Division of Research and Technology
Office of High Energy Physics
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Report from the Comparative Review of Intensity Frontier Projects

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1 Executive Summary

The DOE High Energy Physics (HEP) program has a strategic plan to accomplish the long-term research goals articulated by its federal advisory committee, the High Energy Physics Advisory Panel (HEPAP). In particular, the recent report of the HEPAP subpanel on scientific priorities and project planning for particle physics (a.k.a. "P5") described a scientific "roadmap" for particle physics over the next decade in three major thrust areas, dubbed the Energy Frontier, the Intensity Frontier and the Cosmic Frontier.

HEP is in the process of executing the P5 plan at all three frontiers. In the near-term, P5 called for new investments at the Intensity Frontier to take advantage of unique domestic qualities and capabilities that could ensure a U.S. leadership role in this area for the coming decades. In addition to identifying scientific opportunities, P5 considered and prioritized several possible new projects in different funding scenarios. In one of the mid-range funding scenarios (a constant level-of-effort at the FY2007 budget level, referred to as "scenario B"), P5 called for modest U.S. participation in an overseas Intensity Frontier project, called a "Super-B factory," that will significantly extend the search for new physics, in addition to a proposed suite of domestic projects that would be phased in over the next decade. The actual funding projection for DOE HEP is currently somewhat below what was envisioned in P5's scenario B, and two collaborative proposals have been submitted to DOE to implement this P5 recommendation. At about the same time, DOE also received a revised proposal for a different Intensity Frontier experiment called "g-2" that would be sited at Fermilab and require about the same level of investment as US participation in the international projects.

All three proposals were sent out for mail review, but the results were inconclusive, as all three proposals rated highly and reviewers recommended proceeding with them. To help decide between these competing proposals in a constrained budget environment, HEP convened a comparative review panel to prioritize the three proposals.

The clear recommendation from the panel was to fund both g-2 and U.S. participation in the Japanese Super-B proposal if possible. The Italian Super-B proposal was not recommended for funding.

2 Introduction

The mission of the DOE Office High Energy Physics (HEP) is to understand how our universe works at its most fundamental level. To pursue that research program, The High Energy Physics Advisory Panel (HEPAP), responding to a charge from the DOE and the NSF, has endorsed a long-range vision for the future of the program as recommended by the Particle Physics Project Prioritization Panel (P5), which studied the scientific facilities and experiments that could be implemented over the next decade under several budget scenarios. The panel heard presentations from many of the proposed new projects and made prioritized recommendations. The P5 report[1] outlines a balanced US HEP research program at various funding levels, with additional capabilities for scientific discovery available if resources can be provided.

The P5 plan revolves around three main scientific thrusts, denoted as the Energy Frontier, the Intensity Frontier and the Cosmic Frontier. One of the main recommendations of the P5 report was for the US to implement a world-leading program at the Intensity Frontier, building on infrastructure at Fermilab and elsewhere. The scope of the Intensity Frontier program recommended by P5 depends on the funding scenario assumed. In the intermediate funding scenario (scenario B) P5 recommended:

The intermediate budget scenario, scenario B, would allow pursuing significant participation in one overseas next-generation B factory.

The P5 panel also found that another project, the proposed g-2 experiment at the Japanese proton accelerator J-PARC, was not compelling enough to warrant support due to overall cost. In 2010 the DOE HEP office received separate proposals for US participation in both the Japanese and Italian next-generation B-factory projects; and a revised proposal to mount the g-2 experiment at Fermilab. These proposals have estimated costs ranging from $10M to $50M. The more expensive proposals have options for scope reductions.

Since the P5 report was issued in 2008 there have been new developments for all three of these proposed new experiments. Details are given in the following sections. As part of the FY2012 budget development process, HEP has been considering various out-year budget profiles that are broadly consistent with some of the P5 scenarios. Implementing one or more new Intensity Frontier projects on the timescale advocated by the proponents would require decisions to be made before the conclusion of the FY2012 budget development, late in calendar 2010. The DOE HEP office decided, given the changes in circumstance since the P5 report, to convene an independent review panel to reconsider the two Super-B proposals and the revised g-2 proposal and make prioritized recommendations about which experiment(s) to pursue if funding permitted. In addition, the proposals received were sent out for mail review following standard merit review procedures. The HEP office considered soliciting other proposals

[1] Available at http://www.science.doe.gov/hep/files/pdfs/P5_Report%2006022008.pdf

(for example, on rare K decays) but the concluded there were no other proposals considered by P5 that would be consistent with the potential funding available for new Intensity Frontier projects.

