Report of the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP)

Final Version
December 13, 2004

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Report of the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP)

1. Introduction

There is an increasing amount of interest in the U.S. and worldwide in scientific research at the interface of physics and astronomy. The boundaries of this area of research are not rigidly defined, but in general encompass particle astrophysics, cosmology, and non-accelerator particle physics. Both the Department of Energy (DOE) and the National Science Foundation (NSF) have substantial investments in this field – there is thus a need for scientific assessment of experimental projects that may be of interest to either, or both, agencies.

The Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) was convened in response to a letter from Dr. Raymond L. Orbach of the DOE and Dr. Michael S. Turner of the NSF. Drs. Orbach and Turner requested that the High Energy Physics Advisory Panel (HEPAP) establish a sub-panel to assess projects in experimental non-accelerator physics. This sub-panel, known as SAGENAP, reports to HEPAP, and has as its primary consideration the scientific merit of each experimental project brought before it.

The background, organization, and charge for SAGENAP are documented in Appendix A. SAGENAP is to assess projects in three Categories:

1. Projects in the conceptual phase,
2. Projects that are ready to request agency funding for concept studies, design and development, or construction, and
3. Ongoing projects that receive funding from the DOE or the NSF.

SAGENAP met on April 14-16, 2004 in Ballston, VA at the Arlington Hilton Hotel, with the NSF as the host agency. The membership of SAGENAP is given in Appendix B. The chair of SAGENAP for this meeting was Rene Ong (University of California, Los Angeles). Representatives from DOE, NSF, and NASA (National Aeronautics and Space Administration) participated in the meeting, with Richard Imlay (DOE) and Eugene Loh (NSF) serving as co-organizers.

At its April 2004 meeting, SAGENAP heard presentations from 26 projects, along with 6 presentations that provided general background information on a particular area of research. The meeting schedule and the list of presentations are documented on the SAGENAP website at http://astro.ucla.edu/~sagenap/ and are included in Appendix C. The website also includes copies of the talks that were provided by the speakers to SAGENAP.

We have arbitrarily grouped the experimental projects into five broad research areas:

A. **Dark Energy and Cosmic Microwave Background (CMB),**
B. **Dark Matter,**
C. **Very High-Energy (VHE) Particle Astrophysics,**
D. **Neutrinos, and**
E. **Other Experiments**

A tabulation of the various presentations by category and by area is given in Table 1. As mentioned earlier, some presentations provided general background information to SAGENAP and were not considered as specific projects under consideration.

There were a total of thirteen projects in Categories 1 and 2 and thirteen projects in Category 3. We have grouped the projects in Categories 1 and 2 together because the charge to SAGENAP specified that we should assess them in the same manner. For Category 3 projects, SAGENAP received status reports from the proponents of each project. We note that Category 3 included both ongoing experimental efforts and, separately, R&D (research and development) efforts that could lead to a funded experimental project in the future. Efforts in the latter category are DRIFT, EXO, SNAP, and XENON.

This report provides an assessment for each of the 26 projects. Each assessment represents both a summary of the views of the SAGENAP members and an overall consensus of the sub-panel. The project assessments are grouped by science area; a general overview of the scientific and experimental context of each science area is provided.

Science Area	Category 1,2 Projects	Category 3 Projects and R&D Projects	General Background Presentations
Dark Energy and CMB	Dark Energy Survey Destiny LSST Polarbear QUIET	SNAP R&D	
Dark Matter		CDMS II DRIFT R&D XENON R&D ZEPLIN II	
VHE Particle Astrophysics	ASHRA Auger Project N HAWC Tel. Array/TALE	Auger Project S HiRes Milagro STACEE VERITAS	
Neutrinos	ICARUS LANNDD	EXO R&D KamLAND Super-Kamiokande	Double-Beta Decay Off-Axis Neutrino Physics T2K: Beamline & Near Detector T2K: 2KM Detector Theta(13) Experiments
Other Experiments	EDM I EDM II		

TABLE 1

Presentations to SAGENAP 2004, grouped by science area and project category.

