Report of the HEPAP Particle Astrophysics Scientific Assessment Group (PASAG)

23 October 2009

Introduction and Executive Summary

1.1 Introduction

The US is presently a leader in the exploration of the Cosmic Frontier. Compelling opportunities exist for dark matter search experiments, and for both ground-based and space-based dark energy investigations. In addition, two other cosmic frontier areas offer important scientific opportunities: the study of high-energy particles from space and the cosmic microwave background."

- P5 Report, 2008 May, page 4

Together with the Energy Frontier and the Intensity Frontier, the Cosmic Frontier is an essential element of the U.S. High Energy Physics (HEP) program. Scientific efforts at the Cosmic Frontier provide unique opportunities to discover physics beyond the Standard Model and directly address fundamental physics: the study of energy, matter, space, and time.

Astrophysical observations strongly imply that most of the matter in the Universe is of a type that is very different from what composes us and everything we see in daily life. At the same time, well-motivated extensions to the Standard Model of particle physics, invented to solve very different sets of problems, also tend to predict the existence of relic particles from the early Universe that are excellent candidates for the mysterious dark matter. If true, the dark matter isn't just "out there" but is also passing through us. The opportunity to detect dark matter interactions is both compelling and challenging. Investments from the previous decades have paid off: the capability is now within reach to detect directly the feeble signals of the passage of cosmic dark matter particles in ultra-low-noise underground laboratories, as well as the possibility to isolate for the first time the high-energy particle signals in the cosmos, particularly in gamma rays, that should occur when dark matter particles collide with each other in astronomical systems. In the coming decade, the same type of dark matter particles may be produced anew in the Large Hadron Collider (LHC), while relic copies are detected both underground at low energy and from outer space at high energy. Each of these will provide a needed piece of the puzzle. This is a particularly exciting time of convergence of theory and experiment, particle physics and astrophysics.

Astrophysical observations provided another stunning surprise: the expansion rate of the Universe, rather than slowing down due to gravitational attraction, is apparently speeding up. Either three quarters of the energy density of the Universe is of a completely unknown form – dubbed dark energy – or General Relativity breaks down on cosmological scales and must be replaced with a new theory of gravity. Either way, there are profound implications for fundamental physics. The dark energy could be the energy of the vacuum, which is predicted to arise from quantum fluctuations but at a strength that is wrong by more than one hundred orders of magnitude, demanding new particle physics, or something else entirely. By studying the expansion rate history of the Universe with much better precision with several techniques, key questions can be addressed: Is the dark energy density constant over cosmic time, or has it evolved? Are the different manifestations of dark energy consistently described in the framework of General Relativity, or is there something wrong with the framework itself?

The study of high-energy cosmic ray particles was a core element of the early development of particle physics, as beams of comparable energy could not be produced at accelerators. In the modern era, the cosmic particles (both charged particles and gamma rays) are also understood to be messengers from astrophysical accelerator systems harboring extreme conditions that are impossible to duplicate in terrestrial laboratories. These extreme environments, which are not yet well understood, include stellar-mass and supermassive black holes, and discovery of new types of sources is likely as measurement capabilities are increasing dramatically. The origins and detailed characteristics of the cosmic rays are still a mystery, but it is quite possible this century-old question could be answered in the coming decade. Early Universe relics that are too massive to be produced at accelerators have also been hypothesized, including dark matter particles, and in many models they produce high-energy charged particles and gamma rays.

The most pervasive relic from the early Universe is the Cosmic Microwave Background (CMB). A core target of current CMB research is the understanding of Inflation, a period of accelerated expansion in the very early Universe presumably driven by new particle physics at energy scales significantly higher than what is likely ever to be directly accessible at accelerators. Suggested mechanisms include quantum gravity, string theory and/or Grand Unified Theories, or compactification of extra dimensions. The science of the CMB is therefore also high-energy particle physics. CMB experiments now seek to measure the topology of the CMB polarization and, although the instrumentation techniques are those of radio astronomy, the required large-scale integration and detailed analysis of very large data sets are critical areas of HEP expertise needed to make possible the next great step forward.

In April 2009, at the request of the Office of High Energy Physics of the Department of Energy and the National Science Foundation, the High Energy Physics Advisory Panel formed the Particle Astrophysics Scientific Assessment Group (PASAG) subpanel for the purpose of developing a plan for U.S. particle physics at the Cosmic Frontier for the coming decade under a set of budget assumptions. The charge and subpanel membership are shown in the Appendix. For practical reasons, the scope of PASAG is limited to projects in dark matter, dark energy, high-energy cosmic particles (cosmic rays, gamma rays, and neutrinos), and projects seeking HEP resources to study the Cosmic Microwave Background. There are important projects (*e.g.*, to study low-energy neutrinos, low-energy cosmic rays, nucleon decay, as well as projects seeking to study gravitation and to search for gravitational radiation) that were deemed outside the scope of PASAG.

Activities at the Cosmic Frontier are marked by rapid, surprising, and exciting developments. This report is based on a snapshot of where the field stands right now. The subpanel attempted to provide advice that is durable, but significant new developments – and great surprises – are likely. It is important to be open to significant new directions over the decade.

Projects at the Cosmic Frontier naturally exist at the boundary between particle physics and astrophysics. Some projects are obviously very close to the core of particle physics; other projects straddle the boundaries between fields and, in some cases, would not happen without significant HEP participation or leadership. These projects are designed to answer very important scientific questions and, in many cases, have the potential to uncover new directions for particle physics. Our prioritization criteria for HEP investment, described in Section 2, take into account these issues.

As astrophysical observations offer new opportunities to answer questions in fundamental physics, it is necessary to understand in sufficient detail the related astrophysical phenomena. One need not be a particle physicist to study these phenomena, but particle physicists must ensure they are understood to the required precision to use them for solving some of the outstanding mysteries in particle physics. The high standard of proof for new physics is not tied to technique – it is the same at both accelerators and telescopes – so the astrophysics investment is sometimes necessary to realize the particle physics benefit. Furthermore, the relationship is symbiotic: particle physicists have much to offer these important related fields of study and often have a major impact on them. We have much to learn from each other, and there is much we can do together.

The multi-disciplinary, multi-agency, and multi-national character of particle astrophysics is understood by the PASAG as an essential feature. Concurrent with our work is the ongoing NRC Astro2010 “Decadal Survey” of activities in astronomy and astrophysics, jointly funded by NASA, NSF, and DOE. There is also the OECD Global Science Forum Working Group on Astroparticle Physics, following on the European ASPERA and ApPEC processes. The projects that are under consideration by two or more of these studies are appropriately evaluated from the different perspectives provided by the different panels. These cases are noted, and the PASAG hopes its report will provide useful input to the other ongoing studies.

The PASAG charge includes four budget scenarios for particle astrophysics:

1. **Scenario A.** Constant effort at the FY 2008 funding level (*i.e.*, funding in FY 2010 at the level provided by the FY 2008 Omnibus Bill, inflated by 3.5% per year and continuing at this rate in the out-years).
2. **Scenario B.** Constant effort at the FY 2009 President's Request level (*i.e.*, funding in FY 2010 at the level provided by the FY 2009 Request, inflated by 3.5% and continuing at this rate in the out-years).
3. **Scenario C.** Doubling of funding over a ten year period starting in FY 2009 (*i.e.*, funding in FY 2010 at the level provided by the FY 2009 President's Request, inflated by 6.5%, and continuing at this rate in the out-years).
4. **Scenario D.** Additional funding above funding scenario C, in priority order, associated with specific activities needed to mount a leadership program that addresses the scientific opportunities identified in the National Academies of Sciences EPP2010 report or the HEPAP Particle Physics Project Prioritization Panel (P5) report.

The FY10 total budgets used in this study were approximately \$84M, \$94M, and \$96M for scenarios A, B, C, respectively. To calculate the phased resources available for construction and operation of new projects, the committed funding for existing projects and ongoing science analysis (the "base", estimated with some simplifying assumptions) was subtracted for each year. For the entire FY10-FY20 period of this study, the total (in then-year dollars) available for new projects was \$266M, \$389M, and \$640M for scenarios A, B, and C, respectively.

These budget scenarios provided very tight constraints that forced difficult choices in the planning. By constructing the optimal science program possible in each budget scenario, there emerged a consensus view of the priorities and guiding principles provided in this report.

1.2 Summary of main recommendations

Exciting times are ahead for particle astrophysics, with many new results emerging from operating projects and even more expected soon from the projects currently under construction.

Recommendation: Even in the leanest budget scenarios, the full budgets for the projects that are already under construction or that are currently operating should be maintained. Every operating project should have a well-defined sunset review date and a realistic plan for possible extended operations. Sunset reviews and decisions must carefully consider international and multi-agency perspectives.

The panel evaluated the scientific opportunities available under the different budget scenarios. The opportunities include the following:

- For dark matter direct detection: next-generation (G2) facilities capable of reaching sensitivity levels better than 10^{-46} cm^2 (about a factor 400 better than present-day limits and a factor ~10 better than expected for the experiments already under construction), and third-generation (G3) experiments surpassing the 10^{-47}cm^2 level. Details are different for the different technologies. G2 experiments would have typical target masses of approximately one ton, with a construction and operation cost in the range of \$15M-\$20M, and G3 experiments would have target masses of many tons with a construction and operation cost around \$50M.
- For dark energy, several stage-IV projects have been proposed, including the space-based Joint Dark Energy Mission (JDEM) and the ground-based Large Synoptic Survey Telescope (LSST), which are large, and the medium-scale ground-based BigBOSS project.
- For the next step in the study of the highest energy cosmic rays, providing a factor of seven increase in statistics over the existing capabilities of Auger South and building on its achievements and expertise, the Auger North facility has been proposed. To understand features in the cosmic ray spectrum at lower energy, the Telescope Array Low Energy extension (TALE) has been proposed. For the next step in very high-energy gamma rays, providing at least an order of magnitude improvement in sensitivity and new capabilities, the large-scale AGIS array has been proposed as a joint effort with the European-led CTA project. HAWC is a different kind of ground-based very high-energy gamma-ray detector, at much smaller scale, that would provide a factor of 15 improvement in sensitivity over its predecessor, Milagro. There is also a small proposal to upgrade the existing VERITAS detector.
- In CMB research, a relatively small level of support has been proposed for Fermilab participation in the QUIET II experiment.

All of these projects have very high merit, but they do not all fit in the budget envelopes. The prioritization criteria developed by PASAG are described in Section 2. The programs are summarized below, along with the important discussion points that follow. The priorities are generally aligned with the recommendations for the Cosmic Frontier in the 2008 P5 report.

Scenario A (constant level of effort at the FY08 level)

In dark matter, the current world-leading program is maintained, but world leadership would be lost toward the end of the decade:

- Two G2 experiments and the 100-kg SuperCDMS-SNOLAB experiment are supported. The technology selection for the G2 experiments should occur soon enough to allow the construction of at least one G2 experiment to start as early as FY13.
- No G3 experiments can be started in this decade. Progress will be slowed, risking loss of U.S. world leadership. However, due to the risk of picking the wrong technology, this is preferable to descoping to only one G2 experiment.

In dark energy, it is not possible to have major HEP hardware and science contributions to any large project. World-leading participation is supported in only very limited areas (allocations to be determined, see Section 6).

The High-energy Cosmic Particle area is severely curtailed in this scenario in order to preserve viable programs in dark matter and dark energy, and only the VERITAS upgrade and HAWC are possible. Even in this very lean scenario, the diversity offered by these two projects is a priority, and their impacts are large for a relatively small investment. Auger North and AGIS are not possible. This would be a retreat from U.S. leadership in high-energy cosmic rays and high-energy gamma rays (see Section 5).

In Cosmic Microwave Background research, QUIET II is supported, along with possible other small investments in CMB research provided the prioritization criteria in Section 2 are clearly met.

Scenario B (constant level of effort at the FY09 level)

The current world-leading program in dark matter is maintained, but with some risk later in the decade:

- Two G2 experiments and the 100-kg SuperCDMS-SNOLAB experiment are supported. The technology selection for the G2 experiments should occur soon enough to allow the construction of at least one G2 experiment to start as early as FY13.
- Only one G3 experiment can start in this decade. Based on what is known at this time, to mitigate risk of picking the wrong technology, a

broad second-generation program is a higher priority than starting a second G3 experiment.

In dark energy, Scenario B may provide just enough funding for significant participation in only one large project, but at significant risk since the total costs are still uncertain and the one project probably will not adequately address all the scientific issues. A program with world-leading impact in dark energy is possible, but in a limited way (see Section 6). The overall funding profile requirements are uncertain, but the straight-line budget scenario does not appear to allow sufficient resources for a fast start early in the decade, and some adjustments to the profile would be necessary.

In High-energy Cosmic Particles, the VERITAS upgrade, HAWC, and a reduced, but still leading, AGIS that is fully merged with CTA are highest priority in this scenario. Auger North is not possible. This would be a retreat from U.S. leadership in high-energy cosmic rays (see Section 5 and the discussion below).

In Cosmic Microwave Background research, QUIET II is supported, along with possible other small investments in CMB research provided the prioritization criteria in Section 2 are clearly met.

Scenario C (doubling budget)

A world-leading program in dark matter:

- Two G2 experiments plus the 100-kg SuperCDMS-SNOLAB experiment are supported. The technology selections should occur soon enough to allow construction to start on at least one experiment as early as FY13.
- Two G3 experiments can start in this decade.

In dark energy, a world-leading program is enabled, with coordinated activities in space and on the ground (see Section 6). Significant HEP roles in one large project are possible, along with a moderate-scale project and/or a substantial role in a second large project. As in Scenario B, the straight-line budget scenario does not appear to provide sufficient resources for a fast start early in the decade. Although the overall funding profile requirements are uncertain, some adjustments to the profile would likely be necessary.

In High-energy Cosmic Particles, a world-leading program is enabled, with:

- the VERITAS upgrade, HAWC, and a reduced but still leading role in AGIS that is fully merged with CTA; and
- U.S. leadership of Auger North.

In Cosmic Microwave Background research, QUIET II is supported, along with possible other small investments in CMB research provided the prioritization criteria in Section 2 are clearly met.

Scenario D (additional funding beyond Scenario C)

Augmenting the program in Scenario C, an additional $200M investment over the decade would enable major roles in two complementary, stage-IV dark energy projects, ensuring continued U.S. leadership in this field and providing the best chance of a major breakthrough in dark energy in this decade.

Discussion

The following points are important to note:

- For the budget exercises, the available construction and operations costs were used for each project. The uncertainties in the costs vary widely and can only be better determined with detailed cost/technical/schedule reviews.
- The leaner scenarios A and B forced extremely difficult choices. In any scenario, if the funding available for a project was judged to be insufficient to support a world-class result, the project was removed. Similarly, in any scenario if only R&D-level funding could be accommodated, with insufficient funding for construction, the R&D was also removed from the program. It is therefore important to revisit these choices if sufficient resources outside of HEP (e.g., from astronomy and astrophysics programs in the U.S. or from additional agencies outside the U.S.) become available. In the cases of JDEM, LSST, and AGIS, the subpanel recommends contributions from HEP agencies that are a portion of the total project costs, appropriate to the shared scientific interest with astronomy and astrophysics, and therefore a decision to proceed must rely on strong support from other agencies and/or nations.
- In all three scenarios, projects in dark energy represent the largest total investment, reflecting the very high scientific priority and the fact that large projects are required to make significant progress in this area. Even with that large fractional investment, there are significant challenges and risks, particularly in the leaner scenarios. In Scenario A, it is not possible to have major HEP hardware and science contributions to any large dark energy project. Scenario B is still very risky because it is near the threshold for significant participation in only one large project. This will require great vigilance and careful consultation with the scientific community. For example, because JDEM is not currently well defined, yet is very expensive, there is at present considerable risk that a large fraction of the total available resources will be spent on a project that does not provide a scientific return that matches HEP priorities while precluding any significant participation in other dark energy projects that could (see Section 6).