The Intensity Frontier review panel met on August 10-12, 2010 in Bethesda, Maryland. The goal of the review was to assess: the quality of the recent scientific performance by the collaborations proposing these new activities; the merit and feasibility of the proposed research for achieving the scientific goals of the field; and the relevance of their research efforts to the overall HEP mission. The review guidance, distributed to the review panel and to the PIs, can be found in Appendix I.

The review panel consisted of seven experts in High Energy Physics drawn from U.S. universities and laboratories (see Appendix II for panel membership). No reviewers from universities or laboratories who were collaborating on the new proposals were used. The review was organized and chaired by DOE HEP with help from both the DOE and NSF Nuclear Physics programs who participated as observers. The PIs submitted proposals three weeks in advance of the review, adhering to standard HEP proposal guidelines. The proposing collaborations were asked to make presentations to the panel and funding agency observers that addressed the review charge, leaving ample time for discussion. The review agenda can be found in Appendix III.

This report, assembled by DOE from individual letters submitted by each of the external reviewers (both mail reviewers and panelists), summarizes the findings, observations and recommendations of the reviewers. The observations and recommendations of the mail reviewers are based only on the submitted proposals (some reviewers evaluated more than one proposal). The observations and recommendations of the panelists are based on the presentations by the collaborations, on the material provided prior to the review, and on the discussions and answers to questions posed by the panel members during the review. The committee had extensive discussions during executive sessions in the presence of agency officials. A draft of each panelist's initial evaluation was presented to DOE at a close-out presentation. In addition, committee members were asked to provide an ordered ranking of the three proposals. A summary of these rankings can be found in Section 4 of this Report.

While this report does not contain every single view expressed by the panel members, it does convey the thrust and diversity of opinions expressed by the panel on the most important issues identified.

3 The Proposals

This section briefly reviews the scientific background for the three Intensity Frontier proposals submitted to DOE and summarizes the substantive elements of the proposals as presented at the Intensity Frontier review.

i. CP Violation and Super B-factories

The Big Bang should have produced equal amounts of both matter and antimatter, yet there appears to be very little antimatter in the present-day universe. In order to create such an imbalance there must be an underlying physics process that favors matter over antimatter. Such processes have been observed in a few fundamental particle interactions as subtle asymmetries in the weak nuclear force. This phenomenon is called "CP Violation" by particle physicists. These processes have been studied extensively since their discovery in the 1960's in the decays of so-called "K" mesons in part because of the light they can shed on how the present day matter-antimatter asymmetry arose.

In the intervening decades, experiments showed that the asymmetry observed in K meson decays was far too small to account for the present-day matter-asymmetry. Within the context of the Standard Model of particle physics, these same asymmetries were predicted to be observable in the decays of the heavier "B" mesons that were first discovered in accelerators in the 1980's, if one could produce enough of them. In addition, there might be new physics processes not accounted for in the Standard Model which increased the matter-antimatter effect in B meson decays and could explain the matter-antimatter asymmetry in the universe.

In the mid-1990's both the US and Japan built high-intensity electron-positron colliders known as "B-factories" to address this question. These accelerators achieved luminosities approximately 100 times better than their predecessors and made many measurements of B meson decay modes to test the Standard Model picture. These measurements wound up confirming the Standard Model picture of CP Violation with high precision and were cited in the 2008 Nobel Prize in physics, awarded in part to the two Japanese theorists who developed this theoretical framework. While this was another triumph for the Standard Model, it also ruled out another possible new source of matter-antimatter asymmetry.

The matter-antimatter asymmetry in the universe still motivates searches for new sources of CP Violation and a continued attack on the Intensity Frontier to try to find them. Research groups in Europe, Japan and the US have proposed "super-B" factories (sometimes called "flavor factories"), challenging new accelerators that are designed to increase the luminosity of the existing B-factories by a factor of 50 to 100. The greatly increased data rate will allow much more sensitive searches for new physics, and will complement LHC discoveries by measuring the indirect impact of (say) new heavy particles on B meson decays, helping to clarify the nature of the new physics observed at the LHC. One area of particular interest, unique to the Super-B factories, are the rare decays of the heavy "tau" lepton (also produced copiously in electron-positron collisions) which may open a window to understanding a different, but equally important phenomenon, lepton flavor violation – observed in neutrinos, but not yet in other particles – in the same way the B-factories advanced the study of CP Violation.

In 2008 the HEPAP P5 panel evaluated the physics case for US involvement in proposed overseas "super-B" factories, two of which are considered further in this review. P5 recommended:

The physics reach of a super flavor factory is well motivated and grounded in the very rich suite of measurements produced by the current generation of B factories. Two offshore super flavor factory initiatives are now being developed with US involvement. A modest level of R&D should continue toward a goal of supporting an informed consideration of any significant US investment in a super flavor factory. The maturity of the field of B physics supports a strategy of significant US investment in a single next-generation overseas facility.

