cost manufacturing techniques and industrial scale-up of conductors. It will also enable increased support for HTS material and magnet development in order to realize the tremendous potential of high-temperature superconducting materials via demonstration of accelerator-quality HTS magnets. This multi-faceted program comprises the urgent, high-priority accelerator R&D project towards the very high-energy proton-proton collider that is identified by the Subpanel for immediate investment in Scenario C.

Recommendation C1a. Ramp up research and development of superconducting magnets, targeted primarily for a very high-energy proton-proton collider, to a level that permits a multi-faceted program to explore possible avenues of breakthrough in parallel. Investigate additional magnet configurations, fabricate multi-meter prototypes, and explore low-cost manufacturing techniques and industrial scale-up of conductors. Increase support for high-temperature superconducting (HTS) materials and magnet development to demonstrate the viability of accelerator-quality HTS magnets for a very high-energy proton-proton collider.

Following this first R&D project on superconducting magnets, and guided by the results of that project, it will be appropriate to launch additional, second-generation R&D projects targeting the next set of breakthroughs needed for the realization of the very high-energy proton-proton collider. As the enabling technology and cost-driver, superconducting magnets will demand further R&D. The second phase of superconducting magnet and material R&D will include increased support for conductor R&D, as well as longer and more complex model magnets, for example, dual-bore dipoles, quadrupoles, and injector magnets. Other critical path R&D projects towards a very high-energy proton-proton collider should begin to address such profound technical challenges as: the extremely high stored energy in the beams; the synchrotron radiation load on magnets, cryogenics and vacuum system; electron cloud effects; and space charge effects in the injector complex. They should also include study of accelerator physics issues that affect the collider design, while folding in the results of R&D into the overall collider optimization, including cost. Simulation activities should also be conducted to address the technical challenges and accelerator physics issues, and to guide optimization of the collider design. Each second-generation

R&D project will move the program forward in some critical aspect, and will guide the subsequent steps in R&D.

Subsequent R&D projects will address such superconducting magnet R&D activities as conductor scale-up, manufacturing studies, and critical performance demonstrations. An important example is to study magnet performance under an actual synchrotron radiation heat load. As the R&D program matures, further R&D projects should be initiated at a steady rate commensurate with progress.

By following this path, the U.S. can continue to be a world leader in superconducting magnet research and development and be a major partner in the design and development of a very high-energy proton-proton collider as a future global project.

9: Advanced Acceleration

Advanced acceleration, defined as concepts in a broad range of new approaches to accelerating particles, with emphasis on significantly higher gradient operation, pushes the limits of science and technology. Its novel concepts offer the potential of dramatic reduction of the size and cost of future accelerators, and development of these concepts has led to high-profile results that have captured the interest of the wider science community. There are various acceleration mechanisms associated with this new branch of accelerator science including: plasma wakefield acceleration driven by electron beams (Particle-driven Wakefield Acceleration, PWFA) or by lasers (Laser-driven Wakefield Acceleration, LWFA); dielectric wakefield acceleration (DWFA) using an electron drive beam to create electromagnetic (Cerenkov) wakes in a dielectric structure; direct laser acceleration (DLA) using optical scale dielectric structures to generate a longitudinal electric field driven by a laser; and muon acceleration.

The P5 report endorses long-term R&D on advanced acceleration in the context of Recommendation 26:

> *Pursue accelerator R&D with high priority at levels consistent with budget constraints. Align the present R&D program with the P5 priorities and long-term vision, with an appropriate balance among general R&D, directed R&D, and accelerator test facilities and among short-, medium-, and long-term efforts. Focus on outcomes and capabilities that will dramatically improve cost effectiveness for mid-term and far-term accelerators*

and Recommendation 25 "Reassess the Muon Accelerator Program (MAP). Incorporate into the GARD program the MAP activities that are of general importance to accelerator R&D."

For the last thirty years, the U.S. has been at the forefront of this new field. Notable progress has been made with sustained acceleration gradients of 50 GV/m achieved and narrow energy spread beams produced up to 4 GeV. However, in order for any of the advanced acceleration approaches to be applied to practical uses such as an energy-frontier physics facility, or a photon-science facility, significant further scientific and technical advancements are required.

Research efforts in advanced acceleration represent excellent accelerator science and have noteworthy university contributions and laboratory-university collaboration. The programs have been successful in attracting excellent researchers and students into the accelerator and beam physics communities. The importance of accelerator science, the role in the field played by university programs, and laboratory-university collaborations are recognized by the P5 report Recommendation 23: "Support the discipline of accelerator science through advanced accelerator facilities and through funding for university programs. Strengthen national laboratory-university R&D partnerships, leveraging their diverse expertise and facilities." Although the advanced acceleration program is only beginning to address the challenges of enabling an energy-frontier physics facility, its research raises the profile of accelerator physics as a scientific discipline in its own right, and delivers considerable value to U.S. high energy physics. It is important to note that advanced accelerators are now approaching beam parameters needed to enable compact x-ray free-electron laser light sources driven by GeV-class electron beams. Realization of this new generation of light source may be a highly synergistic stepping stone in the development of a TeV-class e^+e^- collider.

9.1: Current GARD Program

The advanced acceleration thrust has been funded at the level of 35% of the total GARD budget in the last two fiscal years. Progress in research and development in advanced acceleration heavily relies on facilities. Operations of facilities at national laboratories account for about 60% of the total advanced acceleration budget. Although the scale of facilities at universities is small in comparison, they play an important role in science and workforce development.

9.1.1: Facilities at National Laboratories

FACET is a proposal-driven user facility at SLAC for developing advanced acceleration concepts. First among these is electron beam-driven PWFA with the beam having the unique properties of high charge, short pulse, and emittance needed to create > 10 GV/m wakefields. Other experiments at FACET include > 1 GV/m wakefield acceleration in dielectric structures, tests of periodic metallic structures, and tests of the effectiveness of linear collider final focus feedbacks and alignment algorithms. FACET can provide appropriate drive and witness beams of electrons or positrons, along with high-power lasers for plasma ionization. It will cease operations at the end of 2016 when LCLS-II takes over the first third of the SLAC linac.