- No previous panel has been charged with selecting a national or international portfolio of dark energy projects maximizing scientific return within resource constraints. Coordination between projects, and within the JDEM design process, to insure a coherent U.S. dark energy program has been lacking. PASAG is not properly constituted to allocate resources within the dark energy portfolio; however HEP community issues that should be addressed in a process for selecting a dark energy portfolio are described in Section 6. The ongoing Astro2010 survey is an important element of this process.
- A balanced program is itself a priority. For example, in Scenario A, while more resources would be required to have full participation in even one large dark energy project, PASAG advises not to reduce the Dark matter project investment below a level critical to maintain leadership. As the Dark matter experiments scale up in size, it is important to have at least one frontier-sensitivity experiment operating at all times throughout the decade. A discovery could be imminent.
- Continued support for theoretical research is an essential part of a strong particle astrophysics program.
- Cosmic Microwave Background measurements are important to particle physics as a unique probe of the extremely high-energy processes associated with Inflation. Given the central importance of the CMB to our understanding of energy, matter, space, and time, and the unique contributions HEP can provide to CMB science, small investments are highly recommended in all budget scenarios, if the prioritization criteria in Section 2 are clearly met.
- The U.S. has played a leading role in the study of high-energy cosmic particles (cosmic rays, gamma rays and neutrinos) from space. This field sits at the interface between high-energy physics and astrophysics, enabled by techniques and personnel drawn from both areas. The main goals of the field are to understand the acceleration processes in cosmic sources that produce particles with energies well beyond what can be achieved on Earth and to use these particles to search for physics beyond the Standard Model. AGIS well exemplifies this interdisciplinary nature, having significant capability for indirect detection of dark matter in addition to its main goal of exploring the TeV gamma-ray sky. The novel AGIS design concept has the potential to offer much better instrument performance over the baseline design of the planned European-led Cherenkov Telescope Array (CTA). Given the expense, only one large array is likely to be built. The AGIS and CTA teams are discussing ways to merge efforts. To make sense programmatically and technically, and to maximize the effect of a U.S. investment, AGIS and CTA should move quickly toward a joint project. AGIS is also under review by the Astro2010 Survey; should it be highly ranked in that study, it would be expected that a significant fraction of the AGIS cost would be borne by the U.S. programs in astronomy and astrophysics.

- Establishing the high-energy cutoff in the cosmic ray spectrum was a great achievement of the past decade. This also fundamentally changed the intellectual landscape for the study of the highest energy cosmic rays, removing the need to explain them with new physics such as exotic, massive particles or topological defects at the GUT scale. Now, the scientific focus is on finishing the quest to determine the astrophysical origin of the highest energy cosmic rays. Auger North is “shovel-ready”, and the world is looking to the U.S. for leadership. The Astro2010 survey is ongoing: Auger North may be highly ranked in that survey, in which case astronomy and astrophysics agencies will presumably then plan to fund it and the costs to HEP will be lower. If not, then Auger North can only be substantially supported by HEP in the best funding scenarios.
- Given the current status of the proposed Deep Underground Science and Engineering Lab (DUSEL) and the uncertainty in the funding that could be made available, PASAG chose not to assume the funding of experiments through DUSEL in the budget planning exercises, even though the U.S. dark matter program would be greatly strengthened by it. DUSEL is central to the future dark matter and neutrino experimental programs, both of which require large underground laboratories. DUSEL would provide a unique location with needed infrastructure in the U.S. In addition, the funding for dark matter that may be available when DUSEL goes forward would enable key enhancements of variety, scope and schedule of the program.

In summary:

- Dark matter and dark energy remain extremely high priorities.
- Dark energy funding, which receives the largest budget portion, should not significantly compromise U.S. leadership in dark matter, where a discovery could be imminent.
- Dark energy and dark matter funding together should not completely zero out other important activities in the particle astrophysics program. The recommended programs under the different scenarios follow the given prioritization criteria.
- The priorities are generally aligned with the recommendations for the Cosmic Frontier given in the 2008 P5 report.

2 Process and Report Outline

Each subfield within the PASAG scope has a unique history and a unique set of issues. Some subfields and projects have been reviewed in detail recently, while others have not. It was also not practical in the time available to hear presentations from all the projects. Given these issues, and the charge, the panel defined the following goals and methodologies for the assessments in the subfields:

- **Dark Matter.** The 2007 Dark Matter Scientific Assessment Group report provided a detailed survey of experiments designed for direct detection of dark matter along with a roadmap for future investments. To obtain the information needed to update the DMSAG report, PASAG issued a request for written information from the experiments.
- **Dark Energy.** Several panels, including the 2005 Dark Energy Task Force (DETF) and the 2007 Beyond Einstein Program Assessment Committee (BEPAC), have evaluated dark energy goals and a subset of the proposed projects. The two large projects that would have HEP funding -- LSST, which is very well defined, and JDEM, in its various forms -- have been extensively reviewed. A moderate-scale project, BigBOSS, is very new, so PASAG heard a presentation from that project. A coherent overall strategy, optimizing observations both from the ground and space, taking into account the priorities of both the astronomy and physics communities, has been lacking. As described in Sections 1 and 6, PASAG is not constituted to do this. However, as dark energy is a very high scientific priority, PASAG sought to define the scope of dark energy within the broader particle astrophysics program. The detailed allocation to projects in the different budget scenarios awaits a coherent plan. The Astro2010 Survey, which is ongoing, will presumably play a key role in this planning. As input, issues of importance to HEP for participation in dark energy projects are provided in Section 6.
- **High-energy Cosmic Particles (cosmic rays, gamma rays, neutrinos).** This is a broad area with many new results, but there has not been a devoted scientific assessment group. PASAG therefore issued a request for written information that was similar to the one for dark matter and, based on the responses, invited the major projects in this area to make presentations.
- **Cosmic Microwave Background.** This is a broad area of research, primarily funded by agencies other than those HEPAP advises; however, small investments by HEP have had a large and visible impact. PASAG was specifically asked to comment on one project seeking HEP support, QUIET II, which also made a presentation. To make this assessment, PASAG also reviewed the overall importance of the science of the CMB to particle physics.

Prioritization Criteria

- **The science addressed by the project is necessary**
  - Addresses fundamental physics (matter, energy, space, time).
  - Anticipated results: either at least one compelling result or a preponderance of solid, important results. Check that anticipated results would not be marginal, either in statistics or in systematic uncertainties, relative to the needed precision for clear science results.
  - Discovery space: large leap in key capabilities, significant new discovery space, and possibility of important surprises.
- **Particle physicist participation is necessary**
  - Transformative techniques and know-how to have a major, visible impact; project would not otherwise happen.
  - Leadership is higher priority than participation
- **Scale matters, particularly for projects at the boundary between particle physics and astrophysics.**
  - Relatively small projects with high science per dollar help ensure scientific breadth while maintaining program focus on the highest priorities.
- **Programmatic issues:**
  - International context: cooperation vs. duplication/competition.
  - Readiness, risk, timeliness

Figure 2-1 Criteria developed for the prioritization process.

With these, and the budget scenario definitions, the panel performed the very difficult exercise of optimizing the overall particle astrophysics program within the PASAG scope under each of the budget scenarios described in Section 1. All of the major projects under consideration are of very high merit, but they certainly do not all fit in all of the budget scenarios. The budget exercises therefore helped the panel to focus on priorities. To make the difficult choices in a systematic way, PASAG first developed the set of prioritization criteria shown in Figure 2-1.

When relevant, other programmatic issues were also considered. In all cases, however, scientific importance was the primary deciding factor.

The resulting programs in the different budget scenarios are summarized in Section 1, along with the rationale and other accompanying issues to note. In the following sections, more details are given about the programs in the areas of Direct Dark Matter detection (Section 3); High-energy Cosmic Particles (Section 4); Cosmic Microwave Background (Section 5); and Dark Energy (Section 6).

The PASAG charge and panel membership are given in the Appendix, along with the list of meetings and presentations.

3 Opportunities in Direct Dark Matter Detection

3.1 Physics opportunities

The direct detection and understanding of dark matter remains one of the most important scientific priorities of particle physics. The evidence for dark matter is clear, but so far it has been inferred only through its gravitational influence and its origin and nature are unknown. The existence of dark matter implies new particles beyond the Standard Model. Two leading candidates for dark matter are Axions and weakly interacting massive particles (WIMPs). These are well motivated, not only because they resolve the dark matter puzzle, but also because they simultaneously provide solutions to longstanding problems associated with the Standard Model of particle physics. In addition, since dark matter constitutes the majority of mass in the Universe, it plays a major part in the formation of large-scale structure.

The theory of the strong interactions naturally predicts large CP violating effects that have not been observed. Axions would resolve this problem elegantly by suppressing CP violation to experimentally allowed levels. Cosmology and astrophysics set the allowed Axion mass range from 1 μeV to 1 meV, where the lower limit follows from the requirement that Axions do not overclose the Universe, and the upper limit is set by stellar evolution constraints, Supernova observations, and accelerator-based searches. In a static magnetic field, there is a small probability for cosmologically produced Axions to be converted by virtual photons to real microwave photons by the Primakoff effect. This would produce a faint monochromatic signal with a line width of dE/E of 10^{-6}. The ADMX experiment, for example, consists of a high-Q microwave cavity tunable over GHz frequencies to search for this effect.

WIMPs have masses and interaction cross-sections of order of the electroweak scale [ten's of GeV to a few TeV] and are widely believed to be the most promising particle physics candidates for cold dark matter. Such particles appear naturally in models of new physics (e.g. Supersymmetry or Extra Dimensions) independently motivated by attempts to understand electroweak symmetry breaking and which usually introduce an extra discrete symmetry such that the lightest non-Standard Model particles are stable. In most models, WIMP interaction cross sections are sufficiently large that they would have been produced and annihilated for some period of time in the early Universe, therefore being in thermal equilibrium. The assumption of thermal equilibrium during that period allows a precise prediction of the relic density - assuming standard cosmology at the early epoch - compatible with CMB (*e.g.*, WMAP) measurements. It is an amazing coincidence that under such assumptions the estimate for the scale of new physics to explain dark matter coincides with the scale at which we expect new physics in relation to electroweak symmetry breaking.

There are three major thrusts to detect WIMP dark matter: direct dark matter detection, astronomical “indirect” dark matter signals, and dark matter particle production at colliders. None of these approaches alone is capable of providing a complete understanding of dark matter. The rates of direct detection experiments depend on the local density and velocity distribution of dark matter particles, which may be revealed by the astrophysical indirect detection experiments. In turn, signals of astrophysical WIMP annihilation cannot be related to dark matter density without postulating couplings and branching ratios, which are highly model dependent. At colliders, events with missing energy may provide evidence for new, weakly interacting neutral particles, but one is unable to prove that those particles have all the right properties to constitute the dark matter – in particular, the stability of such particles on cosmological scales cannot be verified. Information from all three probes is therefore essential for a complete understanding of dark matter.

With the imminent start of the LHC, a unique opportunity is opening for the creation and detection of dark matter at the energy frontier. It is therefore prime time to invest in the other two avenues, which are at the cosmic frontier, at the appropriate levels to allow a breakthrough in our understanding of roughly 23% of our Universe.

3.2 Issues for WIMP detection

PASAG is concerned with the search for dark matter via direct detection and through the end products of a cosmic WIMP-WIMP annihilation (“indirect” detection). The following subsections are devoted to direct detection, and astrophysical indirect detection is discussed in Section 4.

The field of dark matter direct detection has expanded rapidly in the last several years, with new ideas and new technologies competing for the lead in detection sensitivity. The capabilities of these new technologies are being explored with a succession of more sensitive, and therefore more massive, detectors. The sensitivity of the current generation of detectors is approaching a spin-independent cross-section of 10^{-45} cm^2 for WIMP masses of ~100 GeV, a factor 10 improvement over previous limits. This sensitivity is beginning to probe an interesting region in which some theories suggest a signal might be found. Thus, in addition to providing critical R&D stepping-stones toward very large-scale detectors, the current and next-generation (G2) detectors, with a combined two-orders-of-magnitude improvement in sensitivity for spin-independent cross-sections, may at any time produce a major dark matter discovery.

The current generation of experiments should reveal the strengths and limitations of the various techniques and the results can be used to plan the next, larger and more sensitive experiments which will probe WIMP-nucleon cross-sections of 10^{-46} cm^2 and smaller.

The energy spectrum and density of WIMPs depend on the distribution in the Galactic halo. (A smooth, spherical distribution with a Maxwell-Boltzmann velocity distribution is usually assumed, though structure formation simulations suggest clumping should occur even at relatively small scales.) The experiments are designed to detect the elastic nuclear scattering of the WIMP with the nucleus. Since the WIMPs have a low velocity, the interaction with the nucleus is a coherent interaction. For a spin-independent interaction the cross-section increases very rapidly with atomic mass number, A, but very large-A targets lose the coherent enhancement more quickly than do lower-A targets as the WIMP recoil energy increases. In the case of ^{131}Xe, for example, the form factor suppression due to the loss of coherence is about a factor of 20 at a recoil energy of 40 keV. For a spin-dependent interaction one needs non-zero spin nuclear targets such as ^{73}Ge. For most targets in use, the scalar interaction gives more sensitivity. The present cross-section limit for spin-dependent interactions is of order $10^{-38}cm^2$. The overall expected rate is very small ($\sigma=10^{-42}$ cm^2 gives about 1 event/kg/day, whereas some models predict values below $\sim10^{-48}cm^2$ for spin-independent interactions).

This presents extraordinary experimental challenges. The WIMP signal is a low-energy (10-100 keV) nuclear recoil. A large, low-threshold detector that can discriminate against the various backgrounds is required. Background photons scatter off electrons, while WIMPs and neutrons scatter off nuclei. In addition, it is necessary to minimize both internal radioactive contamination and external incoming radiation. A deep underground location is especially important for dark matter experiments.

Experimental techniques include detectors that record ionization, scintillation light and phonons. The most sensitive of the detectors employ multiple techniques, and the interplay of each is used to discriminate against backgrounds. The Cryogenic Dark Matter Search (CDMS) Collaboration has pioneered the use of both phonon and ionization detection in low temperature Ge or Si crystals. These detectors have excellent event-by event background rejection. In the last several years, the field has been further energized by the emergence of noble liquids (argon, xenon, neon) in various detector configurations, as well as new ideas for use of warm liquids and various gases under high or low pressure. These offer several possible advantages, such as an increased reach in sensitivity enabled by large, homogenous detectors with diverse background control methods (*e.g.*, single phase vs. two-phase in noble liquids and various combinations of multiple signatures). There is also a range of target types suitable for establishing a WIMP signature. The XENON10 and WARP experiments are examples of two-phase cryogenic liquid detectors, while the MiniCLEAN detector is an example of a single-phase detector.

3.3 History and status

The last detailed technical review of direct dark matter detection was completed in the 2007 report of the DMSAG committee. The DMSAG panel report made several recommendations. These included continuing the ongoing CDMS and ADMX experiments and funding the expansion of the noble liquid experimental efforts to their next level. In addition, DMSAG recommended the development of superheated liquid detectors and detectors capable of determining WIMP direction. Several projects that were started or were ongoing at the time of DMSAG are still underway. A new technical review is needed but is premature at this time. This report of PASAG is based on written material solicited from the research groups for updates and future plans on their projects. A complete technical review should be organized as soon as scalability to the G2 level is demonstrated by any of the present-day projects. This should happen in the next 1-2 years.

The current detection upper limit for the WIMP-nucleon spin-independent cross section is 4.4 $\times 10^{-44}$ cm^2 for a WIMP mass of 60 GeV. This result is based on observing less than 1 nuclear recoil event in an exposure of 121.3 kg-days ($\sim 3\times 10^{-4}$ ton-years) with recoil nuclear energies from 10 to 100 keV, the range expected in WIMP-nuclear collisions. For WIMP masses below 40 GeV, the best spin-independent limits are also as low as $4\times 10^{-44} cm^2$. Future detectors discussed here will extend the reach for detecting WIMP signals by more than a factor of 1000 and will require a background of less than 1 event in an exposure of 1 ton-year. The technology for achieving this high level of sensitivity is under continuous development, but appears to be reachable in the next decade by a staged program of detectors of increasing mass, and decreasing background.

The dark matter research program is very diverse, but in the absence of a full review, our comments focus on the prospects for CDMS, and the liquid xenon and argon detectors, all of which are poised for significant advances in sensitivity.

The CDMS Detectors

CDMS has a proven technology and a history of cutting edge achievements. Superior multi-parameter background suppression makes CDMS an attractive future option. CDMS backgrounds come from a variety of sources, the dominant of which are gamma rays from components within the detector system (cryostat, lead shielding, etc.) and surface background events due primarily to betas from ^{210}Pb deposited on the detector surfaces (due to radon exposure), and external gammas that interact in the dead layer on the surface of the detector. Rejection of beta and gamma backgrounds is based on the ratio of ionization to phonon signals and the timing of the phonon signal. The development of the 1”-thick Super CDMS detectors and more recent tests of the double-sided iZIP detectors both aim to reduce surface backgrounds. Preliminary tests of the iZIP detectors carried out in an above-ground laboratory look very promising; the rejection factor for surface events is claimed to be of the order of 10^3, and the overall rejection factor is ~10^7. With this new advance in background rejection power and reductions in bulk background planned for the cryostat of the 100-kg SuperCDMS-SNOLAB experiment, CDMS technology may be poised to reach the ton-scale sensitivity.

Maintaining leadership in WIMP sensitivity will require a timely scale-up of the CDMS detector mass from its present ~5 kg mass to hundreds of kilograms. The CDMS plan includes upgrades to a detector mass of 15 kg in the SuperCDMS-Soudan detector and subsequently, to a 100-kg SuperCDMS-SNOLAB experiment. The related GEODM project proposes to deploy a 1-ton germanium array based on the new detectors under development for SuperCDMS-SNOLAB.