The intermediate budget scenario under which P5 made this recommendation is somewhat above the FY2011 President's Budget Request for HEP, and projected funds available for new projects in future years are extremely limited. Therefore the HEP office asked an independent panel for its evaluation of the specific proposals received.

ii. The BELLE-II Proposal

An international collaboration of about 300 physicists from 13 countries has proposed an upgrade to the existing Japanese B-factory accelerator (KEK-B) and its associated detector (BELLE). The upgraded facilities are referred to as SuperKEK-B and BELLE II, respectively. The upgrade to KEK-B involves increasing beam currents (and associated RF acceleration to accommodate those currents) and reducing and manipulating the beam size to achieve a very small beam spot. These changes are expected to result in a factor of 40 increases in luminosity and instantaneous data rate. Many elements of the BELLE detector need to be upgraded to handle the increase in data rate.

The Japanese government has recently announced that the KEK-B and BELLE upgrade projects have been approved and has committed the equivalent of $100M to begin long-lead procurement and final design efforts. In-kind contributions from some international partners have already been secured. Much of the fabrication, installation and commissioning will take advantage of existing, experienced personnel at the KEK laboratory. The advertized schedule has SuperKEK-B beginning operations in 2014. The total cost of the KEK-B accelerator upgrade project has been estimated at $400M, not including contingency or labor. A final decision by the Japanese government on the funding profile for the out-years is anticipated in 2011.

US groups from four universities and one national lab have proposed contributing to the BELLE-II detector upgrade. The university groups were all active in BELLE and have extensive experience in that collaboration, though they did not make major hardware contributions to the original detector. The national lab group (PNNL) joined shortly before submission of the proposal and does not have a history of involvement in HEP research but has a few staff scientists with backgrounds in HEP research, and institutional experience in project management and computing. The primary deliverable envisioned for the US groups are optical components for an upgraded time-of-flight system for particle identification. Some of these groups have already been working on R&D for these systems.

The initial estimate of the cost for the proposed US scope presented at the review was $8.5M including 15% contingency but no escalation. These costs had not been internally reviewed at the time of the panel review, though the PNNL group stated that would be one of their first tasks. The total cost of the BELLE-II detector upgrade was quoted as approximately $45M, not including contingency or labor.

Members of KEK laboratory management attended the panel review and endorsed the participation of US groups in the BELLE-II upgrade. In response to questions from the review panel, they stated that additional participation by US groups in both the detector and accelerator upgrades would be welcomed, and that there were areas where additional in-kind and/or technical contributions would be valuable.

iii. The Italian Super-B Proposal

An international collaboration of about 300 physicists from 9 countries has proposed a new accelerator and detector complex to continue the B-Factory research program at a site in Italy. The new facilities are collectively referred to as the SuperB project. The lead sponsor of the project would be the Italian government, through its national HEP/NP laboratory system, the *Insituto Nazionale di Fisica Nucleare* (INFN). The nominal site for the new facility would be the Frascati campus of INFN, near Rome, where there is existing expertise and infrastructure for conducting accelerator-based research. The new construction required for the accelerator project would include a 1.3km tunnel and a linac and damping ring for the injector system; the detector would be almost entirely new. Both the accelerator and detector proposals assume re-use of many components of the SLAC B-Factory complex to realize significant cost savings and expedite the construction schedule. This beneficial re-use of components is a central part of the US proposal discussed below.

The Italian government has repeatedly expressed interest in this proposal and in December 2010 formally announced its support for the project. The proposal has also been reviewed by the European Strategy Session of the CERN Council, which conducted a scientific "roadmapping" exercise for the European HEP community similar to the HEPAP P5 report. The project has attracted a few MOUs between laboratories involved in the collaboration but no major commitments have yet been made, as other counties were awaiting the Italian government's decision. The advertized schedule has SuperB beginning operations in 2017. The total cost of the SuperB accelerator project has been estimated at $520M, not including contingency or labor, and assuming a contribution of SLAC B-Factory components valued at approximately $140M. The total cost of the SuperB detector was quoted as approximately $145M, including contingency and labor and assuming a contribution of SLAC B-Factory components valued at approximately $50M.

US groups from 13 universities and three national labs have proposed contributing to the SuperB project. Many of these groups made major hardware contributions to the original SLAC B-Factory detector (known as BaBar) and have extensive experience in the research collaboration; in addition there is a core group of accelerator physicists from SLAC who propose to participate in the design and construction of some major elements of the SuperB accelerator. SLAC is the largest and strongest group in the US collaboration although the co-spokespersons of are university faculty members.