BELLA is an LWFA experiment at LBNL utilizing a large 40-J, 1-Hz laser. It also employs a 10-Hz terawatt-level laser, TREX, in its research program. Significant progress has been made in accelerating electron beams up to > 4 GeV with ~6% energy spread and ~1% energy spread for 0.5 GeV beams. The work at BELLA is currently world-leading, but heavy competition is expected soon from the nearly one billion euro ELI Project, which is being constructed in Europe. Positron acceleration experiments are not currently possible at BELLA, but may be enabled in the future.

The **Argonne Wakefield Accelerator (AWA)** facility at ANL has been built to demonstrate the two-beam concept and key technologies of wakefield generation by high-charge beams in dielectric cylinders (DWFA). Research is concentrated on operation at 200 MV/m to 400 MV/m gradients in the frequency range of 20 GHz to 60 GHz. The recently commissioned AWA upgrade can deliver a 75-MeV drive beam with up to 10 pulses of 100-nC charge with a few picosecond pulse length and a beam power within the macro pulse of 10 GW at a repetition rate of 60 Hz.

BNL's **Accelerator Test Facility (ATF)** is a highly productive user facility funded via DOE's Accelerator Stewardship program. Experiments at the ATF are proposal-driven and some are funded by GARD. It provides synchronized high-brightness electron-beams and high-power laser-beams to three beam lines. At the ATF, users study beam physics relevant to modern accelerators, broad applications, and new techniques of particle acceleration. An approved upgrade will bring the facility to 160-MeV beams with the possibility of a further extension to 500 MeV.

Among these facilities, FACET provides the highest energy beams and the ability to drive very high-field wakes. The ATF will provide beams in the medium energy range with the flexibility to service a wide variety of users. The AWA has more limited energy reach, but has the ability to examine issues associated with electromagnetic fields and beam powers needed for future colliders.

9.1.2: University Programs

The university programs in advanced acceleration have had a profound effect on the GARD program. The concept of plasma acceleration began with the papers of John Dawson and quickly led to campus-based experimental programs on laser-driven wakefields and to pioneering computational efforts that have grown to become essential to progress in advanced acceleration. University researchers have played an integral role in the development of the PWFA program at SLAC and have been major users of both the FACET and NLCTA facilities at SLAC, and the ATF at BNL. There are also significant efforts in universities based on on-campus laboratories with smaller-scale lasers and accelerators. Groups at several universities have launched investigations on several alternate acceleration approaches including DWFA and DLA. In addition, there are notable university efforts in the development of the theory and computational tools needed for understanding advanced accelerators. These efforts continue to form a strong intellectual foundation for the existing experimental programs in advanced acceleration techniques, both in the universities and in the national laboratories.

9.1.3: Wakefield Acceleration in Plasmas

Present PWFA activities are primarily conducted at FACET and the ATF. LWFA activities are conducted at BELLA and university-based facilities. Already tens of GV/m fields have been produced in plasmas excited by both high-intensity particle beams (PWFA) and high-intensity laser beams (LWFA), resulting in total acceleration of 50 GeV and 4 GeV, respectively. Further, LWFA accelerated beams with ~100 pC charge with large energy spread and 6 pC beams to 4 GeV with 6% energy spread and an angular divergence of 0.3 mrad. Low emittance beams with an energy of 0.46 GeV, 0.6 pC bunch charge, normalized emittance of 0.1 mm mrad to 0.2 mm mrad, and a 2% energy spread have been accelerated at LOASIS at LBNL. With these accomplishments and attendant promise, the wakefield accelerator research has attracted a large community. For example, the majority of 270 participants of the 2014 Advanced Accelerator Concepts Workshop were associated with the plasma acceleration working groups, giving more than a hundred presentations.

9.1.4: Dielectric Wakefield Acceleration

DWFA has been pursued for a few decades by the Argonne Wakefield Accelerator (AWA) group in the cm-wavelength and 100-MV/m regime and by dedicated experiments at FACET and the ATF which explore the mm-wave-to-THz spectral region and GV/m fields. The AWA concept is to generate an accelerating field in vacuum from a drive beam in a dielectric cylinder, while more elaborate photonic structures with other symmetries are explored elsewhere. DWFA has seen a surge in activities by a number of universities, small businesses, and national laboratories. Recent UCLA-SLAC experiments at FACET have shown sustained fields of 2 GV/m acting over a 15-cm length, resulting in 300-MeV energy change.

9.1.5: Direct Laser Acceleration

DLA may be constructed by use of planar dielectric structures having similarities to DWAs, or through use of the inverse free-electron laser (IFEL) mechanism. The achievements of the small DLA structure community are very modest in terms of acceleration, with only modest (100 keV) broadening of the electron energy spectrum observed. The IFEL has had notably greater success, with acceleration of over 50 MeV observed at the ATF. The IFEL is also often used to produce optical microbunching for advanced accelerator and FEL applications; this technique has served as the basis for injection and staging experiments.

9.2: Opportunities and Challenges

Most of the activities in advanced acceleration represent excellent accelerator science. However, in order to make significant progress toward the TeV-class e^+e^- collider, more dedicated efforts will be required. This research may be defined and honed by introduction of a challenging "stepping stone" demonstration project at a few-GeV energy, such as an ultra-compact FEL light source. Such an initiative would enable beam quality and system feasibility issues to be vigorously explored.

9.2.1: Wakefield Acceleration in Plasmas

Plasma-wakefield accelerators arguably offer the best possibility of providing accelerating gradients much in excess of 1 GV/m. However, it remains to be demonstrated whether plasma wakefield techniques can be incorporated into practical accelerators useful for high-energy physics or other areas of accelerator-based discovery science.

For both laser- and beam-driven wakefield accelerators, the major issues essential for a collider remain open. Requirements are beam stability and control, narrow energy spread of the beam, emittance and brightness preservation, then stageability (a witness beam accelerated by a number of drive beams), high repetition rate and, eventually, positron acceleration. It is also critical that sufficiently high wall-plug-to-beam efficiency and high operational reliability of the approaches be demonstrated. Both the PWFA and LWFA research programs require substantial research infrastructure to take these next steps.

9.2.2: Dielectric Wakefield Acceleration

Dielectric wakefield accelerators may offer a possible approach to deliver ~1 GV/m gradients. With the expected closure of the CLIC Test Facility 3 at CERN, the AWA would be the only facility designed to conduct two-beam accelerator tests at cm wavelengths. To reach mm wavelengths at THz frequencies, a FACET-class beam is needed to explore GV/m DWFA performance and would require the full capability of the ATF.

