The Nuclear Physics Scientific Horizon:

Projects for the Next Twenty Years

Report of the Ad-hoc Facilities Subcommittee of the
Nuclear Science Advisory Committee

K. Lesko
B. Jacak
K. de Jager
R. Janssens
R. G. H. Robertson
B. Sherrill
W. Zajc
C. Glashausser (Chair)

MARCH 3, 2003

Introduction

In December 2002, Dr. Raymond Orbach, Director of the Office of Science of the Department of Energy, charged the Nuclear Science Advisory Committee to consider what new or upgraded facilities would be needed over the next twenty years to position the field at the forefront of discovery. He asked that the facilities be categorized in three tiers in both the **importance of the science** and the **readiness** of the facility for construction. A report was requested by March 2003. No ranking was to be assigned, and no facility with an estimated cost less than $50 million was to be considered. Dr. Orbach's letter is included in Appendix A.

In January 2003, a subcommittee of NSAC was formed to prepare a draft response to this charge in a timely manner. Members were chosen by the chair in consultation with Dr. Rick Casten, the chair of NSAC, Dr. Dennis Kovar, program officer for Nuclear Physics in the Office of Science, and Dr. Bradley Keister, program officer for Nuclear Physics at the National Science Foundation, with the advice of the members of NSAC. Representatives of projected facilities (Appendix B) were asked to compile short reports on the science and readiness of their facilities, and to make brief presentations to the subcommittee at a meeting held at the Department of Physics & Astronomy of Rutgers University in Piscataway, New Jersey, on February 15, 2003. The agenda is shown in Appendix C. The complete subcommittee discussed the written reports and oral presentations, and prepared this report for submission to NSAC at its meeting on March 6-7, 2003.

Executive Summary

Nuclear science is a key component of the nation's research portfolio, providing fundamental insights into the nature of matter and nurturing applications critical to the nation's health, security, and economic vitality. It is a field with tremendous breadth that has direct relevance to understanding the evolution of matter in the universe. The field is broadly characterized by five major scientific questions that define the main lines of inquiry. These questions will continue to drive nuclear science in the coming decades:

- What is the structure of the nucleon?
- What is the structure of nucleonic matter?
- What are the properties of hot nuclear matter?
- What is the microphysics of the universe?
- What is to be the new Standard Model?

Many of these issues share the scientific thrust of the eleven questions expressed recently in the National Research Council report "Connecting Quarks with the Cosmos".

Addressing this broad and compelling science will require a number of new and upgraded facilities; this report reviews those projects. It follows a recent extensive review of proposed facilities for nuclear science that resulted in the comprehensive report "Opportunities for Nuclear Science: A Long-Range Plan for the Next Decade" in April 2002. The process that led to that report (LRP 2002) involved much of the nuclear science community over many months. The recommendations and discussion in LRP 2002 thus carried great weight with the present subcommittee, and all of the evaluations in the present report are consistent with them.

The committee assigned the various projects to the categories shown in the box below. The basis of the assignments for each project is briefly summarized in the pages that follow.

PROJECT	**SCIENCE**	**READINESS**
Rare Isotope Accelerator (RIA)	1	1
CEBAF 12 GeV Upgrade	1	1
GRETA	1	1
RHIC II/eRHIC	1/1	2
Underground Detector I	1	2-3
CEBAF II/ELIC upgrade	1	3
Upgrade Stable Beam Facility	3	3
RIA II	3	3
Underground Detector II	1	3

Those projects in **Science category 1** that were included in one of the four major recommendations of LRP 2002 have a very strong endorsement from the community. Other projects in this category, while clearly very strong, have generally not yet been as thoroughly reviewed.

A number of proposals that might have been put into **Science category 2** "important" were considered during the long range planning process. Because the community considered these projects to be less compelling, we did not consider them here.

Projects included in **Science category 3** have not yet been considered by the community; they are very long term projects. While we expect them to do excellent science, the precise science goals can be expected to develop significantly before they are ready for review in the next decade.

Readiness category 1 "Ready to initiate construction" was interpreted as "ready for Critical Decision 0 (CD-0)" in standard DOE terminology, with no significant scientific/engineering challenges to resolve prior to construction.

Readiness category 2 "Significant scientific/engineering challenges to resolve before initiating construction," was applied to projects that had significant R&D issues to address or whose technical design and goals had not yet been fully vetted by the community. Note that it may be possible for a project in this category to be in a position to request a near-term CD-0 so that it can initiate the required R&D.

Readiness category 3 was interpreted as "mission **and/or** technical requirements not yet fully defined."

Some of the projects have physics goals that are at least in part closely related to those of other projects. Examples are eRHIC and ELIC, and the Underground Detectors I and II. The subcommittee has assigned each of these projects to Science category 1—we decided that the science proposed for each of them is absolutely central. The fact that they were assigned to different Readiness categories of course in no way reflects on their relative merits, which must be evaluated in future Long Range Plans.

PROJECT TITLE: Rare Isotope Accelerator

First Estimate : ☐ $50M -$99M ☐ $100M-499M ☒ $500M-$1B ☐ >$1B

SCIENCE (Category 1)

The nuclear science community has recognized the study of rare isotopes as central to the field and made RIA its highest priority for major new construction. The 2002 NSAC Long Range Plan (LRP 2002) states: *"The Rare Isotope Accelerator (RIA) is our highest priority for major new construction. RIA will be the world-leading facility for research in nuclear structure and nuclear astrophysics."* RIA is essential to maintain U.S. competitiveness in the field of basic nuclear science and, indeed, is central to the mission of the DOE Office of Science. RIA will provide unprecedented intensities of rare isotopes for many areas of research, especially those critical to the expansion of the frontiers of knowledge of nuclear many-body systems, to the understanding of the chemical evolution of the universe, and to the measurement of fundamental symmetries at low energies. It represents a major step in pursuing the compelling questions:

- What is the structure of atomic nuclei and how do complex systems derive their properties from their individual constituents?
- How are the heavy elements created and how do nuclear properties influence the stars?
- What are the fundamental symmetries of nature?

The science of RIA has been reviewed thoroughly; it is absolutely central to nuclear physics. The recent NRC report "Connecting Quarks with the Cosmos: Eleven Science Questions for a New Century" listed "*understanding the origin of the elements heavier than iron*" as one of the eleven questions and noted that RIA will be essential to address this question. Moreover, an improved understanding of the universe and the interpretation of the wealth of astronomical observations from new and planned telescopes requires the new quantitative description of nuclei that RIA will provide. Indeed, RIA offers the promise of guiding the development of a unified theory of the nucleus in which both the properties and excitation modes of nuclei at or near stability and the exotic, loosely bound structures far from stability are encompassed in a single theoretical framework. RIA-driven progress in theory will also benefit other areas where nuclear properties are critical, such as the determination of the neutrino mass scale from double-beta decay. RIA will provide access to nuclei with special symmetries where, for example, standard electro-weak theory can be tested to high precision at low energy, and limits can be set on CP violation in flavor conserving interactions, which will in turn help us understand the baryon asymmetry of the Universe.

RIA will also have an impact on other fields. Research quantities of isotopes important for biomedical research yield enormous potential for medical diagnostics, treatment, drug development, and metabolic studies. RIA will also support stockpile stewardship by providing essential nuclear data. It will make important contributions to materials science and other disciplines by implantation of radioactive isotopes for studies of wear and corrosion, material modifications, and space radiation effects.