The SuperB accelerator design is an extension of the SLAC B-factory design with extremely small beam sizes in order to achieve a very high particle interaction rate. This design feature was originally proposed by Italian accelerator physicists and later adopted in the Japanese SuperKEK-B design. This design is expected to result in a factor of 100 increases in luminosity relative to the SLAC B-factory. It also includes the possibility of providing a polarized electron beam, which is useful for some physics analyses.

The US proposal is structured in three separate tiers, following guidance from the DOE HEP office, which earlier requested SLAC to lead a cost study examining three possible options for US participation in the SuperB project : (a) only in-kind contributions of re-usable accelerator and detector components from the SLAC B-Factory; (b) contributions to the design and fabrication of specific new deliverables for the

SuperB detector; and (c) contributions to the design and fabrication of specific new deliverables for the SuperB accelerator. Option (a) would not cost the US program anything and would in fact save an estimated $15M of decontamination and decommissioning (D&D) costs since these components would have to be disposed of if they are not re-used; option (b) was estimated at $16.7M including 25% contingency and escalation for a "base" contribution, with $8.3M of possible "enhancements"; and option (c) was estimated at $16.7M including 25% contingency and escalation for a "base" contribution, with $10.2M of possible "enhancements."

The primary deliverables envisioned for the US groups on the SuperB detector are refurbishment and testing of the re-used BaBar detector components and fabrication of a new particle identification system. Some groups have already been working on R&D for this system. The primary deliverables envisioned for the US groups on the SuperB accelerator are components of the RF acceleration system and the main interaction region. In both cases these contributions make use of the expertise of US groups in designing, building and operating similar systems for the SLAC B-factory.

Members of INFN laboratory management did not attend the panel review.

iv. Physics Beyond the Standard Model

In the years since the establishment of the Standard Model of particle physics in the 1970's there have been innumerable searches for phenomena that would indicate new physics processes occurring at higher energies. All of these searches have ultimately been unsuccessful, though there have been many possible indications or "hints" of new physics over the years.

These indications are usually manifested as experimental results that deviate from the Standard Model (SM) prediction with some statistical significance (typically quoted as the number of standard deviations or σ away from the SM prediction, assuming a Gaussian distribution of outcomes). Although results with a statistical significance of 3σ or greater are relatively unlikely to be random statistical fluctuations (approximately 1 in 200 experiments with no true signal would have such a "false positive"), given the large number of experimental searches performed, there are likely to be a few such results which are inconsistent with the SM at any given time. The history of the Standard Model is that most of these results have either not been repeatable by other experiments, or their significance has declined as additional data is collected, indicating the initial result was indeed a random fluctuation. [2]

The challenge in many experiments searching for physics beyond the Standard Model is to find experimental observables which can be both measured precisely and calculated from theoretical models with very little uncertainty, so that the confrontation of theory and experiment is unambiguous. One of the classic experiments in this regard is measurement of the spin precession of the muon in a uniform magnetic field. This property is known as the "magnetic moment" of the muon. Due to quantum

[2] For this reason the HEP community has informally agreed that an experimental result departing from the SM with statistical significance of 3σ constitutes "evidence for" new physics, the threshold for "discovery" is a much more restrictive 5σ. So far there have been no 5σ results indicating new physics beyond the Standard Model, though several approximately 3σ results have come and gone.

effects, this property (conventionally denoted as *g-2*) differs slightly from the conventional Dirac field theory prediction and is called "anomalous". The original measurements were performed in the 1960's to verify that the quantum theory of electrodynamics (QED) originally developed in the 1940's and 50's was correct. These measurements subsequently verified the predictions of QED to very high accuracy. In the present day the theory predictions have been extended to include even smaller effects arising from quantum loop corrections in the SM, and the experimental precision has also improved so that this measurement can now probe for new physics beyond the Standard Model, such as new particles present in these quantum loops, at the energy range of order 1 TeV (i.e. complementary to new particle searches the Fermilab Tevatron and the LHC).

The most recent version of this experiment ran at Brookhaven National Lab as Experiment 821 (E821) from 1997-2001. The final result had an experimental precision of about ½ part per million and a similar estimated theoretical uncertainty. The theory and experimental results differ by a few parts per million so the difference between this result and the SM expectation is 3σ . Due to some controversy over the magnitude of the estimated theoretical uncertainty, and the value of some of the experimental inputs which have to be used to estimate that uncertainty, whether this result constitutes 'evidence for' physics beyond the SM is the subject of debate, but if the uncertainty could be reduced and the result remains unchanged, this would be the first clear discovery of new physics beyond the Standard Model.

v. The g-2 Experiment at Fermilab

An international collaboration of about 80 physicists from 6 countries has proposed disassembling and relocating the experimental apparatus of the Brookhaven E821 experiment to Fermilab, where it can be put in a high intensity proton beam line, along with appropriate new conventional facilities and shielding, in order to re-mount the experiment with a goal of improving the experimental precision by a factor of four. If the central value of the measurement remains unchanged, it would indicate a 5σ discrepancy with the current theoretical prediction and a discovery of new physics beyond the Standard Model, even if the current theoretical uncertainties are not improved. Expected improvements in the theoretical uncertainties would only add to the significance of the result, assuming they do not change the central value of the theoretical prediction.