Besides the need for lower backgrounds and improved background suppression, the high cost and long delivery time of the detectors pose challenges for very large detectors.

In summary, the necessary developments and risks to CDMS are as follows.

- Bulk and surface backgrounds need to be reduced.
- The acceptance of good events should be increased from the current value, limited by a 30% fiducial volume.
- Detector production should be streamlined.
- Sensitivities of the noble liquid detectors may surpass projected CDMS sensitivities.

The CDMS collaboration is addressing these issues and significant progress has been made. To advance the CDMS technology, **PASAG recommends a technical review of SuperCDMS in FY2010 to evaluate the performance of the new detectors currently in operation at Soudan. Funding for the 100-kg SuperCDMS-SNOLAB experiment should begin as soon as the detectors meet the design requirements.** Tests of the iZIP detectors in SuperCDMS-Soudan are also highly desirable.

The Xenon Detectors

The liquid xenon detectors under development by the LUX and XENON collaborations are based on the XENON10 and ZEPPLIN experiments. The XENON10 detector records scintillation and ionization signals in a two-phase liquid-gas xenon time projection chamber detector. A central fiducial mass of 5.4 kg was selected from a total xenon mass of 15 kg. The non-fiducial xenon provides self-shielding to suppress external gamma-ray backgrounds. Additional background rejection is based on the ratio of ionization to scintillation signals. The background rejection factor for beta and gamma radiation is ~200 for energies of 10-20 keV and seems to improve below 10 keV, reaching ~1000 at about 5 keV. This improvement at low energies is particularly useful for xenon since the WIMP cross-section increases with lower energy owing to the onset of coherent scattering off all the nucleons.

While background rejection is not as powerful as in CDMS, the external background is smaller due to the self-shielding. The definition of an inner fiducial mass shielded by the outer xenon is made possible by measurement of the position of the event, which is accurately done with the two-phase TPC. The self-shielding against external gamma rays is very effective in xenon due to its high density, but a substantial fraction of the xenon (>50%), and the cost, is needed for adequate shielding. Self-shielding becomes more efficient as the detector size increases. Since the main source of external radiation is radioactivity in the array of photomultiplier detectors, the development of low-background photodetectors will reduce the amount of xenon needed for shielding, and the overall cost.

Beta or gamma background that originates in the xenon can be suppressed only by using the ratio of ionization to scintillation. The rejection of internal background in the xenon by this single rejection technique is considerably weaker than the combination of two rejection factors that are employed in CDMS, and it poses a risk for achieving the ideal “zero-background” requirement for dark matter detectors. Internal background due to radon emanation from internal detector parts, and other sources such as ^{85}Kr and muon induced radioactivity in the xenon, have been evaluated and are believed to be tractable, but remain a non-neglible risk to the ideal “zero-background” goal of dark matter detectors.

Another risk factor for liquid xenon detectors is the uncertainty in achieving the purity of the xenon needed to drift the ionization electrons over long distances. Though xenon is an “inert” gas, it is well known that xenon leaches impurities off surfaces, which shortens the drift distance due to attachment of the electrons to the impurities. Drift distances needed for the large ton-scale xenon detectors have not yet been directly demonstrated, but will be addessed in the LUX-350 and XENON-100 detectors currently underway.

In summary, self-shielding, scaling to large masses, high purity, and the precise definition of events in a TPC detector make liquid xenon an attractive detector material for future large-scale dark matter detectors. Risks concerning background rejection and ionization drift distance are difficult to judge, but will be addressed in the current program of LUX and XENON. **A future xenon program that avoids duplicate efforts and meets the technical requirements**

for low background should be supported in any of the funding scenarios. The high cost of xenon and its unstable price, however, consitute a financial risk that is difficult to evaluate.

The Argon Detectors

Argon has some of the same virtues as xenon for dark matter detectors: an inert high-purity material with excellent scintillation and ionization properties, possibility of multi-ton unsegmented masses, possible operation as a TPC detector with precise localization of each event, and background rejection of beta/gamma events based on the ratio of ionization and scintillation signals. An additional feature of argon is pulse shape discrimination, a method to reject background that is very powerful in liquid argon but not very effective in liquid xenon.

A disadvantage of argon relative to xenon is the smaller atomic number and lower density. Another negative feature of argon is the radioactive ^{39}Ar present in argon taken from the atmosphere, the source of commercial argon. The ^{39}Ar is produced by cosmic ray interactions in the atmosphere and has a decay rate of ~1 Bq/kg. With a half-life of 260 years it is an intrinsic background unless it can be removed by isotopic separation methods or, as seems more practical, other sources of argon can be found with low levels of ^{39}Ar.

The ^{39}Ar background in commercial argon imposes requirements for background rejection that limit the detector size to ~1 ton. A significant advance in argon technology for dark matter detectors was the recent discovery of underground argon sources that are depleted in ^{39}Ar. The upper limit of ^{39}Ar is measured to be less than 4% of that in atmospheric argon. Because underground argon is shielded from cosmic rays, the ^{39}Ar could be much lower than the current upper detection limit. However, even at this level, the background due to ^{39}Ar is low enough to allow multi-ton detectors to be developed for dark matter.

The feasibility of producing useful quantities of underground argon has been established with a small-scale pilot plant. A production capacity of ~1kg/d was achieved and will be upgraded to produce ~100 kg of depleted argon for use in dark matter detectors. Production of tons of argon is possible, but will require a larger plant.

a. Single Phase Liquid Argon Detectors.

The DEAP/CLEAN collaboration is developing single-phase liquid argon detectors. The scintillation light from a large, unsegmented volume of liquid argon is detected with a 4π array of photomultiplier detectors. Background rejection of ^{39}Ar decay by pulse shape analysis requires a photon detection system of high efficiency that is still under development. Additional background suppression is achieved by defining a fiducial volume from the position of the events as determined from the hit pattern on the photo detectors. Surface events from the alpha decay of radon daughters are dangerous and must be rejected by a fiducial volume cut of high efficiency.

The DEAP/CLEAN collaboration reported background rejection based on data taken in an above-ground laboratory. In a 20-day run with 1.7×10^7 beta events acquired, no background events were detected in the nuclear recoil region for signals with 120-240 photoelectrons. Results agree with a simple model based on photoelectron statistics and signal electronic noise. Underground operation of the 7-kg DEAP-1 test detector is underway at SNOLAB to acquire more beta events, the goal being to demonstrate a rejection factor of 1×10^8 against electron recoils for a 1-ton single phase DEAP detector. Assuming an efficient detection of the scintillation photons (~10% efficiency, to be demonstrated), pulse shape discrimination is expected to reject the ^{39}Ar and other beta/gamma backgrounds in normal argon for the proposed 1-ton DEAP detector.

The Mini-CLEAN detector is a 500-kg single phase liquid argon detector under construction at SNOLAB. With normal argon and a fiducial mass of ~150 kg, the sensitivity to WIMPs is expected to be $\sim 2 \times 10^{-45}$ cm^2 for a WIMP mass of 100 GeV. The performance of the detector is based on a projected light yield of 6-7 photoelectrons per keV (12-14% efficiency) and a position resolution of 5-6 cm. These performance parameters are challenging and pose risks, but will be a major step forward in dark matter experiments if they are demonstrated. The full-scale CLEAN detector is anticipated to be operated with both argon and neon.

b. Two-phase Liquid Argon TPC Detectors.

The two-phase liquid argon TPC detector is similar to the xenon detectors described above, except that for argon there is additional background rejection by pulse shape discrimination. The first of these detectors for dark matter was the small WARP 3.2-kg detector operated at Gran Sasso (LNGS) in Italy. It had limited exposure and sensitivity to WIMPs ($\sim 10^{-42}$ cm^2) but achieved a high level of background discrimination: zero recoil background events were detected with a threshold of 32 photoelectrons (~30 keV) and 3×10^8 beta background events. The background rejection achieved is the highest demonstrated to date, not only in argon detectors, but also in CDMS and in xenon detectors. The WARP 140-kg detector, currently being commissioned at LNGS, is based on the technology developed in the small detector and is expected to achieve a sensitivity of $\sim 5 \times 10^{-45}$ cm^2 for spin-independent interactions with normal argon.

The proposed DAr detector is an instrument primarily to demonstrate the advantages of depleted argon (see description above). With ^{39}Ar at 4% of atmospheric argon the beta background in the nuclear recoil region of 10-100 keV (~2.5-25 keV) is ~150 counts/kg-day (5.5×10^7 counts/ton-year). At this rate the background is less than the current CDMS background and the rejection factor demonstrated in WARP can fully suppress the ^{39}Ar background in an exposure of multi-ton-years of target made of depleted argon.

The liquid argon technique may be especially promising with the use of depleted argon and should also be explored in any of the funding scenarios.

Spin-dependent Detection

While the main thrust in WIMP detection assumes spin-independent cross-sections, there are also significant efforts in spin-dependent detection, *i.e.*, a detector with spin-nonzero target nuclei. Some spin-dependent results come from detectors with a fraction of their isotopic composition in spin-nonzero isotopes. These include ^{27}Al, ^{73}Ge and ^{129}Xe. There are also detectors that search for a spin-dependent signal, for example COUPP. It uses a superheated liquid which will form a bubble if the energy deposition is sufficiently large and localized. COUPP is operated at sufficiently low pressure that it is intrinsically insensitive to electrons. A recoiling heavy nucleus, on the other hand, produces a very large dE/dx and a bubble is formed. While COUPP has promising reach for spin-dependent interactions, the range of target nuclei also makes it possible to vary the sensitivity to both spin-dependent and spin-independent interactions. If backgrounds from alpha particles and other effects can be controlled, it may be possible to build large volumes and surpass the sensitivity of other techniques.

Direction-sensitive Detectors

In the long-term future, the emphasis will be on obtaining some measure of the direction of the incoming WIMP particle. This will help to understand the local WIMP density and velocity distribution, which are crucial for the calculation of rates. The velocity distribution should change noticeably throughout the year due to changes in the earth's velocity with respect to the ambient WIMP density. There will also be a day/night variation. The DRIFT experiment, for example, uses a large volume of low-pressure gas (CS_2), which allows the nuclear recoil to leave an extended recoil track. This track can be imaged using advanced TPC technology, and the specific ionization, dE/dx, can be measured.

International Projects

In addition to the U.S.-led experiments, there are presently several other dark matter direct detection experiments, principally in Europe, Canada and Japan. These experiments are designed to probe similar regions of parameter space as the current U.S.-led experiments. Together, they are helping to provide the evidence as to which technique or techniques can be scaled-up with sufficiently low background. Those programs are making significant progress, with additional experiments expected. There is already significant collaboration, which is expected to grow, between these efforts and the U.S. groups.

Among the experiments in Europe, an experiment in the Gran Sasso laboratory, DAMA/LIBRA, a multi-crystal NaI detector, has claimed the observation of dark matter. DAMA reports an annual modulation with 8.2σ statistical significance and a phase that agrees with that expected from the standard Galactic dark matter halo model. Many possible systematic causes for this signal have been investigated, and it has been reported that none reproduce all of the observed signal characteristics. This signal is dramatically inconsistent with upper limits from other experiments for elastically scattering weak-scale WIMPs. However, the results could be compatible with various other dark matter models in which the scattering rate, which depends on the halo profile, becomes sensitive to the tail of the velocity distribution in specific ways that will vary with the mass of the target nucleus. Models of this type include inelastic dark matter, form-factor dark matter, and resonant dark matter. Other possibilities to reconcile the apparent discrepancy involve uncertainties in translating the observed DAMA signal to nuclear recoil energies that can be compared to other dark matter experiments such as CDMS, CRESST, KIMS, XENON10 and ZEPLINIII. The issue is not yet resolved, as regions of parameter space still remain that are compatible with the DAMA signal and all null results. Future efforts from xenon-based dark matter experiments that have a target nuclear mass similar to that of iodine are underway and could help to prove or disprove existing models. It would also be advantageous if a second experiment using NaI detectors as the target, but with preferably lower backgrounds, could either verify or refute the DAMA/LIBRA result. The present situation illustrates clearly the importance of having at least two confirming measurements for a measurement as challenging as dark matter scattering. The necessity of confirmation is one of the guiding principles for these recommendations.

3.4 Findings and recommendations for WIMP detection

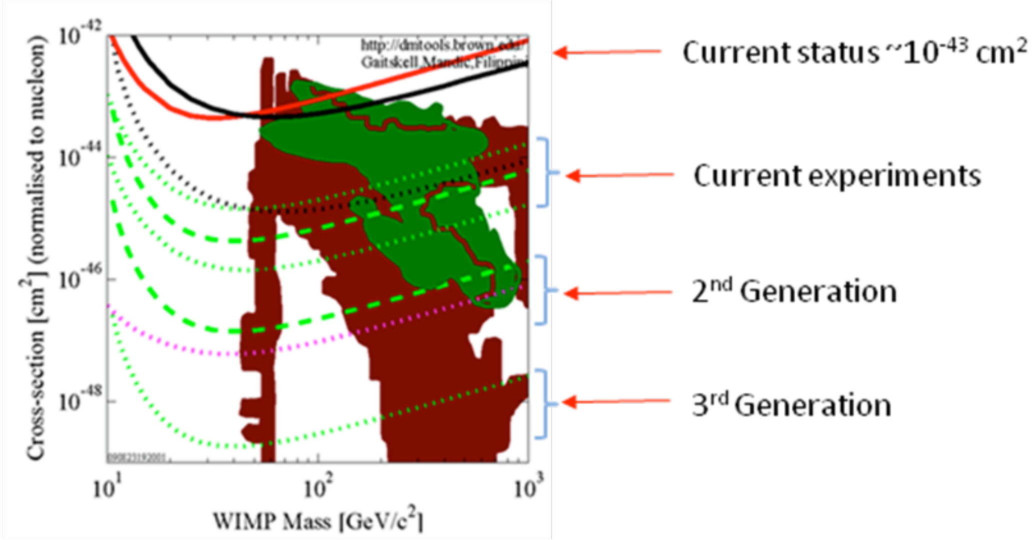
Affirming the DMSAG recommended strategy, a push to complete at least two experiments of differing target materials or technology, improving sensitivity a factor of 10 to 100 over present limits, should be the primary near-term goal. At the same time, aiming for the longer term and next level of sensitivity, R&D should be conducted on all techniques with potential for scalability and/or background control (such as true directionality).

The DMSAG report (July 2007) recommended another review in 2009 to assess the progress and suggest the technology choices to be followed in the future. Although there has been significant progress, it may still be a few years before the choices are clear. The results from 100kg-scale noble liquid detectors are still not available.

The current exploration of techniques is crucial to maintain, and it is paying off. The U.S. experiments are presently leading the field in sensitivity in two or more of the major techniques (e.g., ADMX, CDMS, XENON10). **At the larger and more expensive scales, there should be a consolidation of groups that are focused on the most promising technologies. In addition, smaller scale R&D should be supported to enable more discriminating detection techniques that will be necessary if a signal appears.**

A sequence of U.S. projects, with 2-3 second-generation detectors covering the major technologies (CDMS and cryogenic liquids) and 2 third-generation detectors is optimal. More details for each of the budget scenarios are given below. Experiments should move forward as soon as they demonstrate essential technical requirements. Plausible starting years for construction of second-generation and third-generation detectors are 2013 and 2017, respectively. **An essential feature of this program is a sequence of detectors with increasing mass, operating with multiple background rejection tools, and crosschecks. A final configuration of two large G3 detectors with independent targets would assure a clear interpretation of a signal.**

The second-generation detectors should have sensitivity for detecting WIMPS with spin-independent cross-sections of 10^{-46} cm^2 or lower, while the third-generation should surpass 10^{-47} cm^2 (see Figure 3-1).



Current status ~10^{-43} cm^2

Current experiments

2nd Generation

3rd Generation

Figure 3-1 The range of experimental sensitivity that can be reached in the sequence of detector developments. The experimental ranges are shown overlapping the solid colored projected regions of the minimal supersymmetric models. Current experiments include SuperCDMS, Xenon100, LUX350, MiniCLEAN, and WARP140. Note that the current generation is already reaching deeply into the projected WIMP cross-section range. A discovery could be imminent.

To advance the CDMS technology, **PASAG recommends a technical review of SuperCDMS in FY2010 to evaluate the performance of the new detectors currently in operation at Soudan. Funding for the 100-kg SuperCDMS-SNOLAB experiment should begin as soon as the detectors meet the design requirements.**

A future xenon program that avoids duplicate efforts and meets the technical requirements for low background should be supported in any of the funding scenarios.

The liquid argon technique may be especially promising with the use of depleted argon and should also be explored in any of the funding scenarios.