More recently, more innovative approaches to structure optimization such as Bragg reflectors and photonic bandgap structures have been introduced, and impressive progress has been made. DWFA has applications in other areas of the DOE's Office of Science, such as the generation of unique narrow-band, very high-power sources THz radiation, and beam energy chirp compensators.

9.2.3: Direct Laser Acceleration

The potential advantages of structure-based direct laser accelerators have not been demonstrated. Opportunities for DLA testing with beam at the SLAC NLCTA would end in the near future. However, the ATF at BNL would provide beams with higher energy and better emittance. The CO_2 laser at the ATF represents a possible shift in approach for DLA to higher charges and should be considered, particularly given the recent success of IFEL research there.

9.3: Comments

For any of the approaches in advanced acceleration, the following facilities will likely be needed in order to make significant intermediate steps toward the eventual goal of a multi-TeV e^+e^- collider:

1. a flexible, dedicated R&D facility, with a witness beam and a number of drive beams, either laser or particle as appropriate to the approach, for staging experiments; and

2. a demonstration facility based upon the advanced acceleration approach, with beam characteristics scalable to future colliders.

The university and laboratory proponents of advanced accelerator approaches should be convened to develop concepts for a demonstration accelerator for discovery science with beam characteristics scalable to future colliders. Next-generation facilities should be encouraged, through cooperative funding from other parts of the Office of Science if appropriate, to examine the scientific possibilities opened by such initiatives.

9.3.1: PWFA

Progress in ultra-high-gradient PWFA research demands a next dedicated user facility that demonstrates advanced accelerator technology aimed at multi-TeV electron-position colliders. Special attention should be given to stageability, wall-plug efficiency, emittance preservation, beam stability and control and, eventually, positron acceleration. In that way, the U.S. will enhance its present, world-leading capability in this promising sector of advanced acceleration research. A technology with an overall real-estate gradient significantly above 100 MV/m and with excellent emittance and high wall-plug efficiency for a multi-TeV e^+e^- would be transformational. An initiative supporting high-gradient PWFA to the application stage could also open substantial opportunities for applications across the Office of Science and other Federal agencies.

The substantial R&D required for PWFA and related schemes needs a dedicated facility with reasonable and reliable access, sufficient space, flexibility, and capacity to demonstrate emittance preservation of very high brightness beams and to support the multiple beams needed to demonstrate PWFA stageability. It is desirable to have this facility be upgradable to allow full exploration of issues related to positron acceleration. It would also need to support related research such as GV/m DWFA. The facility should be operated long enough to complete the missions described above.

9.3.2: LWFA

The research relevant to future HEP accelerators that can be done with the world-leading 1-Hz laser lab at BELLA is impressive. It includes examination of beam brightness optimization (emittance, pulse length, charge yield) in the LWFA, and concomitant development of ultra-fast beam diagnostic techniques. By opening BELLA to outside user groups, the research being carried out there could be enhanced. This would strengthen the U.S. program in LWFA research; however, it would require added funding.

A 1-kHz facility such as the proposed 1-kHz upgrade of BELLA, k-BELLA, would be the next step once issues of beam stability and control, and stageability have been experimentally demonstrated. Development of this facility would also be predicated on advances in laser technology that may diminish the cost and complexity of suitable power sources. Design and construction of such a high repetition rate laser facility would enable critical progress towards meeting collider requirements in average beam power and efficiency. Given the promising performance of GeV-class beams extracted from LWFAs, LWFAs may be suitable for light sources.

9.3.3: DLA and related

Although DLA (both structure and IFEL) does not appear to be a viable approach for a multi-TeV collider, it has appeal for lower-energy applications. This technique may be supported by funds outside of GARD. The research to develop DLA structures and IFELs may be preserved through experimental efforts at the ATF, and at small university labs. The importance of theory, computations, and experimental activities to develop alternative approaches underlying advanced accelerator techniques should be recognized. Modest amounts of funding will be needed for these supporting activities.

9.4: FACET-II

FACET-II is the only next-generation R&D facility for PWFA that has been proposed at this point. Operation at high energy (10 GeV) uniquely permits continued access to > 1 GV/m gradient studies, and energies relevant to foreseen wakefield modules. FACET-II would utilize the middle third of the SLAC linac and employ a new photoinjector to produce electron beams with high energy, high charge, and short length. FACET-II would allow significant progress on much lower-emittance and lower energy spread electron beams in the context of very high acceleration gradients. It would eventually have a new small damping ring for positrons that would utilize the existing positron source and a "sailboat" chicane, which would allow adjustable separation of the drive electron and witness positron beams. FACET-II would enable beam matching and transport at the entrance/exit of a single module, but does not permit independent stages with drive beams. Initial staging experiments can be performed at the ATF and AWA facilities.

The cost of this project is substantial and cannot be accommodated within the current GARD budget. The operational costs of FACET-II, as presented, are projected to be at the level of those of FACET. Limitations in experimental space as presently designed challenge the fullest exploitation of the facility. Because the beamline of the LCLS-II FEL runs through the tunnel that would house FACET-II, the PWFA program may encounter operational conflicts with the BES photon science program that must be managed by SLAC. In addition, the middle third of the SLAC linac may be attractive for further LCLS energy upgrades, and the impact of this potential conflict must also be assessed to move forward. In order to address staging issues before a demonstration accelerator is proposed, a successor facility to FACET-II will be necessary to study staging for both electrons and positrons.

9.5: Recommendations

Even with some relaxation of the present, tight budget constraints, some consolidation into joint test facilities would be required. Under the most constrained funding scenario culling of the least promising approaches would be necessary. Such decisions by HEP would be informed by research activities of the several program elements as indicated in these recommendations.

Recommendation 7. Vigorously pursue particle-driven plasma wakefield acceleration of positrons at FACET in the time remaining for the operation of the facility. Between the closing of FACET and the operation of a follow-on facility, preserve the momentum of particle-driven wakefield acceleration research using other facilities.

Recommendation 8. Continue to support laser-driven plasma wakefield acceleration experiments on BELLA at the current level.

Recommendation 9. Reduce funding for direct laser acceleration research activities.

Recommendation 10. Convene the university and laboratory proponents of advanced acceleration concepts to develop R&D roadmaps with a series of milestones and common down-selection criteria towards the goal of constructing a multi-TeV e^+e^- collider.