READINESS (Category 1)

The Rare Isotope Accelerator has undergone a series of reviews of the technical plan devised to achieve its scientific goals. The conclusion of these reviews is that there are no remaining major technical issues preventing construction and that the cost and schedule are on a firm basis. Thus, RIA is ready to initiate construction. The rare isotope intensities available at RIA will be at least one to two orders of magnitude higher than any other existing or planned facility worldwide. This will be accomplished at RIA by a combination of innovative technologies, which in 1999 were considered by the NSAC ISOL Task Force to be "*based principally on moderate extrapolations from proven technologies. ... No technical show stoppers were identified and the community is ready to proceed to the conceptual design stage*". To ensure expeditious construction and to take account of potential cost leveraging, the ISOL Task Force identified a list of important areas for R&D funding. Starting in 2000, seven universities and national laboratories have participated in the ensuing R&D effort, and some of these R&D items have been incorporated as DOE performance milestones. Significant progress has been achieved in each of the major R&D items, including gas-stopper beam extraction, fragment range compression, ECR sources, multiple charge state acceleration, superconducting RF structures, and high-power targets.

An NSAC subcommittee carried out a RIA Facility baseline cost and technical review in 2001. This committee concluded: "*the TEC [Total Estimated Cost] presented is reasonable. The 32% contingency is judged to be appropriate at this point in the development of the project.*" and "*the technical risk on the major components is low with appropriate R&D.*" Recent updates and refinements have shown that the overall cost is stable and the technical risks are low.

PROJECT TITLE: CEBAF 12 GeV Upgrade at Thomas Jefferson Laboratory

First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENCE (Category 1)

The 2002 NSAC Long Range Plan *"strongly recommend[s] the upgrade of CEBAF at Jefferson Laboratory to 12 GeV as soon as possible. [It] is critical for our continued leadership in the experimental study of hadronic matter…"* This was one of the four major recommendations of the LRP. The Upgrade has the support of a large and active user community (~1100 scientists from 29 countries); it has been enthusiastically reviewed by numerous outside peer groups and will be unique worldwide. The realization of the Upgrade will create synergies with other fields of research, most notably with large-scale computing, high-energy physics, and astrophysics.

The 12 GeV Upgrade will provide answers to questions of fundamental importance, probing issues that are absolutely central to nuclear science in four main areas:

- The experimental study of gluonic excitations in order to understand the confinement of quarks. Theoretical conjectures, now strengthened by lattice QCD calculations, indicate that the most spectacular prediction of QCD - quark confinement - occurs through the formation of a string-like "flux tube" between quarks. Determining the spectrum of gluonic excitations of mesons will directly test our understanding of confinement and provide key information for unraveling its mysteries if current theory is incorrect.
- The determination of the quark and gluon wavefunctions of the nuclear building blocks. "Deep inelastic scattering" cross sections and polarization observables will be extended for the first time to the critical region where the basic three-quark structure of the nucleon dominates. In addition, similar measurements of new "deep exclusive scattering" will open the door to a comprehensive characterization of these wavefunctions through the framework of "Generalized Parton Distributions", which provide direct access to information on the correlations among the quarks.
- Exploring the basis of our understanding of nuclei. A diverse program of measurements will provide a firm intellectual underpinning for all of nuclear physics by answering the question "How does the phenomenological description of nuclei as nucleons interacting via an effective interaction parameterized using meson exchange arise from the underlying dynamics of quarks and gluons?"
- Tests of the Standard Model of electro-weak interactions and the determination of fundamental parameters of QCD. Precision parity-violating electron scattering experiments made feasible by the 12 GeV Upgrade have the sensitivity to search for deviations from the Standard Model that could signal the presence of new physics. Studies of the three neutral pseudoscalar mesons will provide key parameters of low-energy QCD.

READINESS (Category 1)

The Upgrade project is a proposal to double the maximum energy of the CEBAF accelerator at Jefferson Lab, to build a fourth experimental facility dedicated to the study of gluonic excitations, and to upgrade the existing experimental facilities. The accelerator portion of the upgrade is straightforward; CEBAF was designed with such an upgrade in mind. The key issues were increasing the performance of the superconducting RF cavities and cost-effectively increasing the bending power of the recirculation arcs; both have been addressed successfully. The major equipment in the new end station is a refurbished large superconducting solenoid previously used at LAMPF and SLAC. All aspects of the project, as well as a detailed budget, have been described in reports. The scientific goals and proposed design of the Upgrade have been positively evaluated by internal and peer review committees, including the 2001 Institutional Plan Review and the 2002 DOE S&T Review of JLab, which noted that "*It appeared that the 12 GeV upgrade project is technically ready to proceed.*" The 2002 LRP considered the project "*ready to initiate construction*". All remaining R&D is focused on cost reduction and/or improved technical contingency; no R&D is needed to demonstrate feasibility. The project is fully ready to initiate construction. A CD-0 package has been generated and is awaiting approval.

PROJECT TITLE: Gamma Ray Energy Tracking Array (GRETA)

First Estimate : ☒ $50M -$99M ☐ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENCE (Category 1)

The detection of gamma-rays from excited states in nuclei plays a vital and ubiquitous role in low-energy nuclear science experiments. This was noted in LRP 2002, which added "*The physics justification for a [new] 4π tracking array is extremely compelling, spanning a wide range of fundamental questions in nuclear structure, nuclear astrophysics, and weak interactions.*" Gains in resolving power of two to three orders of magnitude over the best present day arrays such as Gammasphere can be achieved by applying the new concept of gamma-ray energy tracking to a 4π detector shell consisting of electrically segmented germanium crystals. This array, called GRETA, will enable a new class of high-resolution gamma-ray experiments at existing stable and radioactive beam facilities, as well as at RIA.

GRETA is essential to fully address fundamental questions about the structure and stability of nuclei, including the understanding of single-particle and collective modes and their interplay at low and high spin, the description of changes in structure with proton number Z and neutron number N, the delineation of the limits on Z and N for which nuclei can exist even briefly, the unraveling of the properties of exotic nuclei, and the investigation of nuclear matter under density oscillations. These questions are absolutely central to nuclear physics. GRETA can begin to answer these questions at existing stable and radioactive beam facilities. Furthermore, GRETA is an essential complement to RIA to define and map the limits of nuclear existence; make possible the exploration of the exotic quantal systems that inhabit these boundaries; and isolate, amplify, or reveal new phenomena, new types of nucleonic aggregations, and key interactions. GRETA will also be instrumental in carrying out the other aspects of the RIA science program in the areas of nuclear astrophysics and fundamental symmetries. In addition, the technology of tracking and signal processing developed for GRETA will have applications, for example, in homeland security and medical physics. The very active user community for this device represents a significant fraction of the RIA community.

READINESS (Category 1)

Over the last 5 years, major R&D efforts at several universities and national laboratories have validated the GRETA concept and demonstrated proof of principle. Highly segmented germanium detectors have been successfully manufactured and their performance has been characterized. Pulse-shape digitization and digital signal-processing methods have been developed to determine the position, energy, and time of gamma-ray interactions. These efforts have demonstrated that the position resolution (~1 mm) required for tracking has been achieved. Tracking algorithms have been developed that are capable of identifying the interaction points of a particular gamma ray in the germanium crystal.