2. Dark Energy and Cosmic Microwave Background

2.1. Dark Energy Overview

A number of recent lines of evidence point to the remarkable result that the dynamical expansion of the universe is being accelerated by a pressure-like entity popularly called "dark energy." Dark energy has dominated the expansion only relatively recently in cosmic terms. At earlier epochs, the rate of expansion was determined by the amount of dark matter. One of many puzzles is why the relative importance of dark matter and dark energy seem to be balanced so closely in the present epoch. Dark energy affects the formation of structures on smaller scales, such as clusters of galaxies. In an accelerating universe, lumps of matter fall together to form bound structures only at earlier epochs. To quantify how the formation of structure is expected to evolve, we need to know the dynamical effect of dark energy and whether its properties depend on time. The ratio of pressure to energy density is conventionally labeled w. The dependence of w on cosmic epoch is often parameterized by $w = w_0 + w' z$, where z is the cosmological redshift. Current observational constraints suggest that w_0 is within the range of -1.2 to -0.8, if it is assumed that $w' = 0$. Currently, w' is not yet well constrained but its value is consistent with zero. If new observations were to show that w' is not zero (or that w_0 is not equal to -1), a cosmological constant interpretation for dark energy could be ruled out.

In the past few years, cosmological research has entered a more precise era, thanks to detailed information from maps of the temperature fluctuations of the cosmic microwave background (CMB), from maps of the large-scale distribution of galaxies, and from experiments studying Type Ia supernovae. We have developed a physical framework - a standard model for cosmology - that accounts for the available observations (such as the rate of expansion and the dynamical evolution of both baryonic and dark matter) in a consistent and compelling way. There are independent observational constraints that reinforce our understanding of the values of the key cosmological parameters. Several observational approaches relevant to exploring the properties of dark energy have great promise to determine w_0 and w' with much better precision, with corresponding power to confront the range of currently allowed theoretical models.

The nature of dark energy is one of the most profound mysteries facing physics and astronomy today. Several recent studies have endorsed the importance of research in this area, including the “Connecting Quarks with the Cosmos: Eleven Science Questions for the New Century” 2003 report of the National Research Council (NRC). This report emphasized that multi-agency cooperation was a key element for planning major initiatives to address these questions, and it listed "What is the Nature of Dark Energy?" as one of the eleven questions. Two other studies have been recently published that recommend a concerted effort to understand the nature of dark energy: the "Physics of the Universe" 2004 report of the Interagency Working Group on the Physics of the

Universe of the National Science and Technology Council and the "Quantum Universe: The Revolution in 21st Century Particle Physics" 2004 report of HEPAP.

So-called "luminosity distances" are derived from the apparent brightness of objects presumed to have the same intrinsic luminosity. Type Ia supernovae are extremely valuable in many respects, but they are rare, and finding them requires considerable investment in detector area and exposure time. In order to reach sufficiently high redshift to trace the evolution of the effect of dark energy, and in order to control systematic errors to the requisite very low level, it is necessary to undertake a mission in space. This necessity is the premise for the Joint Dark Energy Mission (JDEM) being planned by NASA and DOE, for which Destiny and SNAP are proposed implementations. A Science Definition Team for JDEM has been jointly constituted by NASA and DOE to optimize JDEM's scientific return and to provide scientific guidance for its technical development.

Type Ia supernovae are not the only good probes of dark energy - for example, a census of massive clusters of galaxies as a function of redshift depends sensitively on dark energy. The key is identifying the clusters with minimal selection bias. There are several practical techniques, including recognizing clusters by gravitational lensing and by the Sunyaev-Zel'dovich (SZ) effect (see 2.2).

Gravitational lensing occurs when light from a distant object such as a galaxy or quasar, the "source", passes near an intervening massive object such as a galaxy or cluster of galaxies, the "lens". In this geometry, the light is magnified and sheared, or distorted. An initially round image appears to be slightly elliptical in the case of "weak lensing". With a sufficient number of background sources, the dispersion in intrinsic shapes and orientations averages out, and one can detect a net shear in the shapes and orientations of the background galaxies. This measurement provides a measure of the net projected density of matter along the line of sight, and it is thus a powerful way to map the distribution of matter, independently of what fraction is baryonic (ordinary) matter.