Some new detector elements need to be built for this version of the experiment, but most of the projected cost is due to upgrades to accelerator beamlines and components, conventional facilities, and detector support infrastructure.

Fermilab and BNL management teams have expressed support for this proposal and it has been reviewed by the Fermilab Program Advisory Committee (PAC). The PAC considered the proposal in 2009 and recommended "Stage 1" (approximately, final design stage) approval due to the potential physics impact, relatively low cost, and good "fit" with the emerging Intensity Frontier program at Fermilab. The total cost of the project is estimated to be $36M, including 40% contingency and escalation, but the proponents plan to request $5M from NSF, so the total proposed cost to DOE would be $31M. A significant fraction of that cost could be borne by the core Fermi budget for accelerator improvements

and conventional facilities since these elements could be re-used once the g-2 experiment is completed. The net incremental cost to the DOE HEP program for mounting the project is estimated to be $21M.

The collaboration is led by US university physicists and includes 9 US research universities along with BNL and Fermilab. Significant foreign contributions are not currently expected other than support of personnel for final design, operations, maintenance and physics research. Most of the collaborators on this proposal were involved in the previous BNL E821 experiment and have in-depth knowledge of the experimental challenges. The original designers of the E821 apparatus have been consulted wherever possible concerning the detailed disassembly and reassembly procedures for the main muon storage ring.

The HEPAP P5 subpanel considered a different version of this proposal when they met in 2008. At the time the proponents were advocating re-mounting E821 at the new Japanese proton accelerator facility, J-PARC, because initial estimates for incremental costs to operate the experiment at BNL or Fermilab were quite high. However, tight space constraints at the J-PARC site were expected to incur higher backgrounds in the g-2 storage ring and reduce the statistical precision of the result relative to the US sited versions. Given these constraints, the P5 panel did not recommend going forward with the g-2 experiment at J-PARC:

There is an excellent physics case for this classic precision measurement. Nonetheless, the estimated cost to the US particle physics program is substantial and would compromise the timely development of higher-priority precision physics experiments such as muon-to-electron conversion. US participation in an experiment at JPARC would cost less and the US in-kind contribution of the existing precision storage ring, which is central to the experiment, would be substantial. A modest level of R&D support should be made available for the (g-2) collaboration to determine the optimal path toward a next generation experiment.

In the interim since the P5 report was issued, the g-2 proponents discovered further technical limitations of a J-PARC installation which led them to abandon this option. A more careful estimation of the incremental costs of the Fermilab site option identified some possible cost savings and eventually led to the revised proposal considered by this review.

4 Reviewer Comments and Findings

This section summarizes reviewer comments and findings on each of the proposals presented. Each mail or panel reviewer wrote an individual letter with their analysis and evaluation of the proposal(s), following the standard DOE Office of Science merit review criteria and the guidance for the review. Each section is organized to address each of the merit review criteria in turn. Quotations are extracted from these review letters without attribution to maintain reviewer confidentiality.

Mail reviewers only considered individual proposals on their merits (though some mail reviewers evaluated more than one of the three proposals under consideration). In general the mail reviewers supported all of the proposals, in some cases very strongly. Panel members evaluated all three proposals and were asked to rank them in priority order. Section iv contains the panelist rankings.

i. BELLE-II proposal

All panel reviewers rated this as one of the top two proposals. Most ranked the g-2 experiment higher in overall priority but strongly recommended that DOE try to support this proposal: *"I urge the DOE management to find a way to support both US participation in Belle II and the new g-2 experiment."*

Quality and Impact of Recent Research:

The reviewers agreed that the proponents have made a strong impact on this field of research:

The SuperKEKB proponents also have a very strong history of producing interesting and important physics results on the Belle experiment. For more than a decade, these groups have been playing a key role in the Belle experiment.

Merit and Feasibility of Proposed Research:

Reviewers concurred that the physics case for this proposal was compelling and well-presented. A representative comment:

I think the flavor physics case, and B physics in particular, is very strong. We have a much more significant investment at the energy frontier, but I think the consensus is that precision physics of the type espoused in this proposal is complementary, and it comes at a relatively modest cost, by comparison.

The proposed US contribution was considered feasible, though some had concerns that not all technical issues with the time-of-flight system had been resolved:

The proponents have made impressive progress and presented test-beam results that are consistent with Monte Carlo simulations. In my opinion, however, this did not constitute a complete proof of principle, and if this project goes forward, it will be important to closely monitor progress on this system.