The technical milestones achieved have been published in refereed journals. Workshops involving a large section of the community of potential users have helped determine the design and specifications of GRETA. Engineering designs have been generated for all critical components of the project. A national gamma-ray tracking coordination committee (GRTCC) has reviewed all aspects of the device including the R&D plan, the mechanical design, the specifications for detectors and electronics, the time line for construction, the cost and contingency estimates, etc. A major upcoming milestone will be the testing of the three-crystal detector module. No high-risk technical challenges were identified and GRETA was found to be ready to initiate construction.

PROJECT TITLE: RHIC II/eRHIC

First Estimate : ☐ $50M -$99M ☐ $100M-499M ☒ $500M-$1B ☐ >$1B

The study of nuclear matter under extreme conditions has been identified as a central theme of nuclear physics. The coupled upgrades and developments in experiments and the accelerator complex at Brookhaven National Laboratory that form the RHIC II/eRHIC proposal provide unprecedented capabilities for studying hot nuclear matter at the highest possible energy densities (RHIC II) and cold nuclear matter in a regime of the highest possible gluon densities (eRHIC).

RHIC II SCIENCE (Category 1)

RHIC II will allow a very high luminosity physics program at the Relativistic Heavy Ion Collider, opening experimental channels and providing kinematic reach not currently available. LRP 2002 noted that "*significant upgrades of the collider and the experiments will be needed…[to] allow in-depth pursuit of the most promising observables characterizing the deconfined state."* Indeed, these new capabilities will address the central issues raised in the initial discovery phase of RHIC: the large suppression of high momentum hadrons and back-to-back pairs, and the high initial state densities implied by the significant flow velocities and anisotropies. The recent NRC report "Connecting Quarks with the Cosmos: Eleven Science Questions for a New Century" listed "*Are there new states of matter at exceedingly high density and temperature?*" as one of the eleven questions and noted that "*At higher densities, neutrons and protons may 'dissolve' into an undifferentiated 'soup' of quarks and gluons, which can be probed in heavy-ion accelerators.*"

An initial factor of 4 RHIC luminosity upgrade, currently in progress, coupled with new high precision inner tracking detectors will allow direct observation of heavy meson decays, will increase the momentum reach for hard processes, and will permit the study of photon+jet correlations as a precision probe of the plasma state. These measurements will allow RHIC to move from existence proof of the quark-gluon plasma to measuring its energy and particle transport properties. Detector upgrades to allow clean measurement of low mass di-leptons will allow direct measurement of thermal radiation from the quark-gluon plasma as well as the study of chiral symmetry restoration. Introducing electron cooling of the RHIC beams will yield an additional luminosity increase of a factor of 10. This ultimate luminosity, corresponding to a factor of 40 beyond the RHIC I design value, will allow a complete mapping of the spectroscopy of heavy flavor bound states. This maximum luminosity is required for measurement of the very tightly bound upsilon as an essential control in the program to directly probe the plasma by observing its effect upon quark confinement. This project will create synergies with other areas of research, in particular large-scale data-driven computing, particle physics, and astrophysics. It is absolutely central to U.S. science and will allow a comprehensive characterization of the quark-gluon plasma, the state of matter believed to have existed in the early universe.

eRHIC SCIENCE (Category 1)

Construction of an electron-ion collider (eRHIC) is motivated by the need to better understand the fundamental structure of nuclei. The high center of mass energy provided by eRHIC, together with its very significant luminosity and collider geometry, will allow mapping nuclear structure over the broadest possible range of momentum transfer and momentum fraction (x). This range will explore the complete dynamics of nuclear binding, from meson exchange at low momentum transfer to the parton dynamics probed at small x and large momentum transfer. Studying tagged electron-proton collisions will make it possible to measure the structure of the mesons themselves, while collisions between polarized electrons and protons will further elucidate the role of spin in the proton wavefunction. Of particular interest is the study of the strong gluon fields that are uniquely accessible in an electron-nucleus collision. Very deep inside the nucleon, the density of gluons is known to rise rapidly. In a nucleus there is a further enhancement of the density because of the large number of neighboring nucleons. It has been proposed that the gluon density may actually saturate, and that the strong coupling among the gluons in fact would decrease if one could probe deeply enough inside a nucleus. Indeed, existing RHIC data are consistent with such a scenario. The possibility of directly studying this intriguing kind of matter with an electron-ion collider has sparked considerable synergy among several sub-fields of nuclear physics.

Some of the physics goals of this project overlap those of the ELIC project at JLab, and others are complementary. The physics goals have been widely discussed in town meetings and workshops and are strongly backed by the broad user community of RHIC. These goals are interesting to scientists studying electromagnetic, hadronic, and heavy ion interactions. LRP 2002 stated that such an *"electron-ion collider initiative…is an extremely exciting initiative for the long term"* and recommended R&D support of this initiative. We find the science of an electron-ion collider absolutely central to U.S. science.

READINESS (Category 2)

Carrying out this scientific program will require several steps of upgrades and construction. The first step is to upgrade the existing large detectors at RHIC to allow the lepton and heavy flavor decay measurements, coupled with the concurrent luminosity increase at RHIC by a factor of 4 to yield the first harvest of new physics. This step of upgrades is ongoing and is planned to be supported from within the Nuclear Physics Program base. There are ongoing R&D programs to develop the required detector upgrades, and substantial operating experience and understanding of the backgrounds in the RHIC collision environment. Introduction of electron cooling of the RHIC beams will provide the required additional luminosity boost of a further factor of 10 (RHIC II) and make first preparations for electron-ion collisions.

Initial feasibility studies have established the requirements for the electron cooler, leading to current R&D work and collaborations with other laboratories to develop the required sources and superconducting cavities. Construction of a 10 GeV electron ring will then allow electron-ion and polarized electron-proton collisions. The ring-ring option for eRHIC is based upon existing technology; the electron ring is very similar to existing designs. This option provides for one electron-nucleus interaction region with minimal interference with RHIC operations for nuclear collisions. The detector design is underway, drawing from experience with other asymmetric colliding beam facilities and the subsequent developments in low-*x* physics. There is a well-defined plan of research and development to allow detailed design and construction for each step. However, significant technical challenges are not yet resolved, and additional R&D is needed before construction can begin. The staged nature of the RHIC II/eRHIC project is designed to take advantage of each R&D step as it is completed and carry out construction in stepwise manner.

PROJECT TITLE: Underground Detectors I and II

SCIENCE (Category 1)

Neutrino physics in the DOE's Division of Nuclear Physics has made remarkable discoveries in the past few years. The long-standing solar neutrino problem has been resolved by the Sudbury Neutrino Observatory experiment, showing that the anomaly is caused by neutrino flavor conversion and that the electron neutrino is a linear combination of massive neutrino states. KamLAND, a long-baseline reactor antineutrino experiment, has now shown electron antineutrino disappearance with the same mixing parameters (the "LMA" set). Neutrinos are now known to have mass and to mix, but fundamental questions remain. Are neutrinos their own antiparticles, or is the antineutrino a different entity? Only by studying neutrinoless double beta decay can the answer to this question be found. This is the focus of Underground Detector I. Are there other neutrino states, sterile but slightly admixed with the three active states? The sun is the neutrino source that can best establish this possibility, but experiments sensitive to low-energy neutrinos are required. This is the focus of Underground Detector II. Experiments in both areas will cost approximately $50M to $150M each.