3.5 Specific findings and recommendations for Axion detection

ADMX completed phase-I construction and is operating well. It is estimated to take a total of 1-2 years to cover 10^{-6}-10^{-5} eV down to the first of two model benchmark sensitivities (KSVZ). Phase II of the experiment will cover the same range down to the lower model (DFSZ). This phase requires a dilution refrigerator to go from 1.7 to 0.2 K. This is a unique experiment, and its continuation through phase II is supported in all budget scenarios.

3.6 Budget scenarios

In all budget scenarios, the Xenon100 upgrade, the LUX350 detector, an effort on DAr, funding for the MiniCLEAN detector, the additional towers in SuperCDMS Soudan, the COUPP 500 construction, the 100-kg SuperCDMS-SNOLAB experiment and the phase II upgrade to ADMX are supported. These experiments will allow an exploration of the cross-section region shown in Figure 3-1, as well as continued development of the other major promising directions discussed earlier.

All scenarios support the continued R&D into detectors with directional sensitivity. An important goal is to put a head on the arrow of the recoil track and actually get directionality. This could be very powerful both for background discrimination and for confirmation of the signal as really being due to dark matter. As cross-section limits improve, requiring larger detectors, the practical scalability of direction-sensitive techniques should be re-evaluated.

All budget scenarios also can support two second-generation detectors. At least two such detectors are needed to guarantee continued U.S. leadership, given the risks associated with scaling up any given technology.

In the leaner scenarios, the progress of detector expansion after the second generation is severely limited, potentially slowing progress and risking loss of U.S. leadership. In the lowest funding scenario A, it may not be possible to support any of the third-generation experiments in this decade. In scenario B, only one third-generation detector may be possible in this decade. If that technique fails to achieve the required sensitivity, the U.S. program will be compromised. In scenario C the full complement of dark matter experiments is possible producing a robust program.

The Deep Underground Science and Engineering Laboratory (DUSEL)

The U.S. Dark Matter program and the experimental neutrino program depend crucially on DUSEL, both of which need large underground laboratory space. **DUSEL will provide a unique location with needed infrastructure in the U.S.** In addition, the funding for dark matter that may be available when DUSEL goes forward would enable key enhancements of variety, scope and schedule of the program. As an example, in scenario B, DUSEL funding may allow the construction of a second third-generation detector. However, the existence of DUSEL funding was not assumed by PASAG in its budget exercises.

4 Opportunities in High-energy Cosmic Particles (Cosmic Rays, Gamma Rays, and Neutrinos)

4.1 Physics opportunities

Astrophysical sources are able to accelerate particles to energies well beyond what can be produced on Earth. By studying the high-energy particles produced by these cosmic accelerators, we are exploring the physics of extreme conditions in the Universe. For example, it is surmised that in active galactic nuclei, a supermassive black hole powers jets of relativistic plasma flow that beams GeV and TeV gamma rays to Earth. These objects may also produce high-energy neutrino beams and cosmic rays with energies reaching 10^8 TeV.

Experiments that detect high-energy particles from space also have exploratory capabilities of importance for particle physics. A key aspect of these experiments is that they have sensitivity to various new physics scenarios over a large energy range (10^9 – 10^{20} eV). For example, the same theories designed to explain electroweak symmetry breaking being probed by the LHC also predict new dark matter particles that can annihilate to produce high-energy particles from space. Gamma-ray and neutrino telescopes operating at GeV and TeV energies can detect WIMP annihilations occurring in sources where a relatively high density of dark matter is concentrated (*e.g.,* in the Sun, in the Galactic halo, and in nearby satellite galaxies). With their potential for the indirect detection of dark matter, these instruments provide important complementary capabilities to the LHC and direct-detection experiments.

Other examples of exploring physics beyond the Standard Model come from the potential of using beams of high-energy particles as probes of new physics. Cosmic-ray and neutrino detectors sensitive at energies above 10^{15} eV have the possibility of detecting unexpected changes in the interaction cross-sections of the proton or neutrino, respectively, with matter. GeV and TeV gamma-ray detectors can provide limits on the violation of Lorentz invariance for photons that travel cosmological distances.

The study of high-energy particles from space has long been closely connected with elementary particle physics, having detection techniques in common and using Nature's particle beams to make new discoveries. This is a very active field at present at the interface between astrophysics and high-energy physics, comprising studies of cosmic rays, gamma rays and neutrinos. Over the last decade, there have been major scientific achievements by cosmic-ray and gamma-ray instruments. In the area of high-energy neutrinos, the IceCube detector at the South Pole is poised to start full operations in 2011. The proposals before PASAG mainly seek to build upon the recent achievements in the areas of cosmic rays and gamma rays, with each area having new instruments of significantly enhanced capabilities and exploratory reach. These proposals come with high probability of obtaining important results of interest to astrophysics and with the potential for new discoveries of relevance for particle physics. All overlap with high-energy physics in personnel and instrumentation.

4.2 History, issues

The origin of the cosmic rays is a deep, 90-year old mystery that is still not completely understood. The ultrahigh energy (UHE) cosmic rays ($E > 10^{18}$ eV) have been studied by experiments with increasing aperture (area x solid angle) and resolution during the last few decades. These extremely energetic particles are detected on Earth via the extensive air showers they create upon interacting in the atmosphere. The previous generation of experiments consisted of either arrays of widely spaced surface detectors (e.g. AGASA in Japan) that sampled the particles reaching ground level or arrays of upward-looking optical detectors (e.g. Fly's Eye HiRes in the U.S.) that sampled the nitrogen fluorescence signal produced as the air shower develops in the atmosphere. The advantage of the surface-array technique is high duty cycle while for the fluorescence technique it is a more complete calorimetric measurement of the cascade energy.

The major scientific thrust of this area is to understand the sources, acceleration mechanisms and propagation of the cosmic rays at these extremely high energies. The energy dependence of the particle interactions is also of interest. The primary experimental measurements are the energy spectrum, particle composition and directional distribution of the incoming particles. The study of UHE cosmic rays gathered considerable attention in recent years following the report by AGASA of an apparent continuation of the spectrum out to 10^{20} eV (and possibly beyond), past the expected Greisen-Zatsepin-Kuzmin (GZK) cutoff near 6×10^{19} eV, where extragalactic hadronic cosmic rays would be expected to interact with the Cosmic Microwave Background. AGASA also provided evidence for anisotropy in the UHE cosmic-ray arrival directions. By contrast, the Fly's Eye HiRes found a steepening of the energy spectrum above 5×10^{19} eV and no strong evidence for anisotropy.

Once we knew about the existence of an abundant flux of high-energy cosmic rays, it was natural to conceive of gamma-ray (and eventually neutrino) telescopes that could directly detect the acceleration sites of the high-energy particles. High-energy gamma-ray detectors have developed along two parallel tracks: space-borne and ground-based. The previous space telescopes, most particularly EGRET on the Compton Gamma Ray Observatory in the 1990's, found several hundred astrophysical sources of GeV gamma rays; the major identified source types were pulsars, active galactic nuclei (AGN) and gamma-ray bursts (GRBs). The Fermi Gamma-ray Space Telescope, launched in June 2008, is the most advanced high-energy gamma-ray space telescope yet flown. Although it is still early in the mission, many exciting results from Fermi have already been reported, including the discovery of many new pulsars not seen to pulse at any other wavelength, the detection of multi-GeV emission from distant GRBs (also providing significant new limits on Lorentz invariance violation), the firm identification of numerous high-energy Galactic sources such as supernova remnants and X-ray binaries, the clear exclusion of GeV excess diffuse emission previously interpreted by some as a signal for dark matter annihilation in our Galaxy, and a precise measurement of the spectrum of cosmic-ray electrons and positrons up to 1 TeV. It is noteworthy that Fermi is the successful result of collaboration between DOE high energy physics groups and NASA astrophysics groups in the U.S., with similar partnerships reflected in the international Fermi collaboration that made the mission possible. Particle physicists and astrophysicists worked closely together on all aspects of the mission, including the design, construction and operation of the Large Area Telescope (LAT) detector. The success of Fermi is an existence proof that cultural differences between scientific and technical communities are not necessarily impediments, but rather reinforcing capabilities enabling important new opportunities.

Ultimately, the size of a detector that can be carried into space limits the maximum energy of gamma rays that can be detected. Ground-based instruments achieve much greater collection area (and hence sensitivity) by detecting the air showers produced when gamma rays interact in the atmosphere. Imaging atmospheric Cherenkov telescopes record the Cherenkov radiation produced in an air shower and use pattern recognition to separate the gamma-induced showers from the cosmic-ray cascades, thus achieving excellent background-suppression, good energy resolution, and high gamma-ray sensitivity. Modern Cherenkov telescopes have typical energy thresholds near 100 GeV. At higher energies above 1 TeV, air shower detectors, such as the recently decommissioned Milagro detector in New Mexico, sample the particles that reach ground level to enable the reconstruction of the shower direction and primary energy with moderate resolution. The main advantages of such detectors are high duty cycle and wide field of view.

The field of TeV astrophysics was pioneered by the Whipple 10m telescope in Arizona, which discovered the first sources of TeV gamma rays. The HEGRA experiment on La Palma, Spain, pioneered the stereoscopic array approach whereby the atmospheric Cherenkov radiation is viewed by multiple telescopes at different locations on the ground. An explosion of discoveries in the last five years has led to more than 80 clearly established sources of TeV photons, the large majority discovered by atmospheric Cherenkov telescopes. The sources include Galactic objects such as supernova remnants, pulsar wind nebulae, binary systems, and the Galactic Center. Extragalactic sources include many AGN and, most recently, starburst galaxies. Somewhat surprisingly, there are several dozen TeV gamma-ray sources that cannot be clearly matched to known astrophysical objects seen by longer-wavelength instruments. The mystery of these unidentified “dark accelerators” is an important one that may be resolved with the next generation of instruments. Cherenkov telescopes have also carried out searches for dark matter annihilation from nearby satellite galaxies at energies between 100 GeV and several TeV.

High-energy neutrinos are expected to accompany the high-energy charged particles and photons from most sources, particularly when the accelerated charged particles are hadrons. High-energy neutrino detectors for astronomy were pioneered by the Baikal neutrino telescope in Siberia and by AMANDA at the South Pole. There has since been significant progress, with AMANDA (now decommissioned) dwarfed by IceCube. This cubic kilometer detector will peer into a sensitivity region with a good discovery potential for astrophysical sources. It will also be sensitive to energetic neutrinos from annihilation of WIMPs that have been captured in the Sun; this technique is particularly valuable for detection of WIMPs with spin-dependent interactions with nuclei. So far, no clear source detections have been reported. Beyond the energies typical for IceCube’s sensitivity (TeV-PeV), neutrinos from cosmic-ray interactions with the CMB must appear in the EeV range. This region is the realm of radio detection in ice, or of air shower detection with Auger. First upper limits have been reported with both methods.

4.3 Findings and recommendations

4.3.1 Gamma Rays

As discussed earlier, the majority of our direct understanding of the very high-energy (VHE, E > 100 GeV) Universe comes from the many source discoveries made by imaging atmospheric Cherenkov telescopes. Three Cherenkov telescope arrays now dominate the field: H.E.S.S. in Namibia and MAGIC on La Palma, Spain, are European-led projects, while VERITAS in Arizona is U.S.-led. VERITAS is currently the most sensitive VHE gamma-ray detector in operation; however, both H.E.S.S. and MAGIC are in the process of significant upgrades. An upgrade to VERITAS to improve its sensitivity and capability at low energies was presented to PASAG; this proposed upgrade will be discussed at the end of this section.

AGIS

The current generation of atmospheric Cherenkov telescopes, including possible upgrades, will continue to produce excellent results over the next five to six years, thus very effectively overlapping with the Fermi Gamma-ray Space Telescope. However, given the great excitement in the field and the success of the technique, a more ambitious and likely very highly productive concept for the future is an array of many (~50) atmospheric Cherenkov telescopes distributed over a square kilometer. Such a detector array would produce an order of magnitude advance in sensitivity relative to the current instruments. It would also significantly expand the energy range of the ground-based technique, allowing the sensitive exploration of the gamma-ray band down to ~20 GeV, and up to ~100 TeV.

A major driving factor behind a 1 km^2 Cherenkov telescope array is the probable discovery of many new high-energy astrophysical sources. For example, it is estimated that such an array would discover ~300 new TeV Galactic sources and many hundreds of extragalactic TeV-class sources. Mapping the very high-energy Galactic source population would allow for a quantitative understanding of the processes that produce high-energy protons and electrons in the Galaxy, helping to decipher the mystery of the origin of cosmic rays. A large population of extragalactic sources would help us to understand the acceleration and emission mechanisms in relativistic jets and the jet-supermassive black hole connection in active galactic nuclei (AGN). The detection of distant AGN and gamma-ray bursts (GRBs) would enable the study of the intergalactic radiation and magnetic fields and would probe the nature of spacetime by testing the invariance of the speed of light over long distances.

An important scientific motivation for a large Cherenkov telescope array that directly relates to particle physics is the possible detection of GeV/TeV gamma rays from the annihilation of dark matter. There are a variety of possible sites that house large concentrations of dark matter, including the Galactic center, Galactic halo objects, nearby satellite galaxies, and external galaxies. A detection of this type would be profound as it could lead to the eventual mapping of the dark matter content of the Galaxy through its gamma-ray signature. The technique also has the potential to constrain the mass and couplings of the dark matter particle. The signal strength and accompanying astrophysical backgrounds depend on many parameters. One plausible example is shown in Figure 4-1.

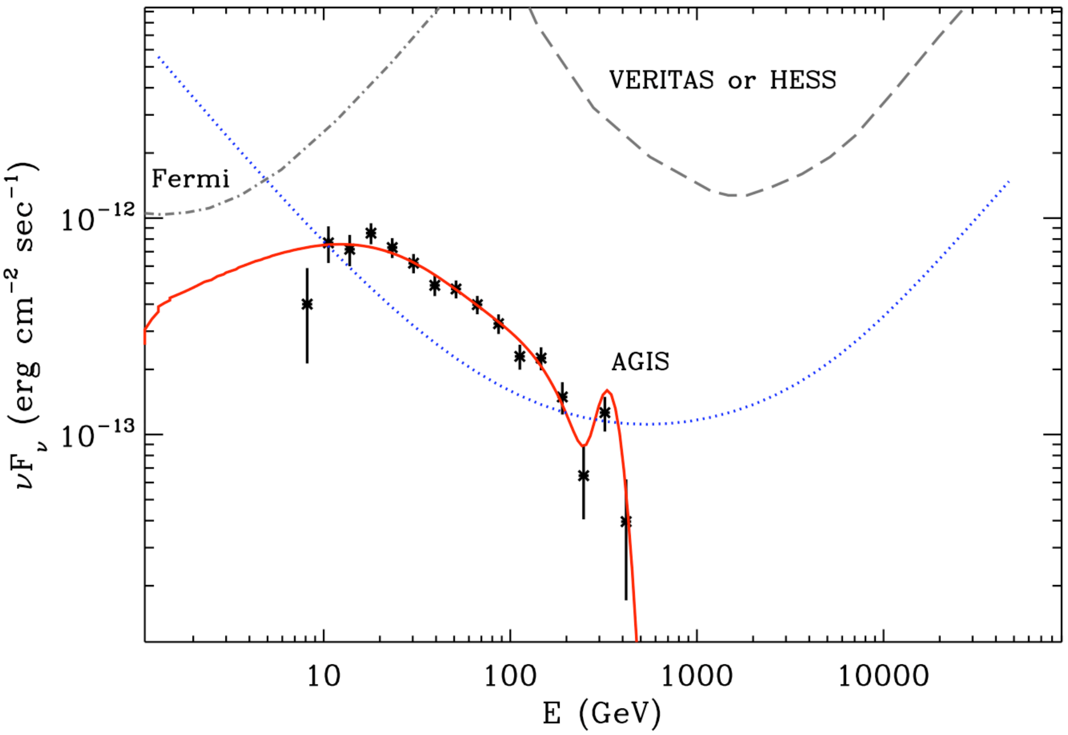


Figure 4-1 Simulation of the response of a 1 km^2 atmospheric Cherenkov telescope array to a signal of high-energy gamma rays produced by WIMP annihilation in a nearby dwarf spheroidal galaxy. For this figure, the distance to Ursa Minor Dwarf (66 kpc) and a boost factor of 20 (or the distance to Sagittarius Dwarf and a boost factor of 3) were assumed. The data points show the estimated signal for a plausible set of MSSM parameters with $<\sigma v>=2\times10^{-26}$cm^3/s. Other assumptions made are a 200-hour observation, a WIMP mass of 330 GeV, a mixed decay of 0.3 Tau-Tau and 0.7 b-bbar branching ratios, a line-to-continuum ratio of 10^{-2}, and a negligible background. The dotted blue curve, marked "AGIS", corresponds to a 5-sigma differential detection level, and the red line is a fit to the data points. Note that the simulation is for a generic 1 km^2 facility. Optimization for energy coverage and position resolution is ongoing.