In addition, the realism of the construction schedule presented for SuperKEKB and BELLE-II was a concern for many reviewers. However, in comparison with the Italian SuperB proposal they felt that the SuperKEKB/BELLE II proposal had significant advantages because (1) the nominal schedule presented for SuperKEKB forecast operations three years before the Italian proposal; (2) the Japanese proposal is an upgrade to an existing facility with little civil construction involved; and (3) the Japanese labs have a strong track-record of delivering accelerator projects on schedule.

Competence and promise of the Research group:

The reviewers agreed that the proponents are experienced and well-qualified to carry out this research.

Adequacy of resources and cost-effectiveness:

Reviewers found the BELLE-II proposal to be very cost-effective and a "good value" for the investment, particularly in comparison to the SuperB proposal. There was concern, however, that the cost as presented by the US BELLE-II collaboration was underestimated:

The budget seems not to be completely realistic, being essentially capital equipment only, without costs for testing, transportation of the components, realistic contingency, etc. Nonetheless, the scale of the proposal, even when fully costed, will be less than even the cheaper scenario requested by the Super-B proposal.

Several reviewers were concerned about the cost-effectiveness and value added by the PNNL group which had recently joined the BELLE-II proposal; and the impact (should the project go forward) of funding a new group without extensive HEP experience in a time of highly constrained funding.

ii. SuperB proposal

All panel reviewers except one rated this proposal the lowest of the three considered, and recommended the BELLE-II proposal be supported instead. While the scientific case was considered compelling and the personnel involved are leaders in the field, the major concerns were the cost and scope of the US involvement in the SuperB effort, even in the most modest scenario presented. The reviewers felt there was significant risk for the US groups to be asked to take on additional scope (and cost) due to their expertise, possibly jeopardizing the success of the project if they cannot deliver. In short, having a major US technical role in a project in which the US does not have a major role in project management was felt to be unwise, particularly in a constrained fiscal environment.

Nevertheless, all the reviewers also recognized the very significant value represented by the SLAC B-factory components which form an integral part of the Italian SuperB proposal. The reviewers recommended that the DOE provide these components to Italy should that project go forward, along with modest technical assistance as needed to help disassemble, reassemble and understand the operating characteristics of these state-of-the-art devices.

Quality and Impact of Recent Research:

The reviewers agreed that the proponents have made a strong impact on this field of research: "*the expertise in detector development and physics analysis of the team is top-notch."*

Merit and Feasibility of Proposed Research:

Reviewers concurred that the physics case for this proposal, which is essentially the same as in the BELLE-II proposal, was compelling, and the remarks above on the physics case for BELLE-II apply here as well; although some commented that the case was not well-presented. One reviewer commented that the SuperB physics presentation could be summed up as "trust us we have done this before."

The proposed US contribution was considered feasible, though as noted above the proposed scope was considerably greater than that of the BELLE-II proposal and that brought with it additional risk. The construction schedule presented for the Italian SuperB proposal was a serious concern as noted above. The lack of a commitment by the Italian government to the proposal at the time of the review was noted by many reviewers, particularly in contrast to the commitment shown by the Japanese government.

Competence and promise of the Research group:

The reviewers agreed that the proponents are highly qualified to carry out this research:

For each of the two international B factory projects the size of the US groups and their competence will assure high visibility and leadership roles for the US participants.

Adequacy of resources and cost-effectiveness:

The higher cost of the proposed US investment in SuperB relative to the BELLE-II proposal, and the potential exposure to significant cost risk, were determinative factors for several reviewers. As one succinctly put it:

Although these estimates were presumably prepared by experienced teams, they have not been subjected to independent review. Moreover…a few potentially costly items that are not in the estimates came to light—e.g., the cost of US contributions to the computing and the cost of additional US participation in the accelerator effort. Finally, indications are that the SuperB project would be critically dependent on US contributions, meaning that the DOE could end up on the hook for even larger contributions to ensure SuperB's ultimate success.

iii. g-2 proposal

Five of the seven panelists gave this proposal the highest priority because of the strong US role in leading this proposal and its potential for major scientific impact, as one put it: *"I rate the g-2 proposal at the top of these three very strong proposals because of its importance to our field, the timeliness and*

the opportunity for the domestic high energy program." As noted above most reviewers urged DOE to try to support both this proposal and the proposed US contributions BELLE-II.

Quality and Impact of Recent Research:

The reviewers agreed that the proponents have made a strong impact on this field of research. The leadership of this proposal is the same as the BNL E821 experiment which is one of the most cited experimental results of the previous decade.

Merit and Feasibility of Proposed Research:

The scientific merit of an improved g-2 measurement was supported by all the reviewers. The experiment is frequently referred to as a "classic" or "textbook" measurement in high energy physics.