The merit and significance of these efforts is highlighted in the NRC report "Connecting Quarks with the Cosmos: Eleven Science Questions for a New Century;" one of the eleven questions is "*What is the mass of the neutrino and how have neutrinos shaped the evolution of the Universe?"* The experiments must be sited at a deep underground laboratory. Both kinds of experiment will determine fundamental neutrino properties with far reaching consequences in particle physics, nuclear physics, astrophysics, and cosmology. The experiments have, in addition, synergistic connections to industrial technologies and homeland security issues. The user community has been growing rapidly over the past few years. One of the four primary recommendations of LRP 2002 was *"the immediate construction of the world's deepest underground science laboratory. This laboratory will provide a compelling opportunity for nuclear science to explore fundamental questions in neutrino physics and astrophysics."* The community has thus strongly endorsed this physics, though individual detectors were not discussed in that report and have generally not yet been peer-reviewed. It seems likely that a major new double beta decay experiment will be ready before a major new solar neutrino experiment; this explains the labels I and II attached to the two detectors. Moreover, it is probable that, within the time frame considered here, an upgraded or next-generation double beta decay experiment will be indicated.

Neutrino physics is on the threshold of several major discoveries, and it is important to realize that a 20-year plan may be rapidly overcome by experimental results. The neutrino community will then need to reconsider its long-term options.

Detector I (A Double Beta Decay Experiment)
First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

The two primary goals of the next-generation double beta decay experiments are to determine whether or not neutrinos are their own antiparticles and to establish the absolute mass scale. Double beta decay experiments are the only means for determining the charge-conjugation (neutrino-antineutrino) properties and may also be the only practical experimental method for the foreseeable future to determine the absolute mass scale. The oscillation experiments indicate that the neutrino mass scale may be within reach of the next generation experiments. At least one neutrino has a mass of at least 0.05 eV, and the recent Wilkinson Microwave Anisotropy Project suggests that the sum of neutrino masses is not greater than about 0.7 eV.

READINESS (Category 2-3)

Two detector projects were considered in this evaluation. In addition, it was noted that the HEP panel is considering another proposal, one based on ^{136}Xe, and that a cryogenic detector, CUORE, is being developed in Europe. The experiments differ in the development of their technologies and maturity of experimental techniques. None has been subject to peer review and none is ready for construction.

The ^{76}Ge experiment (**MAJORANA**) was viewed to be the most mature, based on tried-and-true Germanium semiconductor technology. Earlier versions of the experiment have demonstrated good control over detector technology issues and intrinsic and external backgrounds. Additional improvements are being pursued to further reduce sensitivity to backgrounds by segmenting the detector elements. This experiment requires a large quantity of isotopically separated ^{76}Ge isotope, a source for which has been identified in Russia. The subcommittee noted that collaboration between this effort and the GRETA effort could be beneficial.

The ^{100}Mo-based detector (**MOON**) might be capable of both double beta decay searches and low energy solar neutrino investigations. This experiment might not require isotopically separated target material. Both scintillator and bolometric options are being pursued by a Japanese-U.S. collaboration.

All detectors require a well-shielded underground laboratory environment to achieve ultimate sensitivity. The Majorana experiment would likely be the first large-scale experiment ready for deployment in the proposed U.S. National Underground Laboratory.

Detector II (Low Energy Solar Neutrino Experiments)
First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

Low energy solar neutrino experiments will make use of the best-calibrated and most distant source of neutrinos, the sun, to make sensitive measurements of fundamental neutrino properties and to pursue astrophysical measurements. Neutrinos are now known to transform between families. If there are only three neutrinos, the matrix that describes the three family oscillations must be unitary. Low energy solar neutrinos will permit the most precise determination of the mixing angle θ_{12} that describes the mixing between the first and second families. This precision is a critical element in determining the unitarity of the MSNP matrix and the possible existence of sterile neutrinos. In addition, oscillations to sterile neutrinos can be pursued through the disappearance of solar signals. Neutrino magnetic moments can be probed using the low-energy dependence of neutrino-electron elastic scattering. So there are three fundamental neutrino parameters being investigated with low energy neutrinos: the mixing angles and unitarity of the MNSP neutrino mixing matrix, the existence of sterile neutrinos, and neutrino magnetic moments. Low energy solar neutrinos also permit extremely sensitive tests of solar models. Little is known about the actual rate of CNO reactions in the sun. Observation of CNO neutrinos would constrain both stellar models and the presolar abundances of metals.

READINESS (Category 3)

There are several proposals for next-generation solar neutrino detectors. All the experiments propose to measure the lowest energy solar neutrinos coming from the p-p, ^{7}Be, and pep neutrino branches, and are faced with challenging issues of detector purity and radioactive backgrounds, securing a suitable deep underground site, and addressing underground safety issues for the exotic detector media. A variety of different technologies are being addressed, including cryogenic fluids, high-pressure time-projection chambers (TPCs), and liquid scintillators. The detectors can be roughly grouped into those measuring electron-neutrino-specific nuclear reactions and those measuring neutrino elastic scattering. Both processes must be measured to extract the neutrino mixing parameters independently of other data, but measurements of one of them in combination with existing data would also constrain the parameters significantly.

All the experiments face challenging radio-purity issues. There are two proposed charged current (electron neutrino) experiments. LENS is based on indium-loaded liquid scintillator. Ultimately 400 tons of this scintillator, 100,000 photomulipliers (PMTs), and a deep underground site would be required. The MOON detector, in addition to searching for double beta decay, can potentially also search for low energy solar neutrinos. Three elastic scattering experiments are being developed. Two involve cryogenic media and one a high pressure TPC. The development of large volume cryogenic neutrino detectors is making impressive advances and will likely lead to mid-scale prototypes in the next few years. The CLEAN proposal uses liquid neon at 27°K. Wavelength shifters and submerged PMTs will detect neutrino-induced scintillation light. The HERON proposal will use 20 tons of superfluid liquid helium. In addition to its intrinsic radiopurity and well-characterized scintillation signals, helium also affords roton and electron-bubble detection techniques and benefits from nearly a decade of detector R&D. Shielding of external backgrounds will require careful attention as these proposals advance.

While it is clear that there are significant technical issues still to be resolved in realizing a p-p solar neutrino detector, a number of innovative approaches are under active development. The necessary investment in this R&D is significant and the projects are being aggressively pursued. It seems probable that one or more of the approaches will reach a stage of demonstrating technical feasibility in the next few years. It is also clear that one or more appropriate underground sites will be available for a p-p experiment within several years.