9.6: Scenario C – Roadmap for a Multi-TeV e^+e^- Collider

The P5 report called for a roadmap for the U.S. to “move boldly toward development of transformational accelerator R&D … with an aggressive, sustained, and imaginative R&D program … changing the capability-cost curve of accelerators” in Scenario C. Motivated by the P5 science drivers, the goal is to “make these further-future accelerators technically and financially feasible on much shorter timescales.” Investment in the R&D necessary for the realization of P5’s strategic vision of a multi-TeV e^+e^- collider in the long-term, especially investment in R&D of advanced acceleration techniques, is one of two initial investments that the Subpanel identified for Scenario C.

Recommendation C1. Hasten the realization of the accelerator of P5’s medium-term vision for discovery: a very high-energy proton-proton collider, and the realization of the accelerator of P5’s long-term vision for discovery: a multi-TeV e^+e^- collider.

As explained in Section 4, the Subpanel envisions realizing this “fast-track” program by supplementing the base accelerator R&D program of Scenario A or B with a sequence of R&D projects directed along the path to a multi-TeV e^+e^- collider.

The path to a multi-TeV e^+e^- collider

A multi-TeV electron-positron collider will require novel advanced acceleration techniques, such as wakefield acceleration, with substantially higher gradients and improved power efficiency compared to current accelerators. Novel techniques currently under investigation could revolutionize electron and positron acceleration. Collider quality beams at high accelerator gradients would be a transformational development. Advanced techniques based on RF acceleration or on wakefield acceleration have the potential of achieving cost and performance that enable a multi-TeV e^+e^- collider, and they are synergistic with other possible applications.

Several possible approaches to provide the acceleration mechanisms for the electron and positron beams are currently being investigated. Plasma wakefield acceleration, driven by either laser or particle beams, anticipates the highest accelerating gradients. Dielectric wakefield accelerating structures and cooled high shunt impedance normal conducting cavities driven by efficient RF sources with energy recovery also offer high accelerating gradients. New results in new materials and preparation techniques of SRF cavities show promise for more than doubling the acceleration gradient of SRF linacs as well as operations at elevated temperatures, leading to significantly higher power efficiency. Progress on many of these acceleration techniques has been dramatic recently; nevertheless, extensive R&D remains in order to demonstrate collider quality beams, and technical breakthroughs will be needed.

To reach the goal of having a credible design for a multi-TeV e^+e^- collider, a number of R&D steps will be needed to determine the most promising acceleration technique and to further develop that technique for a practical collider:

1. Continue studies of candidate techniques on existing facilities. (See **Recommendations 7**, **8** & **11**.)
2. Convene the advanced acceleration community to develop R&D roadmaps for each candidate technique, with common milestones to the extent possible, and to define criteria to be used in the down-selection of techniques. (See **Recommendation 10**.)
3. Based on successful results of R&D on existing facilities, build next-generation R&D facilities for selected candidate technologies.

   a. The first next-generation R&D facility will be the successor to FACET for PWFA research. The need to move forward on this facility is immediate because of the impending closure of FACET.

   b. A next-generation R&D facility for LWFA research is likely to be the next new facility to be needed after the next-generation PWFA facility. It will have higher repetition rate than BELLA in order to begin to understand plasma lifetime issues.

4. Down-selection should occur as early as possible after an adequate basis for the selection exists. The two facilities above are likely to be needed before the down-selection. Down-selection to a single technique is desirable; however, an initial down-selection leaving two techniques may also be done.

5. Next-to-next-generation R&D facilities may be needed by one or more techniques before down-selection. For instance, if the currently proposed FACET-II is constructed as the next-generation R&D facility for PWFA, a successor facility will be needed to study staging of several plasma channels. Emittance preservation is the key concern in matching from one channel to the next.

6. After down-selection to a single technique, and when enough R&D has been performed that the technique can be developed for a multi-TeV collider, a demonstration facility based upon the selected acceleration technique should be constructed in order to demonstrate the technology on a scale that gives the confidence that further scaling can be done to the multi-TeV scale of the e^+e^- collider. This demonstration facility could perhaps be designed for an application for discovery science, for instance as a driver for an x-ray laser. The demonstration facility should have beam characteristics scalable to future colliders.

7. The demonstration facility is the last step in the R&D program. Following successful demonstration, one can then embark on the full technical design of a multi-TeV e^+e^- collider.

For particle-driven plasma wakefield acceleration (PWFA), the roadmap leads through at least one new R&D facility. The imminent closure of the FACET facility creates an urgent need to develop, construct, and operate a next-generation R&D facility for PWFA. Because of the immediate need, the Subpanel recommends such an R&D facility.

Recommendation C1b. Develop, construct, and operate a next-generation facility for particle-driven plasma wakefield acceleration research and development, targeting a multi-TeV e^+e^- collider, in order to sustain this promising and synergistic line of research after the closure of the FACET facility.

In the area of laser-driven plasma wakefield acceleration (LWFA), the BELLA facility at LBNL is currently the leading R&D facility. As the multi-year research program at BELLA winds down in the coming decade, a follow-on facility will be needed to continue with the subsequent steps of LWFA research if this acceleration approach remains promising for a multi-TeV collider. These subsequent steps will include tests with the high repetition rate relevant to colliders (~kHz) and will include studies guided by the results of the research program at BELLA. As noted above, this follow-on, or next-generation, R&D facility will likely be needed prior to the down-selection of acceleration techniques.

Dielectric wakefield acceleration also has the potential to achieve accelerating gradients in the GV/m range.

For normal conducting RF (NCRF), the roadmap leads to a multi-stage prototype accelerator (See **Recommendation 13**.). After successful testing of this prototype, construction and operation of a multi-GeV demonstration accelerator based on very high-gradient NCRF technology should be considered. Unless this demonstrator has an additional application, such as in a PWFA R&D facility, its construction should await the down-selection of acceleration technique.

For superconducting RF (SRF), the roadmap calls for the development of new high-gradient SRF cavities, followed by demonstration in linac structures. Based on the success of these developments, the roadmap would then lead to a demonstration accelerator, if SRF technology is not being applied to an upgrade of the ILC.