The development of a large array of Cherenkov telescopes is being aggressively pursued both in Europe and in North America. The European-led Cherenkov Telescope Array (CTA), currently in the planning and prototyping phases, has a nominal design that uses telescopes of the conventional single-reflector design. Three different-sized telescopes, ranging from ~4m diameter to ~25 m diameter, are envisioned in the CTA design to cover the energy range from 20 GeV to 100 TeV. The U.S.-led Advanced Gamma Imaging System (AGIS) proposes to build an array of 36 telescopes, each having a primary mirror diameter of 11m and making use of a novel two-reflector design. The AGIS design concentrates on achieving maximal sensitivity in the important 100 GeV-10 TeV energy region. The main potential advantages of the AGIS telescope design over that of CTA are 1) higher angular resolution, 2) wider field of view (up to 8.0 degrees) and 3) very fast optics (small plate scale) that allows the construction of a compact camera using small pixel elements (*e.g.*, multianode photomultiplier tubes). The disadvantages of the AGIS telescope are an increased complexity of the optical design, a higher required quality of the optical surfaces, and therefore a possible higher eventual cost of each telescope. Since the AGIS design also requires an R&D and prototype phase, there is the important issue of its readiness relative to the more conventional CTA design.

The AGIS collaboration currently consists of scientists from 19 institutions in the U.S, Canada, Argentina, and Italy and from several U.S. national laboratories. The collaboration is presently involved in R&D focused on the optical and mechanical design of the telescope, the design of the focal plane instrumentation and trigger system, the simulation of the performance of the array, and a survey of potential sites. Although CTA and AGIS started separately, there has been considerable communication between the two groups during the formulation of each project. Now, it is generally understood on both sides of the Atlantic that a merger of the two projects should occur to develop a global effort.

The proposed timeline for AGIS calls for a 3-4 year prototype phase that is needed to demonstrate the feasibility of the telescope concept, followed by a construction period of six years. The overall cost of a stand-alone AGIS array of 36 telescopes, including the prototype phase and site development, is estimated to be around $200M. This cost cannot be borne solely by the HEP program in any of the funding scenarios. However, given the unique scientific potential of AGIS, the enhanced capabilities relative to the more conventional CTA design, and the historical leadership of the U.S. in this field, PASAG recommends that a significant level of funding be provided for AGIS in Scenarios B, C and D. The funding would be used to complete the critical prototype telescope phase and to construct a portion of the core array of telescopes. In Scenario A there are not sufficient funds to make a meaningful contribution to AGIS, so in this scenario the U.S. would effectively end its leadership role in a field that it originated.

PASAG also recommends that the AGIS collaboration work expeditiously towards a merger with the CTA effort. This is important for two reasons: first, it could enable a significant reduction in cost through the elimination of duplication (*e.g.,* the costs associated with site preparation and infrastructure, atmospheric monitoring, computing and networking, etc.); second, it would help establish a framework to allow for the possible inclusion of telescopes of the AGIS design into a joint array. A significant level of funding for AGIS from HEP in Scenarios B, C, and D, together with possible funding from non-HEP sources (*e.g.* NSF Astronomy), would enable the U.S. to have a leadership role in this important worldwide effort. AGIS is also under review by the Astro2010 Survey; should it be highly ranked in that study, it would be expected that a significant fraction of the AGIS cost would be borne by the U.S. Astronomy and Astrophysics program.

HAWC

A complementary technique to detect TeV gamma rays on the ground is via a water Cherenkov detector, or an array of such detectors, to sample the air shower initiated by the primary particle. This technique has the advantages over atmospheric Cherenkov telescopes of a wider field of view and a near 100% duty cycle. The disadvantages are a higher energy threshold, weaker sensitivity, and a greater level of background. The water Cherenkov technique was developed and used for the Milagro detector located in New Mexico, which operated until 2007. Milagro mapped the northern hemisphere sky, discovering a number of new Galactic sources at energies above 10 TeV; the Milagro results were surprising and very interesting because of the hard spectra of the detected sources, indicating that they could be responsible for producing an important fraction of the Galactic cosmic ray particles at these energies. More than a dozen of the Milagro sources are found in the vicinity of Fermi-detected pulsars, indicating a connection between the GeV power source and the multi-TeV accelerator. Milagro also detected diffuse gamma-ray emission from the Galactic plane and an interesting anisotropy in the cosmic rays whose origin is not understood.

The High Altitude Water Cherenkov (HAWC) gamma-ray observatory is proposed as a next-generation successor to Milagro. The key science goals are to detect and study TeV gamma-ray sources in the Galaxy, map the diffuse emission at these energies, study transient emission from bright AGN flares and nearby gamma ray bursts, and study the cosmic-ray anisotropy discovered by Milagro. Although HAWC is designed primarily for astrophysics, it has the potential to study topics of high interest to high-energy physics, for example by understanding nearby sources of cosmic rays that are backgrounds to the indirect detection of dark matter.

The proposed HAWC detector, which would be located at a high-altitude site in central Mexico, would comprise 300 large, closely spaced water tanks, each outfitted with three 20-cm photomultiplier tubes to detect the Cherenkov light of charged particles from gamma-ray and cosmic-ray showers as they hit the tanks. The design builds on the experience gained from Milagro, with the major improvements being a site at higher altitude, greater optical isolation of the photomultiplier tubes, and more deep-water area in each tank for better background rejection of cosmic-ray hadron initiated showers. As a result, HAWC is expected to achieve better energy resolution and more than an order of magnitude improvement in sensitivity over Milagro. This improvement factor is well motivated by the recent correlation of low-significance Milagro sources with Fermi sources.

The HAWC collaboration includes scientists from both U.S. and Mexican universities. The site selection and required permits have been completed and there are no major technical hurdles to be overcome. The construction and operation of HAWC is estimated to be at the level of $15M, including equipment, site preparations, and personnel, and the construction is envisioned to take 3-4 years to complete. Given its promising scientific potential and its moderate cost, the construction of HAWC is recommended in all four funding scenarios.

VERITAS Upgrade

As discussed earlier, VERITAS is a premier, currently operating ground-based VHE gamma-ray detector. The other instruments, HESS and MAGIC, are in the process of upgrade programs. An upgrade to VERITAS, consisting primarily of the addition of high quantum efficiency photomultiplier tubes and a new trigger system, has been proposed. This upgrade, at the level of $3M, would improve the sensitivity of VERITAS and would significantly increase its collection area at gamma-ray energies below 100 GeV. The upgrade is designed to be carried out relatively quickly so that the improved array would be online during the projected operational period of Fermi. Because of its relatively low cost and its leveraging of an existing, successfully operating experiment, the VERITAS upgrade is recommended in all four funding scenarios.

4.3.2 Cosmic Rays

The leading cosmic-ray experiment at present is the Pierre Auger South detector in Argentina. Auger South is a hybrid detector consisting of a ground array of 1660 water Cherenkov detectors distributed over an area of 3000 square kilometers and overlooked by four groups of atmospheric fluorescence detectors. Events observed in coincidence by both components allow a good energy calibration of the surface array. Auger South is operating extremely well and has met all its design parameters. Among its important scientific accomplishments, Auger South has confirmed the existence of a steepening of the energy spectrum and found evidence for anisotropy of the highest energy particles. The observed anisotropy indicates a correlation with cosmologically nearby concentrations of matter as marked by certain nearby AGN. Auger South has also put strong limits on the fraction of the primaries that could be photons, thereby excluding most top-down models for the highest energy cosmic rays. Results on the composition of the nuclear component (protons and helium compared to heavier nuclei) are of great current interest. Auger South and Fly’s Eye HiRes show consistent results for the average composition up to 10^{19} eV, above which the Auger South data suggest heavy primaries while the HiRes data suggest light primaries. The divergence is in the highest energy region where statistics are limited.

From its beginning, the Auger Collaboration envisaged a detector in the north to give full-sky coverage. The proposed Pierre Auger Observatory at the Northern Site (Auger North) intends to target the ultra-high energy cosmic ray frontier with an aperture seven times greater than that of Auger South, and to complete the full sky coverage. Understanding the primary composition above 10^{19} eV and the nature of the high-energy cutoff calls for significantly higher statistics. A major goal is to identify individual sources. With a data set of 2000 trans-GZK events, for example, if the present indication of correlation with distributions of AGN turned out to be correct, some bright sources with several tens of events are expected, potentially allowing spectral measurements of individual sources. The spread in arrival directions of energetic cosmic rays around the sources would carry the imprint of extragalactic magnetic fields. The accumulation of more events above 5×10^{19} eV from nearby sources would also allow confirmation that the steepening of the spectrum is indeed the expected cutoff due to energy losses of higher energy particles propagating through the CMB from distant sources and not an accident due to the accelerators reaching their maximum energy. These are the main motivations for Auger North, with the full-sky coverage being a second argument.

Auger North builds on the same concepts as used in Auger South. An array of 4,400 water Cherenkov tanks is proposed to cover 20,700 km², seven times the area of Auger South. A sparser spacing relative to Auger South reduces the cost but also raises the energy threshold. The higher threshold is in accordance with the goal of tackling problems around or above the GZK cutoff. Other cost reductions concern the instrumentation of the tanks (with one instead of three photomultipliers per tank) and a modified communication system. The number of fluorescence telescopes will be 39 (instead of the 24 at Auger South). The estimates of the cost reductions and the overall cost are on a firm ground since they are based on principles proven at Auger South.

The proposed Auger North site is in southeastern Colorado. The available infrastructure is more developed than the southern site and it adds another factor of cost reduction. The site was selected after a careful evaluation process of worldwide available candidate sites. With this site selection, a major international project funded mainly by international partners would be brought to the heartland of the U.S. The total project cost is estimated to be $127M, with the U.S. federal cost of $40M; a five-year construction period is estimated. Funding for Auger North is expected from all currently participating countries. The state of Colorado is expected to contribute to the infrastructure.

The present Pierre Auger Collaboration includes institutions from 18 countries. As with Auger South, the project office for Auger North would be at Fermilab. PASAG finds the science reach of Auger North to be important, and it recognizes the strong international support with the corresponding expectation that the U.S. would be the host site. However, given funding constraints and the scientific priorities of other projects, it is possible to fund Auger North from the HEP program at the requested level only in Scenarios C and D. The Astro2010 Survey is ongoing: should Auger North be highly ranked in that survey, astronomy and astrophysics agencies may substantially fund it and the costs the

HEP would be significantly lower.

The Fly's Eye HiRes group is collaborating with an international group to operate and exploit the hybrid Telescope Array (TA) detector in Utah that was largely funded by Japan. The aperture of TA is somewhat smaller than that of Auger South. A proposal to extend the reach of TA to lower energy (TALE) would allow a better measurement of the primary composition and energy spectrum with a single detector from the lower energy region dominated by particles of Galactic origin, through the transition to a population of more energetic extragalactic cosmic rays. Of the two cosmic-ray proposals, PASAG judged Auger North to have the higher priority because of the possibility to obtain a definitive answer to the origin of the highest energy cosmic rays and because Auger South already has some capability for measurements in the transition region. Alternative funding, via base grants and/or increased funding from astronomy sources, may enable the TALE project to go forward.

4.3.3 Neutrinos

The neutrino frontier is currently being explored primarily by IceCube, with ongoing smaller efforts in Russia (Baikal) and Europe (Antares) and future plans for a kilometer-cubed detector in the Mediterranean. With oscillation properties of atmospheric neutrinos now understood in light of the discovery of oscillations by Super-K, the main goal of these efforts is to detect neutrinos with energy >1 TeV from extra-terrestrial sources, which would directly point to sources of acceleration of hadronic particles, possibly including sources of observed cosmic rays. A second key goal is the search for dark matter annihilation to neutrinos.

An important connection exists between the highest energy cosmic rays and the so-called GZK neutrinos in the 10^{18} eV energy range that should be produced by interactions of the highest energy cosmic rays as they propagate from cosmologically distant sources. Two groups have submitted letters of intent to detect GZK neutrinos via the radio signal emitted from their interactions in Antarctic ice. The detection of GZK neutrinos would provide critical information on the nature of the sources of cosmic rays.

The existing concepts build on the experience gained with the RICE (a radio detector co-deployed with AMANDA at the South Pole) and ANITA (a balloon born radio detector that observed the Antarctic ice shield in two flights). The eventual goal of both collaborations is the construction of a radio array covering several hundred km^2 of ice. One group (ARIANNA) proposes to install an array of surface antennas on the Ross ice-shelf and the other (South Pole Radio) proposes to install a radio array at the geographic South Pole. These projects are in an early development stage and were not reviewed by PASAG.

4.4 Summary of opportunities in Cosmic Rays and Gamma Rays

Recommendations

- **Given its exciting science case covering topics of importance in astrophysics and particle physics, PASAG recommends significant funding for AGIS in Scenarios B, C, and D that would enable the construction of the prototype telescope and strong U.S. participation in a large array of atmospheric Cherenkov telescopes. PASAG also strongly encourages the AGIS and CTA groups to work together to develop a coordinated global effort to build the next major ground-based VHE gamma-ray facility.**

- **PASAG recommends the construction of HAWC and the funding of the VERITAS upgrade in all four budget scenarios. HAWC is a moderate-priced initiative that will carry out excellent astrophysics using a novel technique; there is also the possibility of surprising results of relevance for particle physics. The upgrade of VERITAS is a relatively low-cost way to improve the performance of an existing instrument to allow it to remain world-leading during the upcoming five to six years.**

- **Auger North addresses questions of great interest (namely the origin of the highest energy particles) using an established technique that builds on the success of Auger South. Given its relative science priority for HEP and the funding constraints, PASAG recommends significant HEP support for the construction of Auger North in budget Scenarios C and D.**

5 Opportunities in Cosmic Microwave Background Measurements

5.1 Physics opportunities

Over the past decade, a suite of path breaking ground-based and balloon-borne Cosmic Microwave Background (CMB) experiments, and a highly successful space CMB mission, have revolutionized cosmology. The subject has been transformed from an “order-of-magnitude” field to a precision science, with a wealth of quantitative data with precisely controlled errors, and detailed and robust comparison of theory with observations. More importantly for this committee is the nature of the science: while the techniques are those of radio astronomy, it is the view of PASAG that the current science targets of CMB research are fundamental to high-energy physics. The most prominent science target of CMB research is the understanding of Inflation, a period of accelerated expansion in the very early Universe. The flatness of spacetime and the nature and spectrum of primordial perturbations mapped out so exquisitely by the CMB point clearly toward Inflation. Although the mechanism driving Inflation (quantum gravity? string theory? GUTs? Peccei-Quinn symmetry breaking? compactification of extra dimensions?) remains unknown, it is clear that Inflation requires new physics beyond the Standard Model. For this reason, Inflation now occupies the attention of a very significant fraction of the particle/string-theory community---it is rare to find a theorist who does not devote at least part of her/his attention to the subject---and there is excitement that Inflation/CMB science may provide a unique empirical link to the physics of string theory.

The roadmap to furthering our understanding of the new physics responsible for Inflation is clear. This includes increasingly precise measurements of the power spectrum of primordial perturbations, their distribution (Gaussian? or otherwise?), and the search for inflationary gravitational waves, the “smoking gun” of Inflation. The most promising route to probing the spectrum and nature of primordial perturbations is the CMB (although galaxy surveys provide a valuable complement). The route to detection of inflationary gravitational waves is the distinctive B-mode signature they produce in the CMB polarization.

Even the “secondary science” of the CMB is fundamental to high-energy physics. Precise measurements of the CMB power spectra (temperature and polarization) are necessary input to the interpretation of dark energy measurements and increasingly precise probes of neutrino mass; in fact, such measures of Large Scale Structure may be our only probe of the absolute mass scale for neutrinos, if Nature does not choose to provide us with double beta-decay. Measurements of the weak lensing (“cosmic shear”) of the CMB will probe the growth of structure at small scales and intermediate redshifts, not accessible with galaxy surveys, and thus provide tests of modified-gravity models introduced to explain cosmic acceleration.

While the science of the CMB is high-energy physics, the original motivation and measurement techniques have evolved from radio astronomy. Even so, HEP-funded physicists have played an important role in several of the most significant developments, including one that was awarded a Nobel Prize. DOE supercomputing resources were also used to analyze the data in the revolutionary suborbital year-2000 experiments that first clearly mapped the acoustic-peak structure in the CMB power spectrum. More recently, LDRD funding at national labs have helped the development of instruments for the next generation of CMB experiments.