PROJECT TITLE: CEBAF II Upgrade

First Estimate : ☐ $50M -$99M ☐ $100M-499M ☒ $500M-$1B ☐ >$1B

SCIENCE (Category 1)

A future upgrade of CEBAF beyond 12 GeV would build on the physics insights obtained from the CEBAF 12 GeV upgrade, and expand on our understanding of the structure of the nucleon and nuclear binding. An upgrade of CEBAF to 24 GeV has been considered for some time; it is briefly described in Appendix B. This would result in a machine with unprecedented luminosities and capabilities that would allow the exploitation of a new generation of experiments at high momentum transfer. More recently, attention has shifted to consider the possibility of a high-luminosity electron-light ion collider (ELIC) facility in the center-of-mass (CM) energy range of 20-65 GeV; this prospect is creating a great deal of excitement within the community. The research program of this type of facility at JLab, similar in many ways to the electron-ion collider EIC that received a preliminary endorsement in LRP 2002, will be absolutely central to nuclear physics. Briefly stated, it should:

- Complete our quantitative understanding of how quarks and gluons provide the binding and the spin of the nucleon. There are still glaring gaps in our knowledge of the QCD structure of the proton. How large is the gluon and quark angular momentum in the description of the proton's spin? Can we fully disentangle the contribution of up, down, and strange quarks to the proton's momentum and spin?
- Help us understand how quarks and gluons evolve into hadrons via the dynamics of confinement. Measurements of the spin dependence of this complex process known as hadronization should dramatically improve our understanding. In astrophysics, hadronization emerges as a key aspect of the transition from the deconfined state of free quarks and gluons in the Big Bang (the quark-gluon plasma) to stable hadronic matter.
- Determine how the nucleus affects quarks and gluons. Not much is known about the properties of quarks and gluons in a nucleus. Experiments will try to elucidate the complete spin and flavor structure of the modifications of the behavior of quarks and gluons in nuclear systems.

Some of the physics goals of this project overlap those of the eRHIC project, and others are complementary; the program will evolve naturally over the lengthy period before experiments could begin. The current physics goals have been widely discussed and strongly backed by the large user community of JLab. The realization of ELIC will create synergies with other fields of research, most notably large-scale computing, high-energy physics, and astrophysics. The CEBAF II Upgrade would maintain the Office of Science's leadership in the field of hadronic and nuclear physics.

READINESS (Category 3)

An upgrade of CEBAF to 25 GeV would use the existing CEBAF footprint. The cryomodules would use the 12 GeV design; ARC magnets, the beam switchyard, and the

Hall equipment would be changed. The task is relatively straightforward. The ELIC project requires the upgrade of Jefferson Lab's CEBAF accelerator to a 5 GeV energy-recovering linac and the realization of a storage ring complex, accelerating and storing light ions of up to 100 GeV. The ELIC project could also include the 25 GeV fixed-target facility. Design studies indicate that the luminosity of the colliding electron and light ion beams can be as high as 10^{35} cm^{-2} sec^{-1} with an arbitrary polarization direction of either particle at a CM energy of up to 45 GeV. Up to four interaction regions would be available. Preliminary studies predict that the CM energy could be increased to 65 GeV and that medium and heavy ions can be stored as well, albeit with lower luminosity. A number of technical challenges must be resolved, and several R&D projects have been started. These include development of a polarized electron source with a high average current and high bunch charge, electron cooling of protons/ions, and energy recovery at high current and high energy. The design of an interaction region and detector that support the combination of high luminosity and high detector acceptance and resolution is also underway. Construction would not begin until after the completion of the 12 GeV upgrade of CEBAF; the final design will be influenced by evolving physics goals.

PROJECT TITLE: Upgrade of Stable Beam Facility

First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENCE (Category 3)

In July 2001, NSAC was charged with an evaluation of the scientific opportunities and priorities within the DOE Low-Energy Nuclear Physics Program. The NSAC subcommittee reported that this "*is a field alive with new ideas and vital questions*", that it is *"experiencing impressive productivity",* and that it is *"an important national resource playing a central role in educating the next generation of nuclear scientists".* The subcommittee further recognized that "*it is essential to maintain sufficient capabilities in the production of stable beams of sufficient intensity, energy and atomic mass range to pursue the high quality physics program that is emerging*". The stable beam accelerators available today carry out this program. The forefront stable beam facility envisioned here would start operation after RIA turns on, i.e., well into the next decade. As a result, a precise delineation of the scientific issues it will address is premature. Besides the significant progress to be expected from the current program, the discovery potential of RIA is such that it can be expected to influence and define major research directions at an upgraded stable beam facility.

READINESS (Category 3)

Since the science program of a forefront stable beam facility remains to be defined, the technical requirements are uncertain. Nevertheless, such a facility will be expected to provide the capability of mounting experiments at the frontiers of research in nuclear structure and dynamics, in the areas of fundamental interactions and nuclear astrophysics, and in numerous applied areas as well. All are likely to require beams of elements covering the entire periodic table, with a maximum energy ranging from 50 MeV for protons to 10 MeV/u for uranium, with intensities larger than one particle microampere for the heaviest projectiles and even higher for light ions. The accelerator will have to provide continuous wave operation, high beam purity, excellent energy resolution, low transverse emittance, and flexible beam timing. It will have to be highly reliable, very flexible and cost effective to operate. It is expected that a state-of-the-art accelerator of this type will benefit greatly from the R&D effort for RIA.

PROJECT TITLE: Rare Isotope Accelerator Upgrade

First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENCE (Category 3)

RIA was conceived to have the capabilities required to address the central scientific questions discussed in the LRP 2002. Given the decade-and-a-half time scale that will be required for RIA experiments to fully address and resolve these important issues, it is premature to specify the scientific reach of upgrades for RIA. Because of its remarkable discovery potential, there is little doubt that some of RIA's first results will raise new scientific questions that, along with new emerging technologies, will provide the direction for future upgrades.

It is also important to consider the global context of nuclear science in 15 years. At that time, RIA will possibly compete with other accelerators of rare isotopes on the world scene and upgrade plans may need to be designed to complement future initiatives in other countries. While it may seem early for such considerations, experience with all other major facilities has shown that it is important to plan for upgrade paths on the 20-year time scale. The flexibility of the RIA design is such that it can accommodate a number of different possible kinds of upgrade.

READINESS (Category 3)

Since the specific requirements of the science that would drive a RIA upgrade have not yet been identified, the technical specifications are uncertain. The following suggestions are intended to serve as an illustration of some plausible upgrade paths. One class of RIA upgrades would increase the scientific scope of the facility by focusing on new tools to extend its scientific reach. These include:

- Increasing the isotope yields from RIA by a combination of factors based on evolving new technologies
- Increasing the energy of the primary beams to provide high intensity uranium capability in the GeV range
- Incorporating a storage ring as an experimental tool
- Increasing the energy of the reaccelerated beams to several tens of MeV

Another class of upgrades would utilize the high beam powers available at RIA for new directions not associated with nuclei far from stability. One that appears promising is the use of RIA as a source for ultra-cold neutrons for fundamental interaction studies. While RIA will not match accelerators such as the SNS in raw beam power, it possesses the unique capability of customizing a production target that could provide ultra-cold neutron yields substantially beyond those that will be available at currently proposed facilities.

APPENDIX A

December 18, 2002

Professor Richard F. Casten
A.W. Wright Nuclear Structure Laboratory
Yale University
New Haven, CT 06520

Dear Professor Casten:

For more than a half-century the Department of Energy's Office of Science has envisioned, designed, constructed and operated many of the premiere scientific research facilities in the world. More than 17,000 researchers and their students from universities, other government agencies, private industry and from abroad use Office of Science facilities each year—and this number is growing.