Scenario C funding is necessary to move forward with the future R&D facilities necessary for research in plasma wakefield acceleration. It also enables advanced acceleration R&D projects to develop the promising acceleration approaches to the appropriate level of maturity for the down-selection of technique. Scenario C allows pursuit of promising acceleration techniques in parallel, increasing the likelihood and frequency of technical breakthroughs. Scenario C funding will as well allow down-selection at an earlier time than would otherwise be possible, and it will make a multi-TeV e^+e^- collider technically feasible, and at lower cost, on an earlier time scale than could otherwise be expected. By following this path, the U.S. can continue to be a world leader in advanced acceleration for particle physics, and for synergistic applications; additionally, the U.S. can be a major partner in the design and development of a multi-TeV e^+e^- collider as a future global project for experimental particle physics.

10: Facilities in Support of Accelerator R&D

The GARD program relies on the continued support of U.S. test facilities that are the root of next-generation accelerators, generate science on the cover of Nature and Science, and train the next generation of accelerator physicists and technologists. As noted previously, the cost of operating these facilities is a major component (42%) of the current GARD budget. The origin of these test facilities range from legacy infrastructure built for previous construction projects, such as the present LHC quadrupoles, to unforeseen opportunities such as the 2009 ARRA funding period that provided the funding for the construction of BELLA, FACET, and most of the SRF facilities at Fermilab. The ongoing and future HEP construction projects such as Mu2e and HL-LHC magnet construction will likewise contribute to the pool of test stands that will propel accelerator R&D forward.

Facilities such as FACET and ATF (which is supported by the Accelerator Stewardship program) serve thriving accelerator research user communities. Other facilities important to GARD are also critical and enabling to construction projects broadly in the Office of Science, such as the LCLS-II project, and projects and programs hosted in HEP. These construction projects are under constant pressure to simultaneously minimize technical risk and cost and are the basis for a HEP future. The number of construction projects is presently not large and not sufficiently sequenced, and may never be, to minimize fluctuations in aggregate project funding to support continuous operation of these facilities critical to GARD research. Construction projects do however have a history and in all likelihood a clear future of contributing legacy infrastructure that will evolve into future test facilities.

Funding for GARD facilities in large part today is playing the role of “technical overhead” for accelerator research and development that is not captured in current models of overhead charged to construction projects and operations today. Communicating the enabling and critical role that GARD facilities provide to the research community and broadening the user base can contribute to stabilizing and sustained growth of the GARD facility budget.

11: Summary

The GARD program has supported a variety of accelerator science and accelerator development programs that have been very productive. It is world leading and with continued support it will remain that way. Continued support at least at the Scenario A level is essential. There are opportunities that can be realized with additional funding that will speed the realization of the next accelerators needed to keep the field of accelerator based particle physics vital and exciting.

To guide the R&D needed for the Next Steps and Further Future accelerators, the particle physics community has to come together and agree on the physics parameters for the research programs on these accelerators. The agreement on the physics parameters needs to occur early in the conceptual design of these accelerators to effectively guide the R&D needed to realize them in a cost effective manner. The required luminosity for both the very high-energy proton-proton collider and for the multi-TeV e^+e^- collider is a key cost driver for these accelerators. The synchrotron radiation load on the vacuum system for a very high-energy proton-proton collider is significantly higher than in the LHC and can impact the magnet design. The size and operating energy of the accelerators also directly determine their operating cost and the reliability in addition to the construction cost.

Scenario C funding would enable the U.S. accelerator R&D program to "move boldly toward development of transformational accelerator R&D … with an aggressive, sustained, and imaginative R&D program", as called for by the P5 strategic plan. Funding would be directed towards and would consolidate R&D areas in which the U.S. already has significant strengths and leadership positions. With this additional funding, the U.S. could maintain its traditional leadership in accelerator R&D. The R&D projects chosen would significantly enhance the state-of-the-art; consequently, they can be expected to generate exciting results that will draw new practitioners into the accelerator R&D enterprise, and be applied across the Office of Science. Scenario C funding would energize a vibrant accelerator-based U.S. particle physics program.

Appendix A: Charge

U.S. Department of Energy
and the
National Science Foundation

JUN 10 2014

Professor Andrew Lankford
Chair, HEPAP
University of California at Irvine
Physics & Astronomy Department
4129H Frederick Reines Hall
Irvine, CA 92697

Dear Professor Lankford:

We are writing to ask you to conduct an assessment of the accelerator R&D effort within the Department of Energy (DOE) Office of High Energy Physics (HEP).

Particle accelerators have long been a critical, enabling technology for high-energy physics and have become a key element for advances in many other fields of science. The accelerator R&D effort within the DOE HEP is the major source of U.S. funding for the development of accelerators, both to meet the needs of new accelerator facilities for scientific discovery and to pursue novel acceleration concepts and technologies for broader uses. The portfolio of projects supported by this effort[1] includes research activities in accelerator science, accelerator technology and materials, provision of test facilities, simulation work, and training of accelerator physicists. It is carried out in universities and several federally funded national laboratories. The total annual accelerator R&D budget in the FY 2015 budget request for DOE-HEP is $91M, including $26M for the HEP Directed Accelerator R&D ($14M for LARP—the LHC Accelerator Research Program, and $12M for MAP—the Muon Accelerator Program), and exclusive of the Office of Science (SC) Accelerator R&D Stewardship program.

Accelerator R&D can be partitioned roughly into three categories: short-term research, required for optimization of operating facilities or approved new facilities; medium-term research, to bring new concepts to practice so that they can be considered for the design of a new facility; and long-term, exploratory research aimed at developing new concepts for acceleration, new technologies, new materials, and advanced simulation techniques. The training of accelerator physicists, engineers, and technologists is an additional important goal.

[1] Results from the HEP Accelerator R&D program have been highly influential in developments for accelerators used for nuclear physics, materials science, biology, medical diagnostics and treatment, and for industrial uses. In recognition of this. DOE-HEP has recently been designated by the Office of Science (SC) to oversee, in close consultation with other SC programs, long-term accelerator R&D stewardship activities within SC, including for accelerators critical to applications in areas beyond SC. Note, however, that the SC Accelerator R&D Stewardship program is *not* included as part of this assessment.