A promising technique is "mass tomography" where the distribution of mass over a wide range of distances is determined by how the weak-lensing signal changes with increasing distance. Observations made in space yield sharper and more stable images, but a large-scale weak-lensing survey can also be undertaken from the ground with a large optical telescope. It is fortunate that dark energy can be constrained with a number of independent techniques: the result is too important to be based on a single approach, and moreover the constraints in parameter space between different experiments are often orthogonal. A concordance in the final result between experiments with varied systematic errors will provide essential reassurance that we are interpreting the results correctly.

The recent NRC decadal survey in Astronomy and Astrophysics ("Astronomy and Astrophysics in the New Millennium", 2001) outlined and prioritized the facilities deemed necessary to undertake a suite of programs in astrophysics in this decade, both on the ground and in space. The second-ranked major ground-based facility was an optical telescope which combines a large collecting aperture and a large field-of-view to achieve an unprecedented advance in sky coverage per unit time. Although the case for such a telescope emphasized exploring the "time domain" to search for transient events, from supernovae to nearby asteroids, the surveys it will undertake provide numerous other research opportunities, especially in the area of dark energy.

The HEPAP Quantum Universe report mentioned above addressed the overlapping intellectual themes of high energy physics and astrophysics. It commented on the capabilities of JDEM and a large ground-based telescope, and put these astronomical approaches to dark energy into the context of particle physics as probed by the Large Hadron Collider and a Linear Collider. The report concluded: "We are extraordinarily fortunate to live in a time when the great questions are yielding a whole new level of understanding. We should seize the moment and embrace the challenges."

Dark Energy Roadmap

Studying the dark energy is now well recognized as being a very high scientific priority, but the relative strengths and weaknesses of various approaches have yet to be systematically quantified. SAGENAP recommends that a roadmap for dark energy research be developed by the agencies. Such a study would provide the needed basis for evaluating proposed experiments based on their specific capabilities and how each addresses an aspect of the larger picture. The study should indicate what the scientific requirements are for the determination of w_0 and w', what precision is required to discriminate between theoretical models, what capabilities are required for the experiments, and whether reaching these goals is likely to be practical. Such an overview will be essential for evaluating the cost-effectiveness of the various approaches.

The roadmap study would forecast the results of ongoing observations of supernovae to provide context for what the next-generation supernova surveys could contribute, both in terms of constraints on cosmological parameters, and in terms of understanding the underlying physics of the supernova explosion. The same study would show how a space-based supernova survey instrument, such as JDEM, would complement ground-based surveys, both present and future.

The roadmap would similarly review weak-lensing surveys to determine what we can expect to learn from surveys with existing telescopes and cameras, what can be learned with next generation ground-based facilities, and how ground-based and space-based weak-lensing surveys complement each other.

A number of other related approaches to measuring dark energy and dark matter are being developed, including a census of massive clusters detected via the SZ effect and observations using large telescopes to provide spectroscopic redshifts for galaxies and spectra for supernovae. These efforts are an important component of dark energy research and should be included within the roadmap study.

A multi-agency Dark Energy Roadmap would reinforce earlier studies that show how astronomical measurements contribute to fundamental physics. More specifically, the roadmap would clarify the contributions from the various agencies, and indicate where the agencies can provide the best leverage and where they can benefit from shared technology development.

2.2. Cosmic Microwave Background Overview

Over the past two decades there has been substantial effort in the measurements of the anisotropy of the cosmic microwave background (CMB). Measurements of the CMB serve as a probe of the state of the universe when it was roughly 300,000 years old. As a result, we have a very large (14 billion year) period over which models depending on the fundamental parameters of the universe can be tested. There have been a large number of earlier CMB results, culminating with the measurements made by balloon missions such as BOOMERANG and MAXIMA and by the COBE and WMAP satellite missions. The anisotropy of the CMB is seen in temperature fluctuations at a level of $\sim 10^{-5}$ of the CMB temperature of 2.7 K.