Creating these facilities for the benefit of science is at the core of our mission and is part of our unique contribution to our Nation's scientific strength. It is important that we continue to do what we do best: build facilities that create institutional capacity for strengthening multidisciplinary science, provide world class research tools that attract the best minds, create new capabilities for exploring the frontiers of the natural and physical sciences, and stimulate scientific discovery through computer simulation of complex systems.

To this end, I am asking all the Office of Science's advisory committees to join me in taking a new look at our scientific horizon, and to discuss with me what new or upgraded facilities will best serve our purposes over a timeframe of the next twenty years. More specifically, I charge the committees to establish a subcommittee to:

A. Consider what new or upgraded facilities in your discipline will be necessary to position the Office of Nuclear Physics at the forefront of scientific discovery. Please start by reviewing the attached list of facilities, assembled by Dr. Peter Rosen and his team, subtracting or adding as you feel appropriate, with prudence as to cost and timeframe. For this exercise please consider only facilities/upgrades requiring a minimum investment of $50 million.

B. Provide me with a report that discusses each of these facilities in terms of two criteria:

   1. The *importance of the science* that the facility would support. Please consider, for example: the extent to which the proposed facility would answer the most important scientific questions; whether there are other ways or other facilities that would be able to answer these questions; whether the facility would contribute to many or few areas of research; whether construction of the facility will create new synergies within a field or among fields of research; and what level of demand exists within the scientific community for the facility. In your report please categorize the facilities in three tiers, such as "absolutely central," "important," and "don't know enough yet," according to the potential importance of their contribution. Please do not rank order the facilities.

   2. The *readiness* of the facility for construction. Please think about questions such as: whether the concept of the facility has been formally studied in any way; the level of confidence that the technical challenges involved in building the facility can be met; the sufficiency of R&D performed to-date to assure technical feasibility of the facility; and the extent to which the cost to build and operate the facility is understood. Group the facilities into three tiers according to their readiness, using categories such as "ready to initiate construction," "significant scientific/engineering challenges to resolve before initiating construction," and "mission and technical requirements not yet fully defined."

Many additional criteria, such as expected funding levels, are important when considering a possible portfolio of future facilities, however for the moment I ask that you focus your thoughts on the two criteria discussed above.

I look forward to hearing your findings and discussing these with you in the future. I would appreciate at least a preliminary report by March, 2003.

Sincerely,

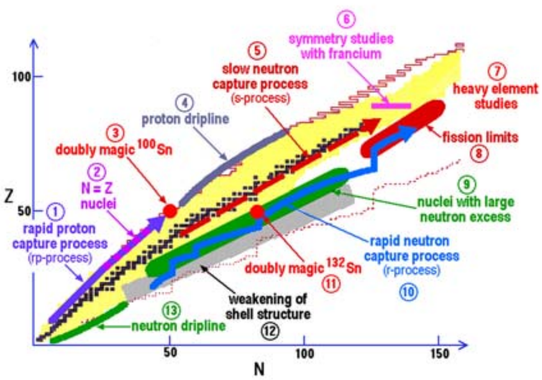
Dr. Raymond L. Orbach
Director
Office of Science

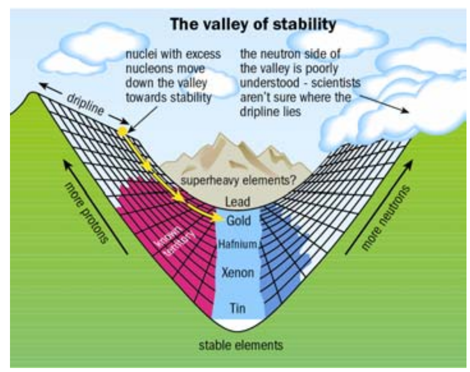
APPENDIX B
Project Descriptions provided by the Nuclear Physics Division of the Office of Science

PROJECT TITLE: Rare Isotope Accelerator
First Estimate : ☐ $50M -$99M ☐ $100M-499M ☒ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- The proposed Rare Isotope Accelerator (RIA) will have next-generation capabilities for producing beams of short-lived nuclei. These "exotic beams" can be used to study nuclei at the very limits of stability that are today inaccessible.





- These capabilities will allow the U.S. to take a world-leadership role in addressing three topics at the heart of fundamental nuclear physics research:
  - The nature of nucleonic matter – how are nuclei formed from protons and neutrons, especially as the extremes of nuclear stability (shortness of lifetimes) are approached?
  - The origin of the chemical elements – how are the nuclei of the elements that make up the world around us formed in the cosmos and explosive stellar events?
  - The fundamental laws of physics – how is the physics beyond the current Standard Model of fundamental particles and interactions revealed in nuclei?

- RIA consists of a superconducting linear accelerator (linac) that provides a high-power, 400 MeV/nucleon beam of stable isotopes incident on production targets where short-lived nuclei will be produced and subsequently reaccelerated. These exotic beams will be used to make measurements in several experimental areas.

- As designed, RIA will provide 10 to 100 times more intensity for re-accelerated exotic beams than any other facility currently operating or even envisioned for the next decade. It makes possible studies of nuclei and reactions inaccessible elsewhere and has a significant impact in applied research areas, such as medical diagnostics, electronics, national security and material sciences.

- The RIA has been identified by the Nuclear Science Advisory Committee as the highest priority for new construction in its recent 2002 Long Range Plan for Nuclear Science.

- An ongoing R&D program is addressing technical issues for the construction of RIA; no technical impediments to its construction have been identified thus far.

PROJECT TITLE: CEBAF 12 GeV Upgrade at Thomas Jefferson Laboratory

First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

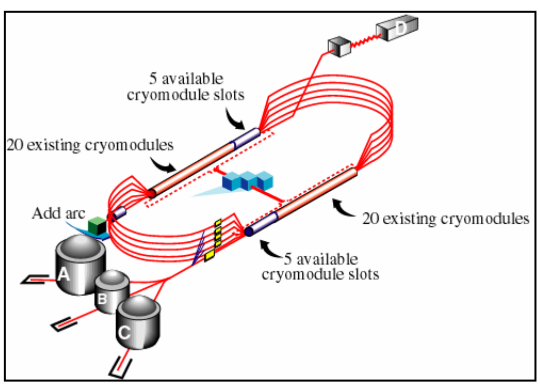
- The Continuous Electron Beam Accelerator Facility (CEBAF) at TJNAF is a world-class, unique facility for the study of the largely unexplored transition between the nucleon-meson and the quark-gluon descriptions of nuclear matter.

- CEBAF's capabilities have allowed its ~1200 users to mount a research program that has provided precise measurements of basic properties of the nucleons and to observe indications of the transition from nucleonic structure to quark structure.

- An outstanding research program is envisioned until about the end of the decade. Thereafter an increase in beam energy will be needed to advance these studies and maintain a world-leadership role in the study of the quark structure of matter.

- The 12 GeV upgrade of CEBAF allows breakthrough programs to be launched in three key areas:
  - Address one of the outstanding fundamental questions in particle/nuclear physics, namely, "What is the nature of quark confinement in QCD?" New lattice QCD studies indicate that force fields ("flux-tubes") may be responsible for confinement and their excitations should lead to a spectrum of new mesons - exotic hybrid mesons – that should be observed in 12 GeV measurements
  - Map out the quark and gluon structure of the proton and other nuclear building blocks at the most basic quantum level
  - Verify and further study the transition from a nucleonic composition (nucleons) to a quark composition of nuclei

- The project includes an accelerator upgrade, construction of a new hall for meson spectroscopy, and improvements in the existing halls. TJNAF's advancements in accelerator technology and use of existing facilities make the project cost-effective.