Experimental work on the CMB is now along a few clear directions. There is the recently launched Planck satellite, which will for the next half decade be the flagship project in the field. However, there is a broad range of sub-orbital experiments that complement Planck. These include (a) experiments with finer angular resolution over smaller regions of the sky; (b) searches that optimize for the inflationary-gravitational-wave B mode signal by digging deeper into the polarization; (c) measurements at a variety of frequencies that characterize the astrophysical foregrounds; and (d) projects aimed at the development of novel detector technology. Space-mission concepts for a post-Planck satellite aimed at inflationary gravitational waves and/or weak lensing of the CMB are also being vigorously pursued.

Astrophysics programs at NASA and NSF fund the majority of current CMB research. However, HEP scientists have in recent years become attracted to this field, not only because the intellectual goals are in high-energy physics, but also because HEP scientists have unique technical expertise, increasingly in demand by CMB science, to contribute.

5.2 Findings and recommendations

PASAG has been asked to comment on request for a relatively small level of support for Fermilab participation in QUIET II, a CMB experiment located in Chile that aims to make sensitive low-frequency measurements of the CMB polarization over intermediate angular scales. To make these measurements, QUIET implements a novel technology that allows thousands of radiometers to be mass produced and packed into the focal plane of the telescope.

QUIET II is an important project to pursue as it makes use of a pioneering technology (MMICs) and covers a unique range in the frequency—multipole-moment parameter space. The project includes among its leadership several outstanding particle physicists from both university groups and from Fermilab.

While QUIET I has been pursued primarily with non-HEP funding, HEP has unique capabilities to offer the project as it moves to the next step, QUIET II:

(a) Fermilab has unique large-scale fabrication capabilities required to mass-produce the detectors;
(b) HEP scientists have valuable experience with the high-speed electronics the project will require;
(c) the approaches to data analysis and related capabilities that have been developed for particle physics experiments will become increasingly important to CMB science as the scope of CMB experiments, and scale of the collaborations, increases.

Recommendations

PASAG recommends that QUIET II be supported at the proposed scope under all budget scenarios.

Given the central importance of the CMB to our understanding of energy, matter, space, and time and the unique contributions HEP can provide to CMB science, PASAG further recommends that **the future upgrade path for QUIET II should be considered for support at the appropriate time.**

Several of the national labs and other institutions now have small groups active in this area. Additional investments in CMB projects should be made when the HEP community can provide unique capabilities. Relatively small (up to ~few M$ per year) investments in CMB research would be appropriate, if the prioritization criteria are clearly met.

6 Opportunities in Dark Energy Studies

6.1 Physics opportunities

The accelerating expansion of the Universe was Science Magazine's discovery of the year in 1998. Subsequent observations have independently confirmed and reinforced this remarkable finding. PASAG reaffirms the 2008 P5 Report statement that the study of dark energy is central to the field of particle physics.

We know of two possibilities that could account for the accelerating expansion: either three quarters of the energy density of the Universe is in an unknown form, called dark energy, or general relativity breaks down on cosmological scales and must be replaced with a new theory of gravity. Either way there are profound implications for our understanding of the cosmos and of the fundamental laws of physics.

The study of dark energy has the potential to guide the reconciliation of quantum theory and general relativity. Dark energy could be the energy of the vacuum, equivalent to Einstein's cosmological constant. Although sometimes considered the simplest model for dark energy, conventional particle physics theory predicts that the vacuum energy density should be many orders of magnitude larger than the value that would account for the present acceleration. This mismatch is a profound challenge to our understanding of quantum reality. Alternatively, dark energy could signal the existence of a new type of scalar field and associated particle not in the Standard Model, an idea known as quintessence.

While the nature of dark energy is unknown, a well-defined set of first questions has emerged: Is dark energy the cosmological constant? Is it energy or gravity? Do its properties evolve over time? Dark energy experiments address these questions by studying the impact of dark energy on both the history of the cosmic expansion and the growth of large-scale structure. As we measure these known effects of dark energy with increasing precision using diverse methods, inconsistencies in the results may point to possible deficiencies in General Relativity.

Signatures of neutrino mass and early-universe physics will also be measured in the large-scale-structure measurements of advanced dark energy experiments. Beyond particle astrophysics, these experiments will provide a wealth of astronomical data, including the most exhaustive inventory of the solar system yet undertaken, the most detailed study of the structure of our galaxy yet performed, and the largest survey of the extragalactic cosmos to date.

The panel recognizes the importance to dark energy science of cooperative efforts between particle physicists and astrophysicists and cosmologists. This partnership has always proven fruitful in the exploration of high-energy particles from space and has proved useful in the short history of the study of dark energy. Noted below are reasons why the panel expects particle physics expertise to be enormously useful in the full exploitation of the large datasets expected for stage-IV experiments initiated in the coming decade. These advanced dark energy experiments also depend essentially upon extension of the theoretical and instrumental techniques developed by the observational cosmology community over the past decades. The panel fully expects that the contributions of the particle physics and the astronomy communities working together offer the best chance of thoroughly exploring dark energy and perhaps divining its nature. Such partnerships are already working effectively within the LSST and BigBOSS projects.

Since the co-discovery of dark energy by DOE-supported scientists, the particle physics community has contributed intellectual leadership to the dark energy program. Many members of the particle physics community have leadership and management roles in the major past, present, and proposed dark energy experiments.

Particle physicists bring unique experience in large-scale experiments. The BigBOSS, JDEM and LSST stage-IV dark energy projects being considered for construction in the next decade will produce results that have greatly reduced statistical uncertainties compared to the current state of the art. With this in mind, these experiments are being designed to have experimental systematic uncertainties that are as small as possible. Particle physicists have been playing an important role in this design process, bringing with them decades of experience designing, simulating and executing successful large-scale experiments.

Particle physicists developed the fully-depleted CCD technology expected to be used in LSST and possibly in JDEM. At the heart of JDEM and LSST are large area, state-of-the-art high-channel-count solid-state photon detector arrays and associated readout electronics. Conventional astronomical CCDs are sensitive to the blue light emanating from distant astronomical targets. Many of the interesting targets for dark energy studies will have light redshifted into the near infrared, requiring much thicker detectors to attain reasonable quantum efficiencies. Particle physicists have played an important role in the development of such detectors, starting from existing silicon detectors that successfully handle the challenging environments and specifications in particle physics experiments. Particle physics experience in prototyping, and engineering design of large detector arrays, and decades of success in the design, fabrication, assembly, quality assurance, installation, and commissioning of large silicon arrays for experiments at accelerators, leverages the outstanding technical resources of the National Laboratory and university communities for dark energy studies. Stage-

III dark energy experiments, such as DES, are making use of detectors and cameras fabricated by HEP laboratories.

Particle physicists have extensive experience of experiments that generate massive data sets. The stage-IV dark energy experiments will generate massive (petascale) data sets that provide data handling, data reduction, data distribution, and data mining challenges on a scale that has not been seen before in astrophysics. Particle physicists are facing many similar challenges especially with the LHC experiments. They have developed solutions that have been extensively tested ahead of LHC operations, and they will be crucial to enable the discovery of new physics once LHC begins operation. Particle physicists are now applying their experience in massive data techniques to dark energy experiments.

Dark Energy Measurement Techniques

Powerful and robust constraints on dark energy, and the ability to discriminate new fundamental fields from modifications to general relativity, depend upon the combination of results from three complementary dark energy probes: weak gravitational lensing (WL), Type Ia supernovae (SN), and baryon acoustic oscillations (BAO), each of which can be conducted from the ground and from space. Two further powerful probes, galaxy clusters (CL) and redshift (z) space distortions, require observational programs similar to the WL and BAO probes, respectively. Ideally, measurements would approach the astrophysical limits set by the finite observable volume of the universe and irreducible uncertainties in astrophysical systems (*e.g.*, intrinsic uncertainties in supernova luminosities, galaxy shapes, and models). Multiple techniques are valuable not only because in combination they increase the sensitivity to dark energy, but also for the opportunities they provide for systematic checks.

The first evidence of the acceleration of the Hubble expansion came from measurements of **Type Ia supernovae**. In the decade subsequent to this discovery, SN measurements still represent the bulk of experimental constraints on the acceleration. The method uses SN explosions as “standardized candles,” *i.e.* events have the same mean calibrated luminosity regardless of the epoch of explosion. In this case, an observation of the apparent fluxes and the redshifts of the supernovae yield a distance-redshift plot (“Hubble diagram”), which in turn yields the expansion history of the Universe over the observed range of epochs (if the curvature is known). In actuality the Type Ia events have a substantial range of luminosities, but the peak luminosity is strongly correlated with the temporal behavior of the explosion (the “light curve”), which allows the SN luminosities to be corrected to a standardized luminosity. The received SN fluxes are also altered by interstellar dust in the host galaxy. As large numbers of supernovae have been measured in recent years, more dimensions of diversity have been discovered in the Ia population, although at present there are only two securely identified dimensions of variation in the peak luminosity. The Dark Energy Task Force (DETF) found that the SN technique is *at present* the most powerful and best-proven technique for studying dark energy. If redshifts are determined by multiband photometry, the power of the SN technique depends critically on the accuracy achieved for photo-z's. If spectroscopically measured redshifts are used, the power of the SN method, as reflected in the DETF figure of merit, is much better known, with the outcome depending on uncertainties in SN evolution and in the astronomical flux calibration.

A critical issue for higher-precision SN studies is the potential for redshift dependence in the empirical relations between luminosity and light curve shape or color, such as might arise from changes in the physics of explosions or dust with epoch. Constraints on such evolution can arise from higher-precision measurements of diagnostic spectral and photometric features in SN behavior, and from observations toward the rest-infrared regime, where both dust extinction and supernova diversity are much reduced relative to rest-UV. For redshifts greater than 0.7, the rest-V band has a wavelength greater than 1 micron, which is essentially beyond the reach of precision ground-based photometry. Ground-based spectroscopic confirmation of SN redshift and type become expensive even on 10-meter-class telescopes at these redshifts, and measurement of spectral diagnostics becomes infeasible from the ground for targets this faint and red. Proposed JDEM space observatories can measure Type Ia supernovae in rest-visible bands to redshift 1.5 or higher before they become too faint for precision measurement. At low redshifts, however, SN events are rare on the sky, so large-area ground-based surveys are required to discover sufficient numbers of events. Future success will depend upon collecting sufficiently high-quality data on enough SN over the full available redshift range such that a precise Hubble diagram can be constructed while maintaining capability to diagnose evolution in supernova and dust properties.

The **baryon acoustic oscillations** method measures remnants of sound wave propagation in the baryon-photon plasma before the "recombination" transition to transparent neutral hydrogen in the Universe ~380,000 years after the Big Bang. Because the atomic physics of this era is well understood and well constrained by observations from the WMAP satellite (and forthcoming Planck data), the total propagation distance of these waves, *i.e.* the "sound horizon" r_s, is well known. The sound waves cause correlations in the density of points separated by r_s, which are potentially observable. Indeed the peaks in the power spectrum of the Cosmic Microwave Background are manifestations of these acoustic waves at the epoch of recombination. The acoustic oscillation signature has now been detected in the distribution of galaxies, although surveys of very large volumes are required since r_s ~150 Mpc. Because the sound horizon is determined to high precision from physical calculations, it acts as a "standard ruler": measurement of its angular extent as a function of redshift yields the same distance-redshift function $D(z)$ as the SN data, and detection of the standard-ruler scale along the line-of-sight (redshift) axis determine the expansion rate, *H(z),* as well.

The DETF found that the BAO technique is likely less affected by astrophysical uncertainties than the other dark energy techniques. Future larger-volume redshift surveys of galaxies can measure the BAO scale with increasing precision. The primary challenge is simply surveying the largest possible volume. The BAO method becomes more accurate at higher redshifts because more of the Universe is available to survey at larger radii – thus the BAO and SN probes complement each other well. The BAO probe can be executed in a ground-based imaging survey using photo-z's but would not approach the astrophysical limit. (Multiband photometry measures the intensity of the object in several colors using filters. A redshift determined this way is known as a photometric redshift, or photo-z.) Exploiting most of the accessible volume of the Universe where dark energy signals are strong will require spectroscopic redshifts for ~100 million galaxies; the Sloan Digital Sky Survey (SDSS), with ~1 million redshifts, is the largest survey to date. Ground-based spectrographs, which multiplex spectroscopic observations of thousands of galaxies, have been proposed to exploit the BAO probe using, for example, the forbidden oxygen emission line doublet at 373 nm to identify redshifts. A space-based observatory would have the advantage of low sky background, permitting detection of the even brighter H-alpha line at 656 nm rest wavelength for $z<2$, and also high multiplexing capability with relatively simple slitless spectroscopy. A space-based observatory can also observe the entire celestial sphere.

The **weak gravitational lensing** technique detects the bending of light rays by the gravity of dark matter density fluctuations in the Universe. A typical galaxy image is stretched by 1-2% due to these deflections, sometimes called "cosmic shear." Cosmic shear was first detected in 2001 and current results constrain certain cosmological parameters to ~10% accuracy or better. The strength and scale dependence of the cosmic shear are determined by both the distances involved in the lensing and by the strength of the dark matter lenses themselves, *i.e.* their masses. By measuring the distortion as a function of source redshift, one can map out not only the *expansion* history *D(z)* of the Universe, but also the history of the gravitational growth of structure, *G(z)*. Simultaneous measurement of these two functions is an essential part of the dark energy program, as it allows percent-level tests for the accuracy of general relativity on cosmological scales. The strength of WL constraints on dark energy and gravity increases with the area of the sky surveyed, and the number and redshift range of the target galaxies.

The first challenge to improved WL constraints is again one of scale: current experiments survey ~100 square degrees of sky, while stage-III experiments under construction will survey a few thousand square degrees, and the stage-IV experiments under consideration aim to survey $>10^9$ galaxies over 10-20,000 square degrees of the sky. The second challenge is to measure the shapes and redshifts of all of these galaxies with systematic and random errors that are small compared to the inherently weak cosmic shear signal. Advances in detector and electronics technology now make it feasible to massively increase the rate of photon collection from telescopes in space and on the ground – with multi-billion-pixel detectors being developed in the latter case. While much higher imaging throughput is affordable on the ground, there are also image distortions induced by atmospheric refraction, and by the imaging system due to gravity loading and thermal variations, which must be corrected to very high accuracy to avoid significant contamination of the cosmic-shear signal. The achievability of this goal from the ground is a topic of considerable debate at present. A space observatory has, in principle, the advantage of stable, high-resolution imaging for measurement of galaxy shapes. Spectroscopic redshift determination for 10^9 targets is currently infeasible, so WL surveys, both on the ground and in space, will infer redshifts from photometric (color) data. The required accuracy of photo-z measures for future WL surveys is well beyond the current state of the art, and there is additional debate over whether near-IR imaging and spectroscopy – available only from space – will be necessary for this goal. The DETF found that if the systematic uncertainties associated with the WL technique are at, or below, the asserted level, WL is likely to be the most powerful individual technique for stage-IV experiments.

WL is not the only possible route to measuring the gravitational growth function *G(z)*: gravitationally-induced motions of galaxies are detectable via **redshift space distortions*,*** detectable in large-volume galaxy redshift surveys. Redshift surveys intended for BAO measurement can yield redshift-space distortion measurements, although the desired resolution and density of sources are somewhat higher than required for BAO surveys. The astrophysical limitations to the redshift-space distortion method, *e.g.* statistical limits and contamination by non-linear motions, are active areas of research.

Galaxy clusters are the largest bound mass assemblages in the Universe. The density of such large masses vs cosmic time is a sensitive measure of the growth of structure and *G(z)*. Galaxy clusters present many observable signatures: (1) large concentrations of galaxies, detectable by optical/NIR (near infrared) imaging; (2) bremsstrahlung emission from hot intracluster gas, detectable by x-ray telescopes; (3) a distortion of the Cosmic Microwave Background due to scattering of CMB photons by hot electrons (the Sunyaev-Zeldovich, or SZ, effect), detectable by radio/mm-wave telescopes; and (4) a weak gravitational lensing signal imparted on background galaxies, detectable by the same visible/NIR surveys needed for cosmic-shear measurement. The challenge to the cluster method is in converting the observable quantity into an accurate mass measurement. The galaxy cluster WL method does not have this difficulty, but has less sensitivity than the other three. A visible/NIR survey designed for cosmic shear would implement one cluster detection method and provide important redshift and calibration information for the other three cluster detection techniques. The DETF found that the CL method has great statistical power but also the largest systematic uncertainty among all the techniques.

The dark energy measurement techniques are not independent. Many share observational resources and can even be done with the same data. Furthermore there are physics tests that are possible only with combined datasets: for example, comparison of WL data with BAO and redshift space distortion data from large galaxy redshift surveys greatly enhances their combined power and enables new model-independent tests of properties of the metric predicted by general relativity.