The recent High Energy Physics Advisory Panel (HEPAP) Particle Physics Project Prioritization Panel ("P5") report has highlighted the importance of accelerator based experiments for the future of particle physics and this places renewed emphasis on accelerator R&D efforts in support of medium- and long-term high energy physics projects. In light of this, we are requesting that HEPAP set up a subpanel to examine the research in the current HEP accelerator R&D program and to identify the most promising research areas to support the advancement of high energy and particle physics. The subpanel should consider:

- *National Goals*: Describe in broad terms appropriate goals for medium- and long-term U.S. accelerator R&D that are, in the subpanel's view, required for a world-leading future program in accelerator-based particle physics consistent with the scientific priorities for DOE-HEP described in the HEPAP-P5 report for Scenarios A and B.
- *Current Effort*: Examine the scope of the current medium- and long-range R&D efforts and evaluate how well these address the HEP mission, as expressed in the HEPAP-P5 report, and the goals articulated in response to the first bullet.
- *Impediments*: Describe any impediments that may exist for achieving these goals including, but not limited to, considerations of resources, management of research efforts, and existing and expected expertise and infrastructure.
- *Training*: Accelerator R&D efforts play a major role in the training of future accelerator scientists and technologists. Assess whether this aspect is adequately addressed in the current programs, including partnerships between national laboratories and universities, and opportunities to enhance the training efforts to meet future needs for such skilled personnel.
- *Balance*: Advise the DOE-HEP program on how to maintain a healthy and appropriately balanced national program for medium- and long-term accelerator R&D, including test facilities, in light of the budget envelopes for Scenarios A and B developed by the HEPAP-P5 panel. Provide further guidance for a plan based on the science and technology case for increased investment in the HEP Accelerator R&D program called for in P5's Scenario C. We would be particularly interested to know how partnerships between universities, national laboratories and international collaborators could be most effective in achieving the goals.

We will explain the distinction and interplay between the HEP Accelerator R&D program and the SC Accelerator R&D Stewardship program at the outset of the assessment. We welcome the subpanel's comments on potential synergies or conflicts between the two programs.

It is requested that preliminary findings of your report should be presented to HEPAP by the end of November 2014, with a final version by March 2015.

3

We thank you for your help in conducting this strategic assessment; the advice of this HEPAP subpanel will be very important to our program planning. We look forward to working with you in this endeavor.

Sincerely,

Patricia M. Dehmer
Acting Director, Office of Science
U.S. Department of Energy

Dr. F. Fleming Crim
Assistant Director
Directorate for Mathematical and Physical Sciences
National Science Foundation

Appendix B: Panel Members

Bill Barletta
Fermi National Accelerator Laboratory and
Massachusetts Institute of Technology

Ilan Ben-Zvi
Brookhaven National Laboratory and
State University of New York at Stony Brook

Marty Breidenbach
SLAC National Accelerator Laboratory

Oliver Bruning
European Organization for Nuclear Research (CERN)

Bruce Carlsten
Los Alamos National Laboratory

Roger Dixon
Fermi National Accelerator Laboratory

Steve Gourlay
Lawrence Berkeley National Laboratory

Don Hartill***, chair***
Cornell University

Georg Hoffstaetter
Cornell University

Zhirong Huang (BES)
SLAC National Accelerator Laboratory

Young-Kee Kim
University of Chicago

Tadashi Koseki
High Energy Accelerator Research Organization (KEK) and
Japan Proton Accelerator Research Complex (J-PARC)

Geoff Krafft (NP)
Thomas Jefferson National Accelerator Facility

Andy Lankford***, ex officio***
University of California, Irvine

Lia Merminga
TRIUMF

Jamie Rosenzweig
University of California, Los Angeles

Mike Syphers
Michigan State University

Bob Tschirhart
Fermi National Accelerator Laboratory

Rik Yoshida
Argonne National Laboratory

Appendix C: Process and Meetings

The Accelerator Research and Development Subpanel process had several components, designed with particle accelerator community engagement in mind:

- A website was maintained, with information, news, meeting information, and a submissions portal with a public archive:
  http://www.usparticlephysics.org/p5/ards

- Three public meetings were held, whose agendas are appended. All talks are posted online.

- Each public meeting included a town hall session.

The panel worked by consensus. There were full-panel phone calls approximately weekly throughout the process. The panel had additional face-to-face meetings on the following dates: July 6–7, November 6–7, December 2–3, January 9–10, and February 27 to March 1. At most meetings, there were sessions without agency personnel in the room.

There were HEPAP presentations and discussions in September 2014, December 2014, and April 2015. Status reports were given at the September and December meetings, and the Report was presented for approval at the April 2015 HEPAP meeting.

The recommendations contained in this report, after adoption by HEPAP, are advisory input to the Department of Energy and the National Science Foundation. The actual design and implementation of any plan in these agencies is the responsibility of program management.

AAAS Science & Technology Policy Fellow Michael Cooke performed the design and typography of this Report. Cover illustration produced by Sandbox Studio, Chicago, and Anastasia Kozhevnikova.

HEPAP Accelerator R&D Subpanel Meeting #1

Brookhaven National Laboratory, Aug. 25–26, 2014
http://indico.fnal.gov/event/HEPAP_Subpanel_BNL

Introduction
Michael Blaskiewicz
Brookhaven National Laboratory

Accelerator Science and Technology
Igor Pogorelsky
Brookhaven National Laboratory

Sergey Belomestnykh
Brookhaven National Laboratory and
State University of New York at Stony Brook

Transformational R&D for a 100 TeV scale *pp* colliders
Robert B Palmer
Brookhaven National Laboratory

Soren Prestemon
Lawrence Berkeley National Laboratory

Vladimir Shiltsev
Fermi National Accelerator Laboratory

LARP
Giorgio Apollinari
Fermi National Accelerator Laboratory

University program
Ralf Eichhorn
Cornell University

World Program for Colliders
Oliver Bruning
European Organization for Nuclear Research (CERN)

World Program for Intensity Frontier
Tadashi Koseki
High Energy Accelerator Research Organization (KEK) and
Japan Proton Accelerator Research Complex (J-PARC)

HEPAP Accelerator R&D Subpanel Meeting #2

Fermi National Accelerator Laboratory, Aug. 27–28, 2014
http://indico.fnal.gov/event/HEPAP_Subpanel_FNAL

FNAL GARD Past and New Proposals
Sergei Nagaitsev
Fermi National Accelerator Laboratory

PIP-II and the Future of Protons at Fermilab
Paul Derwent
Fermi National Accelerator Laboratory

Cold Muon Source and MTA R&D
Mark Palmer
Fermi National Accelerator Laboratory

Vision for U.S. Modeling and Design Tools for Long Term Accelerator R&D
Panagiotis Spentzouris
Fermi National Accelerator Laboratory