The technical challenges of re-mounting the BNL E821 experiment at Fermilab were addressed by the proponents and the reviewers were satisfied that the proposed plan was feasible:

The schedule for g-2 is credible and achievable, provided that civil construction of the building and Fermilab accelerator shutdowns proceed as planned. The scope of the work to transport, rebuild and prepare the g-2 storage ring seems well understood.

We note the plan presented for installing the g-2 ring at Fermilab assumed that the Tevatron will cease operations in 2011 and have a long shutdown in 2012 to install upgrades for the NOvA beamline. Work on accelerator improvements and civil construction needed for g-2 cannot proceed until Tevatron operations are completed, and there is now a proposal to extend Tevatron operations from 2012-4. The proponents tried to address this issue in response to a question from the panel, but a full analysis of the impact of extended Tevatron operations has not been performed yet.

The chief concern of the reviewers was the status and projected evolution of the theoretical uncertainty in the g-2 result. This is nicely summarized in one review:

Previous theory uncertainties associated with the hadronic vacuum polarization (HVP) contributions have been ameliorated by improved low-energy e+e- data from several experiments, and also by a reevaluation of the isospin-breaking corrections affecting the HVP extraction from tau data. The proponents of the g-2 experiment have argued convincingly that a further reduction of the HVP error by nearly a factor of two will be possible using future data from VEPP2000 and KLOE. I am not convinced that the uncertainty associated with hadronic light-by-light scattering (HLBL) will be reduced to the level claimed by the collaboration.

Reviewers had varying positions on the relative importance of the HLBL scattering uncertainty. Many felt that even if one had to admit a larger uncertainty in this contribution to g-2 than was quoted by the proponents, the proposed experimental result would still be very significant, and so they endorsed funding the proposal; however one reviewer took a rather different position:

Thus, the theory error will become a limiting uncertainty, only allowing the overall error to decrease by about a factor of 2. If the central value changes by only 1σ towards the SM (of course, the opposite is also possible), then there will again be a 3σ hint of an effect, without a path to resolve it.

Competence and promise of the Research group:

The reviewers agreed that the proponents are experienced and well-qualified to carry out this research.

The experimental team comprises most key individuals from the BNL experiment as well as a powerful contingent of new members from FNAL and other institutions. There is every reason to think that this team has the breadth and depth to successfully implement the proposed program.

Adequacy of resources and cost-effectiveness:

Reviewers found the costs presented to be adequate for the scope proposed, and the basis for estimate mature, including a Fermilab review by independent experts. Options for transferring some of the costs, as appropriate, to Fermilab infrastructure improvements or other Fermilab projects that will need the same accelerator components have been explored in some detail. The cost-effectiveness versus other HEP investments was generally viewed as good:

The cost of the experiment, though higher than one might have naively guessed given that most of the BNL apparatus will be recycled, is modest compared to most new initiatives that one could imagine.

iv. Panelist Rankings

Reviewer	Top Rated	Middle	Lowest Rated
#1	g-2	BELLE-II	SuperB
#2	g-2	BELLE-II	SuperB
#3	g-2	BELLE-II	SuperB
#4*	SuperB	BELLE-II	g-2
#5	g-2	BELLE-II	SuperB
#6	g-2	BELLE-II	SuperB
#7	BELLE-II	g-2	SuperB

*Reviewer #4 states this is his recommendation **"***If the Italian Government makes a commitment soon".* If not, he would support BELLE-II over SuperB. In all cases he ranks g-2 third.

A majority of reviewers commented that both the g-2 and BELLE-II proposals were highly meritorious and deserved support, e.g.: **"**I urge the DOE management to find a way to support both US participation in Belle II and the new g-2 experiment."

Appendix I

Review Guidance

Dear Colleagues,

This note is to inform you of the process we are developing to review selected new proposals for Intensity Frontier projects. A formal charge for the review will come later.

Current outyear budget projections for the DOE Office of High Energy Physics (HEP) allow for the possibility of funding a relatively modest scale (Total Project Cost approximately $30M) Intensity Frontier project that can provide compelling science opportunities. We have received presentations and /or white papers for three possible projects at roughly this cost scale that we are interested in reviewing further: a relocation of the g-2 experiment currently housed at BNL to Fermilab, along with some new infrastructure and upgrades; US participation in the accelerator and detector components of the Super B-Factory proposal in Italy; and US participation in the upgrade of the BELLE detector at the KEK-B Facility.