6.2 History and context

Previous panels have examined the development of a U.S. dark energy research program from several aspects. The Dark Energy Task Force (DETF) described a future program of "Stage III" and "Stage IV" experiments, giving rough goals for each in terms of a figure of merit (DETF FOM) for the measurement of the dark energy equation of state and its evolution. The JDEM Figure of Merit Science Working Group (FoMSWG) affirmed the value of the DETF FOM and defined a FOM for the detection of deviations from general relativity in the gravitational growth of structure. Neither panel was charged with evaluation of specific proposals for dark energy experiments.

NRC reports have emphasized the importance of dark energy research in the national portfolio, most recently the 2007 Beyond Einstein Program Assessment Committee (BEPAC) report, which selected a version of JDEM as the first mission to implement in NASA's then-existing Beyond Einstein Program. That report also noted the essential complementary nature of a combined space and ground program. Now, the NRC *Astro2010* panel is underway, charged by NASA, NSF, and DOE with setting decadal priorities for all future ground and space efforts in astronomy and astrophysics, including all dark energy projects considered by PASAG.

Substantial progress has been made in all dark energy techniques since the DETF reported: there is increased theoretical interest in and understanding of the consequences of modifications to general relativity as an explanation of the acceleration phenomenon, and there is improved understanding of the reach of various dark energy probes, and proposals for improved dark energy and gravity tests. Experimental progress includes completion of the observing programs for several "Stage II" surveys. The Supernova Legacy Survey (SNLS), the Equation of State SupErNovae trace Cosmic Expansion (ESSENCE), and SDSS II have greatly increased the number of well-characterized high-redshift SNe 1a. The Deep Lens Survey (DLS) and the Canada-France-Hawaii Legacy Survey (CFHLS) are the first attempts at tomographic WL measurements of significant scale. Most of these programs have not published final dark energy constraints even several years after completion of observations, due to the difficulty of controlling systematic errors. BAO constraints have recently been published from the full SDSS spectroscopic survey, completed in 2005. Dark energy constraints from galaxy clusters are now available from x-ray and optically selected samples. The influence of dark energy on the evolution of the Universe can be expressed in terms of the ratio of the pressure (P) to the energy density (ρ), $w(t) = P(t)/\rho(t)$, which may vary with time (or, equivalently, redshift). Current dark energy data suffice to constrain a constant dark energy equation of state to $w=-1\pm0.1$, but constraints on time evolution of w or modified gravity remain weak.

Progressing to Stage III, major experimental programs funded & underway include the following:

- Baryon Oscillation Spectroscopic Survey (BOSS) is a spectroscopic survey with the SDSS telescope and an upgraded spectrograph, which will apply the BAO technique to galaxies at redshifts $z<0.7$ and quasar absorption systems at higher redshifts.
- Atacama Cosmology Telescope (ACT) and the South Pole Telescope (SPT) are mm-wave telescopes in Chile and the South Pole, respectively, which detect galaxy clusters via the Sunyaev-Zeldovich (SZ) effect.
- The Dark Energy Survey (DES) is an imaging survey of 5000 square degrees in the *griz* visible bands using a new large imaging camera on the 4-meter Blanco telescope in Chile. The survey will use the WL and SN techniques to study dark energy, and will use optical techniques to improve the galaxy-cluster information from the SZ experiments.
- The extended ROentgen Survey with an Imaging Telescope Array (eROSITA) is a European/Russian x-ray telescope, which will constrain dark energy with x-ray detection of galaxy clusters.

A stage-IV program could obtain another order of magnitude or more improvement beyond Stage III in the FoMSWG metrics for dark energy and gravity tests before reaching astrophysical limitations. This report considers three proposals for HEP support of stage-IV dark energy projects. There are two large interagency projects, LSST and JDEM, each with potential international collaborations. The projected DOE contributions to construction and operation of each are $200-250M if they proceed on requested schedules. One medium-scale project, BigBOSS, requests $65M for construction and operation within the budget horizon, proposed to be fully funded by DOE. The LSST observatory executes ground-based imaging and the BigBOSS instrument executes ground-based spectroscopy, while the spaced-based JDEM will execute some presently undecided combination of imaging and spectroscopy.

Proposed Projects

The **Large Synoptic Survey Telescope (LSST)** Collaboration proposes an exceptionally capable visible survey with a new ground-based 8-meter-diameter telescope. A 3-billion-pixel CCD array combined with rapid slewing and readout enable imaging of the entire seasonally available sky (20,000 square degrees) every three clear nights. The epochs may be examined individually for transient phenomena, such as supernovae, or summed to produce deep images suitable *e.g.* for cosmic shear and galaxy-cluster surveys. Coverage from 320-1080 nm will allow LSST to push ground-based CCD observing to its limits for dark energy studies and a wide range of astrophysical investigations, from near-Earth asteroids to high-redshift gamma-ray bursts.

The LSST project has engaged a large collaboration of scientists from HEP and astrophysics organizations. The instrument and data processing plans are advanced - the primary/tertiary and secondary mirrors have been cast and a Chilean site selected – using private and preliminary agency funding. The project has prepared a very detailed cost estimate. The bulk of the funds for construction and 10-year operations are sought from NSF, DOE, international research agencies, and potential further partners.

LSST will discover and measure the shapes and photometric redshifts of more than 3 billion galaxies, and use this data to measure cosmic shear. It is the most ambitious and sensitive ground-based WL measurement so far proposed. A ground-based measurement may approach astrophysical limits, but the risk was held by the DETF to be greater than for a spaced-based measurement due to atmospheric and environmental effects on telescope image quality. The NIR imaging and spectroscopy possible from space may also be required to control systematic errors in photometric redshift measurement and calibration. The LSST galaxy data will also be used to execute the BAO probe using the photo-z technique. By combining the results from the WL and BAO probes improved constraints on dark energy and gravity tests can be performed from the same data set. As the BAO probe with photo-z's does not approach the astrophysical limit, these improvements are not as large as would be achieved by combining WL photo-z data with spectroscopic BAO data. LSST images will also yield an enormous catalog of optically selected galaxy clusters and allow WL calibration of the cluster mass scale.

LSST imaging will discover enormous numbers of Type Ia supernovae. As there is no spectroscopic component to the LSST program, precision redshifts and spectral diagnostics will require other resources. LSST light curves for SNe, some 30,000 per year, will be of high quality, albeit with the wavelength (and hence redshift) limitations inherent to ground-based CCD observing. The large SN data set will enable detailed studies of SN diversity, which will be of value to other dedicated SN studies using spectroscopy or imaging, and will strengthen tests for spatial inhomogeneity in the $z<1$ Universe. LSST's low-z population can solidly anchor the Hubble curve in the region of z where dark energy is dominant thereby complementing the necessary measurements of SN at high redshift obtainable from space.

The **BigBOSS** project proposes to greatly extend ground-based capabilities for galaxy redshift surveys by constructing a new 4000-fiber visible/NIR spectrograph for the existing NOAO Mayall 4-meter telescope in Arizona. With full-time use of the Mayall for 6 years, the BigBOSS spectrograph could acquire $\sim 10^{7.5}$ redshifts of galaxies at $0<z<2$ over 14,000 deg^2 of sky. An additional million quasar spectra could measure BAO features to $z<\sim 3.5$ using intervening absorption systems. The survey could be extended to 24,000 square degrees with 4 additional years of full-time use of the Blanco telescope, a twin of the Mayall at NOAO's Chilean site. Galaxy redshifts will come primarily from the 373 nm [OII] line. BigBOSS can resolve this doublet for secure line identification, and the spectral resolution is high enough to enable the redshift-space distortion method as well.

The BigBOSS concept has only been developed in the past year but has quickly built on experience with the SDSS and stage-III BOSS surveys. Further R&D is needed to produce BigBOSS engineering and cost estimates as secure as those of LSST. The dark energy performance of BigBOSS can be more securely predicted than that of LSST because the BAO method is less likely than the SN and WL methods to be limited by hard-to-predict astrophysical and instrumental systematic errors.

BigBOSS will require full-time use of NOAO 4-meter telescopes for a decade. These are important resources for the astronomical community so this is a major commitment. There is precedent: the stage-III *Dark Energy Survey* has been granted 1/3 of the Blanco time over 5 years. The BigBOSS survey will also require an extensive imaging survey to provide targeting information for its spectrograph. Clearly the BigBOSS project will require extensive cooperation with the astronomy community and agencies even if the construction and operations are fully funded by DOE.

The **Joint Dark Energy Mission (JDEM)** is a NASA-DOE space-based visible/NIR observatory to push dark energy techniques beyond the capabilities of ground-based facilities. Concept Study Reports on possible JDEM configurations were produced by three scientific collaborations, incorporating combinations of slitless NIR spectroscopy, NIR imaging, and CCD imaging to implement the BAO, SN, and WL probes with the space-based advantages elaborated above. NASA and DOE Project Offices are currently collaborating to develop mission options. One option presented to the Astro2010 panel (JDEM/IDECS) incorporates a slitless 1.1-2.0 micron spectrograph plus red CCD and NIR imagers on a 1.5-meter diffraction-limited telescope. The mission can conduct a BAO+WL survey of ~20,000 square degrees (spanning the full sky) and repeated imaging of a smaller area for precision measurement of 1000-2000 Type Ia supernovae to redshift ~1.5. The spectroscopic survey measures galaxy redshifts for z<2 using the H-alpha 656 nm line, and it is expected that a significantly higher density of measured galaxies can be obtained than with BigBOSS. Spectral resolution is worse, but sufficient to extract redshift distortion information. The WL survey – which also would achieve all visible/NIR galaxy cluster survey goals – measures galaxy shapes with Nyquist-sampled CCD imaging in multiple red filter bands. The red data are supplemented by on-board deep NIR imaging and ground-based blue/UV photometry to generate superb photometric redshifts, which can be calibrated using deep exposures of the slitless NIR spectrograph. Supernova imaging and spectroscopy extend through the NIR as well, taking advantage of the high angular resolution, high duty cycle, and low NIR background of space.

There is however no definitive design for JDEM at this time and it is unclear what resources will be available to build it and what it will take to produce a mission that exploits the advantages of space missions in all three methods. There are clearly tradeoffs to be made and weighed against ground-based capabilities in all three methods. In parallel, a proposal has been submitted to the European Space Agency's Cosmic Visions program for the Euclid spacecraft, designed to implement the WL and BAO techniques. The Euclid mission has many elements in common with proposed implementations of JDEM.

The history of JDEM is complex, and the programmatic, technical, and scientific definitions of the project all remain highly uncertain. This has presented several challenges to PASAG, as the study of dark energy continues to be a very high priority scientifically and space-based missions are expensive. At the same time, there is an evolving understanding of the relative advantages of space-based and ground-based measurements.

6.3 Findings and recommendations

The elucidation of dark energy is one of the highest priorities because it squarely addresses the fundamental nature of energy, matter, space and time.

The 2008 P5 report recommended support for a staged program, as defined by the 2006 Dark Energy Task Force (DETF), of dark energy experiments as an integral part of the U.S. particle physics program. PASAG reaffirms this staged approach and **recommends funding to complete those stage-III dark energy experiments receiving particle astrophysics (PA) support, *i.e.,* DES and BOSS.**

PASAG also recommends, for all budget scenarios, timely pursuit of a Stage IV program that can obtain another order of magnitude or more improvement beyond Stage III in metrics for dark energy and gravity tests

as specified by the DETF and Figure of Merit Science Working Group. Achieving this level of improvement is within the reach of the three primary methods for exploring dark energy before reaching astrophysical limitations.

The Dark Energy Planning Process

The 2008 P5 report recommended (i) DOE support for JDEM at an appropriate level negotiated with NASA, in any of the budget scenarios considered by that panel. (ii) DOE support for LSST, in coordination with NSF, in all funding scenarios considered by that panel, at a level that depends on the overall program budget. Since that report however, the estimated cost for a comprehensive JDEM that can achieve expected stage-IV goals with multiple dark energy exploration methods has been in considerable flux. This exposes a comprehensive and world-leading U.S. program to considerable risk, particularly in the lower funding scenarios.

While PASAG is not properly constituted to formulate a detailed plan for a comprehensive and optimal dark energy portfolio, the importance of the science and the cost expectations associated with such a portfolio do necessitate a careful consideration of how to achieve the best mix of projects for the available level of funding for dark energy. Hence, this panel recommends development of a coherent plan for achieving stage IV dark energy goals. The planning process must include: (1) all relevant funding agencies, (2) a reasoned approach for balancing ground and space missions to achieve the optimal science reach in a cost-effective way, (3) evaluation of technical issues by experts in each of the dark energy techniques, (4) independent evaluation of the science reach and estimated cost of each proposed project, and (5) full accounting of the possible contributions of international partners. The panel believes that this planning process should result in a world-leading program that delivers a portfolio of experiments that approach the astrophysical limitations for each dark energy method. The results of these experiments should be robust given astrophysical and instrumental systematic errors expected for each project. As a part of the consideration of the international context, the European Space Agency (ESA) Cosmic Visions planning process, which includes a proposed dark energy (BAO & WL) mission, Euclid, will be important.

As was true for the 2008 P5, PASAG recognizes the potential strengths of a space-based dark energy mission. However, the expense of a balanced ground-space portfolio strongly curtails the ability to pursue other high-priority particle astrophysics projects under any but the most optimistic funding scenarios. Given the substantial fraction of HEP funds dedicated to dark energy in any of the funding scenarios considered by this panel, the panel recommends that the JDEM design process should therefore be coupled to plans for ground-based projects so that it exploits the data for each technique that will realistically be obtained from the ground in the coming decade. This will ensure that the JDEM design maximizes the possibility to significantly extend the capabilities of ground-based experiments. The panel also recommends that the observatory and mission design processes be a close, collaborative effort between dark energy scientists and the engineering team to guarantee that engineering tradeoffs are fully consistent with the overall science goals.

As noted previously, the panel recognizes the unique strengths of partnerships between the astronomical and particle physics communities in pursuing the challenge of realizing the most ambitious particle astrophysics experiments. The panel recommends that JDEM include full intellectual participation of the HEP community and a major hardware contribution as integral pieces of the joint agency partnership to build and operate JDEM.

Recommendations

> **The panel recommends the formulation of a detailed plan for achieving a comprehensive and optimal dark energy portfolio under all funding scenarios. This plan should support projects whose science reach approaches astrophysical limitations for the 3 primary dark energy methods. Clearly Astro2010 is an essential component of this process.**
>
> **The JDEM design process should be coupled to plans for ground-based projects to ensure that JDEM offers the possibility to significantly extend the capabilities of ground-based experiments. The joint agency partnership between DOE and NASA should fully exploit the intellectual participation of the particle physics community in the science mission and observatory design, construction, and operation.**

Comments on Proposed Experiments

JDEM: In addition to the uncertainties stated above, PASAG knows of no actively engaged science panel currently advising the JDEM Project Offices. **While the responsibility for the project rests with the project management, it is essential that the observatory design and approach be a close, collaborative effort between dark energy scientists in the community and the project team to ensure a scientifically successful mission. Support of JDEM as a particle astrophysics project should imply that the methods and talents of the HEP community are applied to JDEM at its design, instrument construction, and science analysis phases.**

LSST: This project has a well-developed design and collaboration with very strong HEP participation in design, management, and construction plans, as well as in the LSST Collaboration. **Continuing support of LSST preparatory work is recommended so that ground possibilities are known for timely planning**

of a coherent ground-space dark energy effort. An ambitious ground-based imaging survey over most of the accessible extragalactic sky is an essential element for nearly all approaches to SN, WL, and BAO probes in a cohesive ground-space dark energy strategy.

BigBOSS is in the early planning stages, but presents a legitimate possibility of achieving a significant fraction of the BAO science goals for JDEM at <$100M cost. **Substantial immediate support is recommended for BigBOSS R&D so that ground BAO possibilities are known for timely planning of a coherent ground-space dark energy effort.** The ground astronomy agencies (NSF/NOAO) are essential partners in the BigBOSS project and planning.

Scenarios for Particle Astrophysics Efforts in Dark Energy

Planning for each scenario will have to consider the most effective way to deploy resources for each dark energy probe in order to approach astrophysical limits.

- For BAO, this can in principle be accomplished with either space- or ground-based spectroscopy. The issue is which platform can better approach this goal with the available resources.
- For SN, space and ground based measurements are more complementary: only the space observatory can obtain NIR imaging and spectroscopy of the depth required for precision use of high-redshift SNe. At low redshifts, the larger-area coverage possible from the ground is needed to discover a large number of events, and these closer & brighter SNe can be measured through the atmosphere with sufficient S/N. The astrophysical limits of the SN method are poorly understood.
- For WL, a space-based measurement can in principle approach astrophysical limits. A ground-based measurement may also do so but the risk is held to be greater due to atmospheric and environmental effects on telescope image quality. The NIR imaging and spectroscopy possible from space may also be required to control systematic errors in photometric redshift measurement and calibration.