Vision for U.S. long term SRF R&D
Alexander Romanenko
Fermi National Accelerator Laboratory

Hasan Padamsee
Fermi National Accelerator Laboratory

Transformational Accelerator R&D Program to Enable Multi-MW Beams for U.S. HEP
Alexander Valishev
Fermi National Accelerator Laboratory

University Contributions to Long Term Accelerator R&D
Sarah Cousineau
Oak Ridge National Laboratory

Vision for U.S. High Power Targetry R&D Program for HEP
Patrick Hurh
Fermi National Accelerator Laboratory

Challenges for High-Intensity Proton Accelerators and Long-Term Experimental R&D
Bob Zwaska
Fermi National Accelerator Laboratory

Overview of Advanced Accelerator R&D at ANL HEP
John Power
Argonne National Laboratory

USPAS, PhD program, Midwest University-FNAL Consortium
Swapan Chattopadhyay
Fermi National Accelerator Laboratory and Northern Illinois University

HEPAP Accelerator R&D Subpanel Meeting #3

SLAC National Accelerator Laboratory, Aug. 29–30, 2014
http://indico.fnal.gov/event/HEPAP_Subpanel_SLAC

Welcome
Norbert Holtkamp
SLAC National Accelerator Laboratory and Stanford University

SRF for ILC and LCLS-II
Marc Ross
SLAC National Accelerator Laboratory

Accelerator R&D in Europe
Ralph Assmann
German Electron Synchrotron (DESY)

Accelerator Physics Issues with Wakefield Accelerators
Sergei Nagaitsev
Fermi National Accelerator Laboratory

Overview of Accelerator R&D at LBNL
Wim Leemans
Lawrence Berkeley National Laboratory

Beam Instrumentation
John Byrd
Lawrence Berkeley National Laboratory

Laser Plasma Accelerator
Eric Esarey
Lawrence Berkeley National Laboratory

LPA based Collider Roadmap
Wim Leemans
Lawrence Berkeley National Laboratory

University based Advanced Accelerator R&D
Mike Downer
University of Texas at Austin

Accelerator R&D at SLAC
Robert Hettel
SLAC National Accelerator Laboratory

FACET and Test Facilities
Vitaly Yakimenko
SLAC National Accelerator Laboratory

PWFA Science at FACET
Mark Hogan
SLAC National Accelerator Laboratory

PWFA R&D at Universities
Chan Joshi
University of California, Los Angeles

R&D for TeV Scale e^+e^- Collider at ANL
Chunguang Jing
Argonne National Laboratory

Novel Approaches to RF Acceleration and Sources
Sami Tantawi
SLAC National Accelerator Laboratory and Stanford University

Direct Laser Acceleration
Joel England
SLAC National Accelerator Laboratory

Appendix D: Full List of Recommendations

Recommendation 1. Fund generic high-power component R&D at a level necessary to carry out needed thermal shock studies and ionizing radiation damage studies on candidate materials that are not covered by project-directed research. (p. 9, 19)

Recommendation 2. Construct the IOTA ring, and conduct experimental studies of high-current beam dynamics in integrable non-linear focusing systems. (p. 9, 18)

Recommendation 3. Support a collaborative framework among laboratories and universities that assures sufficient support in beam simulations and in beam instrumentation to address beam and particle stability including strong space charge forces. (p. 9, 17)

Recommendation 4. Direct appropriate investment in superconducting RF R&D in order to inform the selection of the acceleration technology for the multi-MW proton beam at Fermilab. (p. 9, 22)

Recommendation 5. Participate in international design studies for a very high-energy proton-proton collider in order to realize this Next Step in hadron collider facilities for exploration of the Energy Frontier. Vigorously pursue major cost reductions by investing in magnet development and in the most promising superconducting materials, targeting potential breakthroughs in cost-performance. (p. 10, 25)

Recommendation 5a. Support accelerator design and simulation activities that guide and are informed by the superconducting magnet R&D program for a very high-energy proton-proton collider. (p. 10, 25)

Recommendation 5b. Form a focused U.S. high-field magnet R&D collaboration that is coordinated with global design studies for a very high-energy proton-proton collider. The over-arching goal is a large improvement in cost-performance. (p. 10, 25)

Recommendation 5c. Aggressively pursue the development of Nb_3Sn magnets suitable for use in a very high-energy proton-proton collider. (p. 10, 25)

Recommendation 5d. Establish and execute a high-temperature superconducting (HTS) material and magnet development plan with appropriate milestones to demonstrate the feasibility of cost-effective accelerator magnets using HTS. (p. 10, 25)

Recommendation 5e. Engage industry and manufacturing engineering disciplines to explore techniques to both decrease the touch labor and increase the overall reliability of next-generation superconducting accelerator magnets. (p. 10, 25)

Recommendation 5f. Significantly increase funding for superconducting accelerator magnet R&D in order to support aggressive development of new conductor and magnet technologies. (p. 10, 25)

Recommendation 6. Increase funding for development of superconducting RF (SRF) technology with the goal to significantly reduce the cost of a ~1 TeV energy upgrade of the ILC. Strive to achieve 80 MV/m accelerating gradients with new SRF materials on the 10-year timescale. (p. 11, 22)

Recommendation 7. Vigorously pursue particle-driven plasma wakefield acceleration of positrons at FACET in the time remaining for the operation of the facility. Between the closing of FACET and the operation of a follow-on facility, preserve the momentum of particle-driven wakefield acceleration research using other facilities. (p. 11, 32)

Recommendation 8. Continue to support laser-driven plasma wakefield acceleration experiments on BELLA at the current level. (p. 11, 32)

Recommendation 9. Reduce funding for direct laser acceleration research activities. (p. 11, 32)

Recommendation 10. Convene the university and laboratory proponents of advanced acceleration concepts to develop R&D roadmaps with a series of milestones and common down-selection criteria towards the goal of constructing a multi-TeV e^+e^- collider. (p. 11, 32)

Recommendation 11. Continue research on high-efficiency power sources and high-gradient normal conducting RF structures. (p. 12, 21)

Recommendation 12. Make NLCTA available for RF structure tests using its RF power and beam sources. (p. 12, 21)