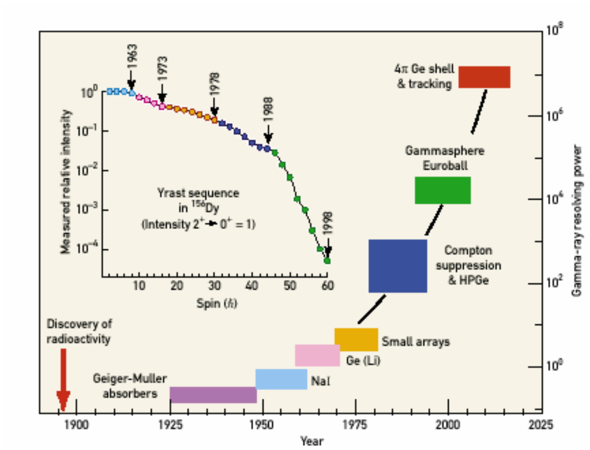
- The merits of the scientific case were recognized in the Nuclear Science Advisory Committee (NSAC) 2002 Long Range Plan which recommended that the 12 GeV Upgrade be implemented as soon as possible to exploit the new scientific opportunities that have emerged.

PROJECT TITLE: Gamma Ray Energy Tracking Array (GRETA)
First Estimate : ☒ \$50M -\$99M ☐ \$100M-499M ☐ \$500M-\$1B ☐ >\$1B

SCIENTIFIC IMPORTANCE:

- High-resolution gamma-ray detector arrays have been responsible for the enormous progress in the last decade in our understanding of nuclear structure, particularly the properties of nuclei with large deformations and high spin.

- Based on R&D over the last few years, a new detector concept that utilizes gamma-ray tracking has emerged with the promise of advancing detection sensitivity by up to 1000 times that of any currently existing array and to exploit the discovery potential in nuclear structure, nuclear astrophysics and fundamental symmetries with accelerated exotic beams.



- The proposed Gamma Ray Energy Tracking Array (GRETA) would be the premier high-resolution gamma-ray facility in the world, consisting of a spherical shell of hyper-pure, highly segmented, tapered hexagonal Germanium detector modules. Each detector module would be able to locate a scattering point in three dimensions.

- GRETA is the prime candidate for major new instrumentation necessary at the possible Rare Isotope Accelerator or another location.

- The technology of this tracking array will also lead to many applications in basic research, space science, medical physics, environmental surveying, and security.

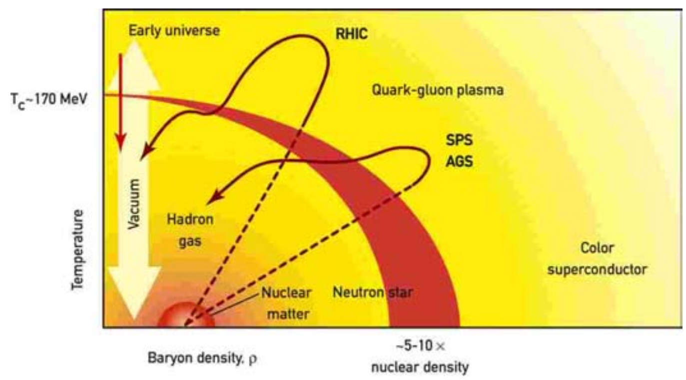
- The Nuclear Science Advisory Committee (NSAC) endorses an initiative in gamma-ray tracking in its 2002 Long Range Plan. A Gamma-Ray Tracking Steering Committee provides input on technical issues and R&D priorities.

PROJECT TITLE: RHIC II/eRHIC
First Estimate : ☐ $50M -$99M ☐ $100M-499M ☒ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- The Relativistic Heavy Ion Collider (RHIC) is the first and only machine in the world capable of colliding heavy ions. Research at RHIC is aimed at re-creating and studying the properties of the Quark-Gluon Plasma (QGP), a form of matter that is believed to have filled the early universe.

- RHIC will allow scientists to characterize the nuclear matter phase diagram in terms of energy density (temperature) and baryon density (pressure) and understand the relationship between the most fundamental constituents of matter and the complex array of particles and nuclei that make up the world around us.



- Initial RHIC results have provided tantalizing indications that a QGP with fascinating properties is being produced. It is envisioned that the discovery and survey phase program will take approximately one decade to complete.

- The RHIC II/eRHIC Project will increase the beam luminosity and adds a 10 GeV electron ring to the current RHIC facility:
  - The factor of ten increase in luminosity with associated detector upgrades will enable studies of new phenomena through rare probes. This scientific program will allow a comprehensive characterization of the Quark-Gluon Plasma.
  - The electron-ion collider (eRHIC) will allow a high luminosity probe of the substructure of nucleons and nuclei. The eRHIC will address some of the key questions in nuclear physics: What is the structure of matter in terms of its quark and gluon constituents? How do quarks and gluons evolve into hadrons via the dynamics of quark confinement? How do quarks and gluons reveal themselves in the structure of atomic nuclei? Can nuclei be used to study partonic matter under extreme conditions? To what accuracy is QCD the exact theory of the strong interaction?

- The NSAC 2002 LRP identified the science of RHIC II/eRHIC to be very promising and endorsed R&D initiatives in this direction.

PROJECT TITLE: Underground Detector

First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- Precision deep underground experiments carried out by nuclear physicists are an essential complement to searches for new physics in accelerator-based experiments.

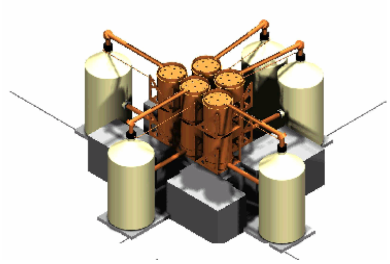
- For decades, physicists have been wondering why detectors on earth have recorded only a fraction of the neutrinos that should have been ejected from the nuclear reactions in the sun's core. Recent results from the Sudbury Neutrino Observatory (SNO) and the SuperKamiokande Detector provide clear evidence that electron neutrinos from the sun change to another neutrino type; they oscillate, and thus have mass. This significant discovery implies new physics beyond the Standard Model, which as presently constituted, assumes that neutrinos do not have mass.

  - Different and complementary experiments are being designed that provide rich new information on the properties of neutrinos and will provide an exciting new frontier at the interface between particle and nuclear physics:
    - The end-point energy of tritium decay to establish the electron neutrino mass
    - Searches for neutrino-less double beta decay for the properties of neutrinos
    - Long baseline oscillation experiments for masses of various flavor neutrinos
    - Next generation solar neutrino measurement to establish further precision

  - Funding is requested here for an underground laboratory experiment that will be identified as most promising in terms of advancing our knowledge. At this time, the most promising experiment appears be the neutrino-less double-beta decay experiment Majorana, which offers the possibility to probe the absolute neutrino mass scale, and determine whether the neutrino is its own anti-particle.