In order to meet our internal budget deadlines, we plan to conduct a comparative review of these proposals later this summer. The review will focus on the relative merits of the science and the discovery potential of the different proposals. **Proponents should be prepared to submit complete project proposals (for the entire experiment or just the US contribution, as appropriate), suitable for peer review, including cost, schedule and technical information, by July 1.**

We tentatively plan to conduct a panel review in late July (week of July 26) or early August (week of Aug 9) in the Washington DC area. The review will include presentations by the proponents and questions and answer sessions with the panelists. Proponents may be asked to provide additional background materials in advance of the panel review.

Based on the outcome of this review process, the proposal(s) selected for funding would be managed according to DOE project management guidelines, and could begin fabrication of components no earlier than FY 2012.

If you have any questions about this process in general please contact me directly. In particular, if you foresee problems with the tentative panel reviews dates or the proposal deadline contact me ASAP.

Sincerely

Glen Crawford

Director, Research and Technology R&D Division

Office of High Energy Physics

US Department of Energy

Appendix II

Review Panel Membership

Glen Crawford, Chair	DOE Office of High Energy Physics
David Armstrong	College of William and Mary
Zoltan Ligeti	Lawrence Berkeley National Lab
Daniel Marlow	Princeton University
Frank Petriello	University of Wisconsin
Kevin Pitts	University of Illinois Urbana-Champaign
Ryszard Stroynowski	Southern Methodist University
Harry Weerts	Argonne National Lab

Agency Observers

Cyrus Baktash	DOE Office of Nuclear Physics
Kyungseon Joo	NSF Nuclear Physics
Brad Keister	NSF Nuclear Physics
John Kogut	DOE Office of High Energy Physics
Ted Lavine	DOE Office of High Energy Physics
David Muller	DOE Office of High Energy Physics
Jim Reidy	NSF Elementary Particle Physics
Eli Rosenberg	DOE Office of High Energy Physics
Alan Stone	DOE Office of High Energy Physics

Appendix III

Review Agenda

DOE/HEP Intensity Frontier Review

August 10-12, 2010

Doubletree Hotel, 8120 Wisconsin Ave., Bethesda, MD

AGENDA

Tuesday August 10

	Topic	Speaker	Time
9:00	Executive Session	Kovar/Crawford	30’
	Muon g-2 at FNAL		
9:30	introduction	Hertzog	20’
9:50	Physics Motivation	Stockinger	30’
10:20	Standard Model Status	Roberts	25’
10:45	Coffee Break		20’
11:05	Experimental Method	Hertzog	25’
11:30	Beam Plan at Fermilab	Syphers	25’
11:55	Installation of g-2 at Fermilab	Polly	15’
12:10	Laboratory Perspective	Kim	10’
12:20	Summary and Discussion	Roberts	10’
12:30	**Working Lunch/Exec Session**	**(committee only)**	**60’**
13:30	Committee Q&A (g-2)		30’
	Super-B in Italy (Physics/Detector)		
14:00	Introduction/Overview	Jawahery	35’
14:35	Physics Opportunities	Hitlin	35’
15:10	Coffee Break		20’
15:30	US Deliverables, Cost, Management	Ratcliff	40’
16:10	Overview of SuperB Project	Seeman	35’
16:45	SLAC Role/Summary	MacFarlane	15’
17:00	Executive Session		60’

18:00	Committee Q&A (SuperB)		30’
18:30	Adjourn		

Wednesday August 11

	Topic	Speaker	Time
9:00	Answers to homework questions	(g-2/SuperB proponents as needed)	60’
	Super B in Italy (Accelerator)		
10:00	US Deliverables, Cost, Management	Sullivan	30’
10:30	Coffee Break		20’
	KEK-B Upgrade		
10:50	Overview of US Belle II groups	Kinoshita	10’
11:00	Physics Case/Discussion	Browder	30’
11:30	Detector Upgrade: Overview	Schwartz	20’
11:50	Muon System	Piilonen	15’
12:05	Electronics	Varner	15’
12:20	Beamstrahlung Monitor	Bonvincini	10’
12:30	**Working Lunch/Exec Session***	**(committee only)**	**60’**
	KEK-B Upgrade Cont’d		
13:30	KEK Plans and Funding Status	Yamauchi	15’
13:45	KEK-B Upgrade	Oide	15’
14:00	Budget and Schedule	Browder/Yamauchi	15’
14:15	Summary and discussion	all	15’
14:30	Executive Session		60’
15:30	Committee Q&A (KEK-B)		30’
16:00	Executive Session		120’
18:00	Answers to homework questions	(KEK-B proponents as needed)	30’
18:30	Adjourn		

*If needed, Committee Q&A for SuperB accelerator will occur during 2nd half of this session, and Answers to questions can be presented in late afternoon Exec Session.

Thursday August 12

	Topic	Speaker	Time
9:00	Executive Session		120’
11:00	Closeout	(Committee + agencies only)	60’
12:00	Adjourn		60’