The space-based JDEM experiment can in principle execute programs in BAO, SN, and WL that approach fundamental astrophysical limitations for each. It is however highly unclear whether the budget and design of JDEM will accommodate strong programs in all three methods. The LSST may approach astrophysical limits using WL probes. LSST will also conduct a photometric BAO survey and complete those aspects of the SN probe that can be addressed with ground-based visible imaging. The BigBOSS project executes a spectroscopic BAO survey that can, with sufficient observing time, approach the astrophysical limit.

Particle astrophysics efforts in dark energy will occur within a complex multi-agency, multi-national, multicultural landscape. This has complicated the history of JDEM development in particular. A comprehensive, coherent (ground+space) planning approach has been absent. The role of PASAG is not to define a detailed dark energy roadmap, rather to place dark energy program in the context of the rest of the cosmic frontier. In all scenarios, dark energy has a sufficiently high priority to warrant the dominant allocation of HEP resources. For each budget scenario, a dark energy budget envelope is given and the likely impact of an optimized allocation within this envelope is characterized.

Scenario A: *The dark energy budget envelope is ~$140M. This allocation will support a capable but limited dark energy program, and allow the large community of DOE scientists to contribute significantly to that program. However, DOE participation is not possible for any large project at the requested level. Significant DOE participation will be limited to a subset of the dark energy probes. A robust global program of dark energy exploration will either be done without DOE leadership or will not happen during the next decade.*

Devoting a larger share of funds available in scenario A to dark energy would unduly compromise the drive toward the direct detection of dark matter. The HEP funding for dark energy in this scenario would be insufficient for DOE to contribute hardware at the level of an instrument package to either large project. DOE leadership in a large project would require eliminating support for the other 2 projects *and* a significant delay to push expenses into the following decade. Given the importance of the science, the need for a robust 3-probe measurement, and the technological readiness of all projects, this is highly undesirable. The alternative is to fund BigBOSS for the BAO probe and hope that one or both large projects approach astrophysical limits for the SN and WL probes through the support of other funding agencies with much-reduced DOE participation.

Scenario B: *The dark energy budget envelope is ~$200M. This allocation will support a very capable but limited dark energy program, and allow the large community of DOE scientists to contribute significantly to that program. DOE leadership role in a single large project may be possible but is at risk – particularly for JDEM, given the large uncertainties in its budget and scope. Lack of HEP participation in the deselected program jeopardizes its existence and jeopardizes the goal of approaching astrophysical limits. Significant DOE participation in a robust program is at risk.*

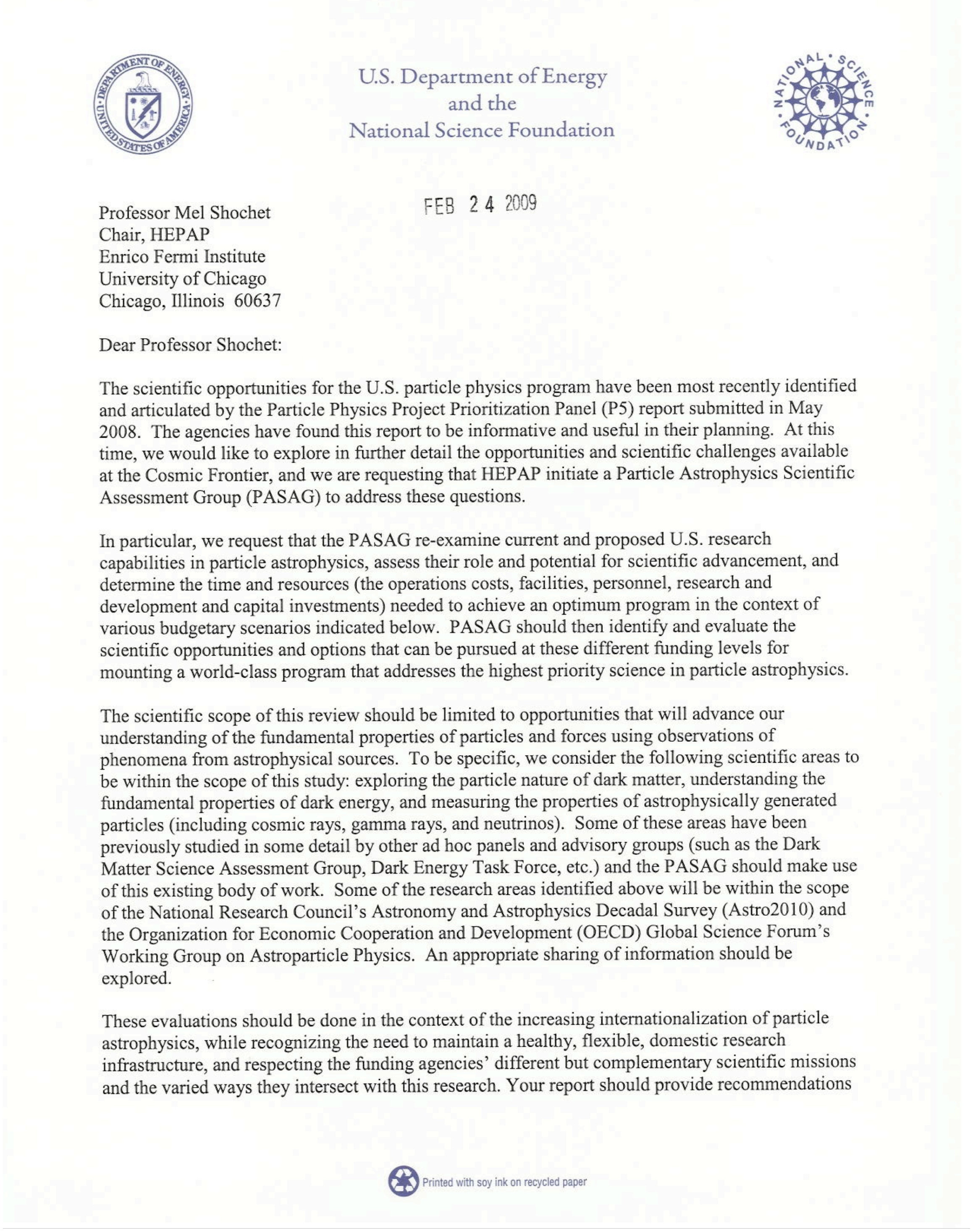
DOE leadership in one large project may be possible (at the level of an instrument hardware contribution) only if the budget is favorable and if participation in other dark energy programs is eliminated. An alternative is to execute BigBOSS for the BAO probe and make contributions to one or both large projects at a lower level, *i.e.,* component hardware.

Scenario C: *The dark energy budget envelope is ~$350M. A world-leading program is possible, with coordinated activities in space and on the ground. A* *significant DOE contribution to JDEM is possible, along with full support of BigBOSS and support for LSST, for example; or support at the full level requested for LSST and BigBOSS with a contribution to JDEM at the component level.*

Scenario D: *The dark energy budget envelope is ~$540M. A world-leading program is assured.* This allocation would fully fund the DOE hardware and scientific contributions to an optimized combination of stage-IV dark energy experiments. This portfolio will likely include a large ground-based imaging survey, a space-based survey with higher angular resolution and infrared capabilities, and a massive spectroscopic galaxy redshift survey, executed through the most cost-effective combination of ground and space approaches. HEP leadership and the resultant dark energy measurements would be strong and secure.

Appendix

a. Charge



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

FEB 24 2009

Professor Mel Shochet
Chair, HEPAP
Enrico Fermi Institute
University of Chicago
Chicago, Illinois 60637

Dear Professor Shochet:

The scientific opportunities for the U.S. particle physics program have been most recently identified and articulated by the Particle Physics Project Prioritization Panel (P5) report submitted in May 2008. The agencies have found this report to be informative and useful in their planning. At this time, we would like to explore in further detail the opportunities and scientific challenges available at the Cosmic Frontier, and we are requesting that HEPAP initiate a Particle Astrophysics Scientific Assessment Group (PASAG) to address these questions.

In particular, we request that the PASAG re-examine current and proposed U.S. research capabilities in particle astrophysics, assess their role and potential for scientific advancement, and determine the time and resources (the operations costs, facilities, personnel, research and development and capital investments) needed to achieve an optimum program in the context of various budgetary scenarios indicated below. PASAG should then identify and evaluate the scientific opportunities and options that can be pursued at these different funding levels for mounting a world-class program that addresses the highest priority science in particle astrophysics.

The scientific scope of this review should be limited to opportunities that will advance our understanding of the fundamental properties of particles and forces using observations of phenomena from astrophysical sources. To be specific, we consider the following scientific areas to be within the scope of this study: exploring the particle nature of dark matter, understanding the fundamental properties of dark energy, and measuring the properties of astrophysically generated particles (including cosmic rays, gamma rays, and neutrinos). Some of these areas have been previously studied in some detail by other ad hoc panels and advisory groups (such as the Dark Matter Science Assessment Group, Dark Energy Task Force, etc.) and the PASAG should make use of this existing body of work. Some of the research areas identified above will be within the scope of the National Research Council's Astronomy and Astrophysics Decadal Survey (Astro2010) and the Organization for Economic Cooperation and Development (OECD) Global Science Forum's Working Group on Astroparticle Physics. An appropriate sharing of information should be explored.

These evaluations should be done in the context of the increasing internationalization of particle astrophysics, while recognizing the need to maintain a healthy, flexible, domestic research infrastructure, and respecting the funding agencies' different but complementary scientific missions and the varied ways they intersect with this research. Your report should provide recommendations

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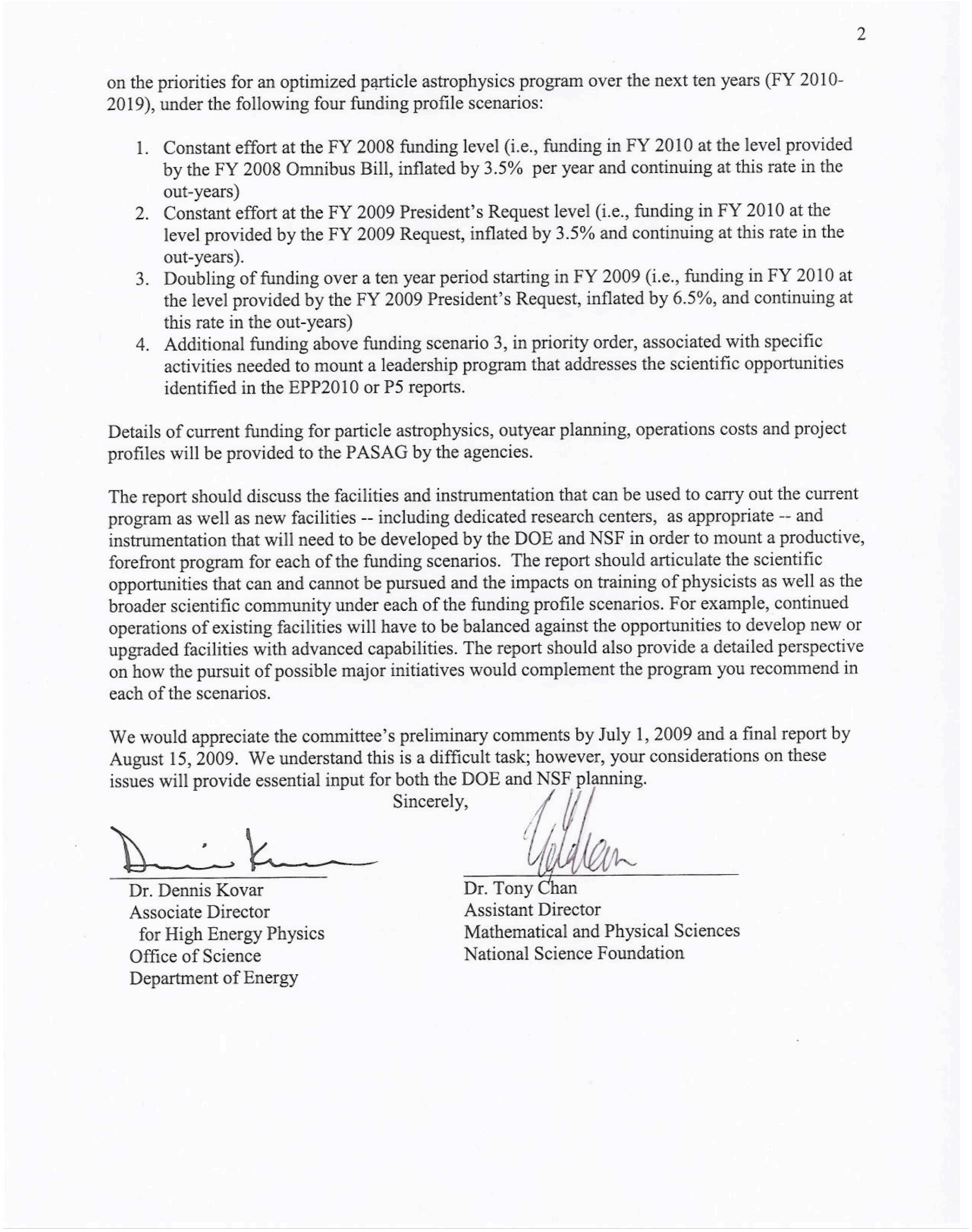

on the priorities for an optimized particle astrophysics program over the next ten years (FY 2010-2019), under the following four funding profile scenarios:

1. Constant effort at the FY 2008 funding level (i.e., funding in FY 2010 at the level provided by the FY 2008 Omnibus Bill, inflated by 3.5% per year and continuing at this rate in the out-years)
2. Constant effort at the FY 2009 President's Request level (i.e., funding in FY 2010 at the level provided by the FY 2009 Request, inflated by 3.5% and continuing at this rate in the out-years).
3. Doubling of funding over a ten year period starting in FY 2009 (i.e., funding in FY 2010 at the level provided by the FY 2009 President's Request, inflated by 6.5%, and continuing at this rate in the out-years)
4. Additional funding above funding scenario 3, in priority order, associated with specific activities needed to mount a leadership program that addresses the scientific opportunities identified in the EPP2010 or P5 reports.

Details of current funding for particle astrophysics, outyear planning, operations costs and project profiles will be provided to the PASAG by the agencies.

The report should discuss the facilities and instrumentation that can be used to carry out the current program as well as new facilities -- including dedicated research centers, as appropriate -- and instrumentation that will need to be developed by the DOE and NSF in order to mount a productive, forefront program for each of the funding scenarios. The report should articulate the scientific opportunities that can and cannot be pursued and the impacts on training of physicists as well as the broader scientific community under each of the funding profile scenarios. For example, continued operations of existing facilities will have to be balanced against the opportunities to develop new or upgraded facilities with advanced capabilities. The report should also provide a detailed perspective on how the pursuit of possible major initiatives would complement the program you recommend in each of the scenarios.

We would appreciate the committee's preliminary comments by July 1, 2009 and a final report by August 15, 2009. We understand this is a difficult task; however, your considerations on these issues will provide essential input for both the DOE and NSF planning.

Sincerely,

Dr. Dennis Kovar
Associate Director
for High Energy Physics
Office of Science
Department of Energy

Dr. Tony Chan
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b. Membership

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c. Meetings

PASAG did much of its work in weekly telephone meetings, starting on 20 April, and in subgroup meetings. There were two face-to-face meetings: one on 21-23 July in Washington, D.C. (open-session agenda below), and another meeting at UC Santa Cruz on 17-18 August. PASAG also heard a presentation by Craig Hogan, representing the perspectives of the laboratories on particle astrophysics, on 29 June.

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PASAG Meeting 21-23 July
Agenda
Location: NSF 4201 Wilson Blvd, Arlington, VA

Tuesday 21 July

9:00-10:00 Executive Session (closed)

10-10:45 Auger North presentation
10:45-11:00 Auger North Q&A

11:00-11:30 TA/Tale presentation
11:30-11:45 TA/tale Q&A

11:45-12:30 Executive Session (closed)

12:30-13:30 lunch

13:30-14:15 AGIS presentation
14:15-14:30 AGIS Q&A
14:30-15:00 HAWC presentation
15:00-15:15 HAWC Q&A

15:15-15:30 break

15:30-18:00 Executive Session (closed)

18:00 questions to projects, adjourn for the day

Wednesday 22 July

8:30-10:00 Answers from projects followed by Executive Session (closed)

10:00-10:30 BigBOSS Presentation
10:30-10:45 BigBOSS Q&A

10:45-11:15 QUIET Presentation
11:15-11:45 QUIET Q&A

11:45-12:30 Executive Session (closed), then questions to QUIET and BigBOSS

12:30-13:30 lunch

13:30-15:30 Executive Session (closed)

15:30-16:30 answers from QUIET, BigBOSS (closed)

16:30-18:30 Executive Session (closed)

Thursday 23 July

8:30-12:00 Executive Sessions (closed)