- The Majorana experiment would substantially improve on present results, and has the potential to determine the electron neutrino mass down to a few 10s of milli-electron volts (MeV), an important mass range since present neutrino oscillation experiments suggest that at least one neutrino has a mass in this range.

- The NSAC 2002 LRP recommends the immediate construction of the world's deepest underground science laboratory, which NSF is considering to support. Marjorana is a prime candidate for the underground laboratory or another location.



The Marjorana Concept

PROJECT TITLE: CEBAF II Upgrade
First Estimate : ☐ $50M -$99M ☐ $100M-499M ☒ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- Research at the Continuous Electron Beam Accelerator Facility (CEBAF) at TJNAF is aimed at obtaining an understanding of the quark structure of nuclear matter.

- Quark confinement is still an open problem in the study of the structure of hadronic systems within Quantum Chromodynamics (QCD). The detailed understanding of quark confinement in hadronic matter calls for subtle measurements, where spin degrees of freedom and the selection of specific final state configurations are key ingredients. Such measurements are necessary to comprehensively study quark confinement in strongly interacting hadronic systems and complete a "genome-like project" that will map out quark-gluon structure.

- With increasing energy, smaller spatial structures can be explored, allowing one to probe the regime where the effects of quark and gluon degrees of freedom become more apparent.

- Research with 12 GeV beams will start in about 2010 following the completion of the CEBAF 12 GeV Upgrade Project and continue for an ~10 year research program, at which time an additional upgrade in energy will be needed to make further progress in these studies.

- A 24 GeV upgrade of CEBAF would result in a machine with unprecedented luminosities and capabilities that will allow the exploitation of a new generation of experiments at high momentum transfer.

- The technology base will exist at the completion of the 12 GeV Upgrade to extend, in a rather straightforward way, CEBAF to a 24 GeV fixed-target facility.

PROJECT TITLE: Upgrade of Stable Beam Facility
First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- Progress in both nuclear structure and astrophysics studies depend upon the availability of exotic beams, to produce and study nuclei that lie in unstudied regions of the nuclear chart and that are involved in astrophysics processes.

- The Nuclear Physics program currently operates three facilities targeted to advance understanding of three of the central questions of nuclear science identified in the NSAC 2002 Long Range Plan: what is the structure of nucleonic matter, what is the nuclear microphysics of the universe and is there new physics beyond the Standard Model of particle physics.

- The NP program supports facilities with limited capabilities for these studies and the NSAC 2002 Long Range Plan has identified the Rare Isotope Accelerator (RIA) as the highest priority for new construction. RIA will allow the U.S. to take a world-leadership role in addressing these three topics that are at the heart of fundamental nuclear physics research.

- In preparation for the support needed to design and construct RIA, the NP program is planning to phase-out operations of two of the three existing low energy facilities..

- It will be crucial to the success of the scientific program to operate and maintain at least one stable beam facility to provide low energy heavy ion beams.. The Stable Beam Facility Upgrade is a necessary complement to RIA, providing the high quality stable nuclear beams for:
  - Physics studies across extended regions of stable and unstable nuclei
  - High intensity beams at and just above Coulomb barrier energies for studies of the heaviest nuclei
  - Beams for development and testing of new instruments and experimental methods
  - Testing of new concepts in accelerator components
  - Improved capabilities for beams for industrial applications

PROJECT TITLE: Rare Isotope Accelerator Upgrade
First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- The proposed Rare Isotope Accelerator (RIA) will allow the U.S. to play a leadership role in addressing the nature of nucleonic matter, the origin of the elements and the fundamental laws of physics.
- Research with exotic, short-lived beams will start in about 2012 following the completion of the construction of the Rare Isotope Accelerator, and continue for an ~10 year research program. At this time, an upgrade to the existing facility is envisioned to further pursue exciting physics opportunities that will have been discovered during the first decade of the life-time of this new facility.

- The very preliminary estimated Total Project Cost is ~$400M and would be completed in FY 2024.

PROJECT TITLE: Underground Laboratory Detector II
First Estimate : ☐ $50M -$99M ☒ $100M-499M ☐ $500M-$1B ☐ >$1B

SCIENTIFIC IMPORTANCE:

- Solar and atmospheric neutrino experiments have pointed to neutrino oscillations as the likely mechanism to explain the long-standing "puzzle" of the deficit of solar neutrinos, and as evidence for incompleteness of the standard model of fundamental particles and interactions. The attention of ongoing and near term experiments is turning to a deeper understanding of detailed neutrino properties and interactions, and of the fusion model of the sun.

- Next generation solar neutrino detectors are now being conceptualized that are real time spectrometers capable of detailed measurements of the low energy solar neutrino spectrum not presently possible. These detectors utilize the specific properties of selected target isotopes, or the unique detection capabilities possible with rare gases, to measure low energy neutrinos.

- Among the physics questions that could be addressed with such a detector is the precise measurement of the neutrino oscillation mixing parameters, an improved limit on the neutrino magnetic moment, a test of the unitarity of the neutrino mass matrix, an indication if sterile neutrinos exist, and further tests of our understanding of the nuclear physics of the sun.

- Funding is requested here for an underground laboratory experiment that will be identified as most promising in terms of advancing our knowledge. The NSAC 2002 LRP recommends the immediate construction of the world's deepest underground science laboratory, which NSF is considering to support. This next general solar neutrino experiment is a candidate for the underground laboratory or another location. The very preliminary estimated Total Project Cost is ~$150M.

APPENDIX C

NSAC Subcommittee on Categorizing Future Facilities
Meeting, Saturday, February 15, 2003
Rutgers University Physics Department

AGENDA

8:30 Executive Session

9:00 RIA and RIA Upgrade Presentation
Science: K. Gelbke (20 minutes)
Discussion: (15 minutes)
Readiness: D. Geesaman (20 minutes)
Discussion: (15 minutes)

10:10 Greta Presentation
Science: T. Glasmacher (10 minutes)
Discussion: (10 minutes)
Readiness: I-Yang Lee (10 minutes)
Discussion: (10 minutes)

10:50 Coffee

11:00 Stable Upgrade Presentation
Science: M. Riley (10 minutes)
Discussion: (10 minutes)
Readiness: C. Lyneis (10 minutes)
Discussion: (10 minutes)

11:40 Underground Detectors I,II Presentation
The Science and Discovery Potential of New
Neutrino Experiments: W. Haxton (14 minutes)
Discussion: (14 minutes)
The next double beta decay experiment:
J. Wilkerson: (13 minutes)
Discussion: (13 minutes)
Measuring the pp solar neutrinos:
T. Bowles: (13 minutes)
Discussion: (13 minutes)

13:00 Lunch (Executive Session)

14:00 RHIC II/eRHIC presentation
- RHIC II: T. Hallman (15 minutes)
- Discussion: (15 minutes)
- eRHIC: R. Milner (15 minutes)
- Discussion: (15 minutes)
- RHIC Machine Plan: T. Roser (10 minutes)
- Discussion (10 minutes)

15:20 CEBAF Upgrade Presentation
- L. Cardman: (20 minutes)
- Discussion: (20 minutes)

16:00 Coffee

16:15 CEBAF II Upgrade Presentation
- R. Ent: (20 minutes)
- Discussion: (20 minutes)

16:55 Executive Session

19:30 Adjourn