Recommendation 13. Focus normal conducting RF R&D on developing a multistage prototype based on high-gradient normal conducting RF structures and high-efficiency RF power sources to demonstrate the effectiveness of the technology for a multi-TeV e^+e^- collider. (p. 12, 21)

Recommendation 14. Continue accelerator and beam physics activities and beam instrumentation and control R&D aimed at developing the accelerators defined in the Next Steps and the Further Future Goals. Develop coordination strategies, both nationally and internationally, to carry out these studies in an efficient manner. (p. 12, 16)

Recommendation 15. To ensure a healthy, broad program in accelerator research, allocate a fraction of the budget of the Accelerator Physics and Technology thrust to pursue fundamental accelerator research outside of the specific goals of the Next Steps and Further Future Goals. Research activities at universities should play a particularly important role. (p. 12, 16)

Recommendation B1. Increase base GARD funding modestly in order to open numerous critical R&D opportunities that do not fit in the current base, as well as to invigorate fundamental accelerator science research, and to step up development of the national accelerator workforce. (p. 13)

Recommendation C1. Hasten the realization of the accelerator of P5's medium-term vision for discovery: a very high-energy proton-proton collider, and the realization of the accelerator of P5's long-term vision for discovery: a multi-TeV e^+e^- collider. (p. 14, 26, 32)

Recommendation C1a. Ramp up research and development of superconducting magnets, targeted primarily for a very high-energy proton-proton collider, to a level that permits a multi-faceted program to explore possible avenues of breakthrough in parallel. Investigate additional magnet configurations, fabricate multi-meter prototypes, and explore low-cost manufacturing techniques and industrial scale-up of conductors. Increase support for high-temperature superconducting (HTS) materials and magnet development to demonstrate the viability of accelerator-quality HTS magnets for a very high-energy proton-proton collider. (p. 14, 26)

Recommendation C1b. Develop, construct, and operate a next-generation facility for particle-driven plasma wakefield acceleration research and development, targeting a multi-TeV e^+e^- collider, in order to sustain this promising and synergistic line of research after the closure of the FACET facility. (p. 14, 33)

HEPAP Accelerator R&D Subpanel Report Transmittal Letter

UNIVERSITY OF CALIFORNIA

BERKELEY • DAVIS • IRVINE • LOS ANGELES • MERCED • RIVERSIDE • SAN DIEGO • SAN FRANCISCO SANTA BARBARA • SANTA CRUZ

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Phone (949) 824-6911
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May 18, 2015

Dr. Patricia M. Dehmer
Acting Director, Office of Science
U.S. Department of Energy

Dr. F. Fleming Crim
Assistant Director
Directorate for Mathematical and Physical Sciences
National Science Foundation

Dear Dr. Dehmer and Dr. Crim:

The Report of the HEPAP Accelerator R&D Subpanel, "Accelerating Discovery: A Strategic Plan for Accelerator R&D in the U.S.", was presented to HEPAP at its meeting on April 6, 2015. This report addresses the request in your letter of June 10, 2014 "that HEPAP set up a subpanel to examine the research in the current HEP accelerator R&D program and to identify the most promising research areas to support the advancement of high energy and particle physics." At the HEPAP meeting, Subpanel Chair Don Hartill reviewed the report and its recommendations, and responded to questions from HEPAP members. Following discussion and deliberation, HEPAP approved the report without objection. HEPAP also separately approved each of the report's recommendations.

A healthy accelerator R&D program both depends upon a healthy accelerator science workforce and contributes to the training of scientists and engineers for the workforce. The Subpanel strongly concurs with reports of other recent HEPAP subpanels that have emphasized the need to recognize and support accelerator science as a *bona fide* academic discipline. For this reason, the Subpanel took special note of the new program in Accelerator Science in the NSF Division of Physics as a significant step. The recent Request for Information "Strengthening U.S. Academic Program in Accelerator Science" from the DOE Office of High Energy Physics is also a positive indication of the importance of accelerator science.

I would like to thank Professor Hartill and all the members of the Subpanel for their dedication in performing this study and producing this report.

With this letter, on behalf of HEPAP, we respectfully submit for your consideration the final report of the Accelerator R&D Subpanel.

Sincerely yours,

Donald L. Hartill
Chair, HEPAP Accelerator R&D Subpanel

Andrew J. Lankford
Chair, High Energy Physics Advisory Panel

Conflict of Interest Resolution During HEPAP Approval Process

In promulgating this report to the Department of Energy and the National Science Foundation, HEPAP recognizes that there are particular recommendations that could affect the interests of several organizations and facilities engaged in particle physics research. We further recognize that some of the members of HEPAP are members of those identified organizations or work at those facilities and have interests which could be affected by the recommendations that are being forwarded. Prior to the review of and voting on this report, and in accordance with advice from the DOE General Counsel's office, those individuals have been identified and recused from participating in discussions associated with their home organizations or from voting on recommendations associated with those institutions, as follows:

For Recommendations 1 and 2, Prof. Gerber and Dr. Tschirhart did not participate in discussions or voting on these recommendations;

For Recommendation 4, Prof. Carlstrom, Prof. Gerber, Prof. Hofstaetter, and Dr. Tschirhart did not participate in discussions or voting on this recommendation;

For Recommendation 5f, Dr. Ben-Zvi, Dr. Bishai, Prof. Gerber, Dr. Ligeti, Prof. Murayama, and Dr. Tschirhart did not participate in discussions or voting on this recommendation;

For Recommendation 6, Prof. Carlstrom, Prof. Gerber, Prof. Hofstaetter, and Dr. Tschirhart did not participate in discussions or voting on this recommendation;

For Recommendation 7, Prof. Shutt and Prof. Wechsler did not participate in discussions or voting on this recommendation;

For Recommendation 8, Dr. Ligeti and Prof. Murayama did not participate in discussions or voting on this recommendation;

For Recommendations 9, 11, and 12, Prof. Shutt and Prof. Wechsler did not participate in discussions or voting on these recommendations;

For the proposed FACET-II facility in the context of recommendations in Scenario A, Prof. Shutt and Prof. Wechsler did not participate in discussions or voting on these recommendations;

For Recommendation C1a, Dr. Ben-Zvi, Dr. Bishai, Prof. Gerber, Dr. Ligeti, Prof. Murayama, and Dr. Tschirhart did not participate in discussions or voting on this recommendation;

For Recommendation C1b, Prof. Shutt and Prof. Wechsler did not participate in discussions or voting on this recommendation.