The experimental work on CMB anisotropy does not stand on its own. There has been extensive theoretical work to link the early universe to the present universe. Measurements of the CMB have been combined with measurements of large-scale structure, distant supernovae, and theory to determine a number of cosmological parameters with unprecedented accuracy. The next-generation CMB experiments will probe extremely high-energy scales, as well as the amount of dark energy.

New ways to use the CMB as a probe of the universe are being developed. There are two major directions that are currently being explored. The first can be described as using the CMB as a "backlight" on the universe. Models based on the existing measurements can be extended to finer angular scales (approximately 1 arc-min). These scales can then be probed to see how the evolution of the universe is imprinted on the CMB. Electrons in very hot (10^8 K) gas in large clusters of galaxies can scatter off photons from the CMB via the inverse-Compton process, raising the energy of the photons in a distinctive way. This effect, known as the Sunyaev-Zel'dovich (SZ) effect, can be used as a tracer of massive structures as a function of time. The evolution of structure is dependent on, among other things, the equation of state of the universe and on the masses of the neutrinos. These same parameters can be probed somewhat independently by looking for distortions in fine-scale CMB maps imposed by gravitational lensing from massive structures. Several experiments are being developed to pursue these routes, and they should be collecting data within the next five years.

The second, and possibly the most exciting, new direction is the search for polarization in the CMB. The pattern of polarization directions on the sky can be decomposed into "E modes" with no handedness and "B-modes" with handedness. The E-modes are generated from the same dynamics as the temperature anisotropies, namely temperature-driven density fluctuations. The B-modes are generated by inflationary gravity waves and by gravitational lensing of the E-mode signals. The expected magnitude and scales of these effects have been extensively modeled. The expected backgrounds have also been estimated. To put it in perspective, the E-mode signal is predicted to be about a factor of ten smaller than the primary anisotropy signal. The first verification of this has been provided by the DASI experiment. Current best estimates of the B-mode signal are about 100 times smaller than the primary anisotropy. B-mode polarization measurements will be the focus of much experimental effort over the next two decades.

The challenge offered to CMB polarization experiments is substantial. The detector technology must be at the cutting edge, and the systematic effects from the receivers, the telescopes, and the atmosphere must be controlled to very high accuracy. Emission from non-CMB polarized sources such as dust, and synchrotron emission from our Galaxy and extra-galactic sources, must be identified, understood, and removed. However, the payoff will be well worth the investment of effort. There is the potential to detect the signature of gravitational waves from the end of the inflationary period at a time of 10^{-38} s after the Big Bang, corresponding to grand unified theory (GUT) energy scales of approximately 10^{16} GeV. The measurements of the conversion of E-modes into B-modes via gravitational lensing will provide an independent probe of dark energy. The considerable investment that is currently planned for dark energy missions justifies serious consideration of orthogonal techniques, such as the SZ effect and CMB polarization.

The current state-of-the-art in polarization comes from the pioneering results from the ground-based Degree Angular Scale Interferometer (DASI) and Cosmic Background Imager (CBI) and the space-borne Wilkinson Microwave Anisotropy Probe (WMAP). There are also results expected from the BOOMERANG (Balloon Observations of Millimetric Extragalactic Radiation and Geophysics) group by 2005. Finally, the Planck satellite, scheduled for launch in 2007, will provide an all-sky polarization map at frequencies up to 500 GHz. None of these instruments will have enough sensitivity to probe the GUT scale described above. Thus, there is a clear need for more sensitive experiments (on the ground, on balloons, or in space) to measure the CMB polarization.

Cosmic Microwave Background Roadmap

Measuring the polarization of the CMB will certainly be a high priority for the scientific community over the next several decades. In the Physics of the Universe report, the polarization of the CMB is specifically identified as an area for inter-agency investment. The report states that: "The three agencies (DOE, NSF, and NASA) will work together to develop by 2005 a roadmap for decisive measurements of both types of CMB polarization. The roadmap will address needed technology development and ground-based, balloon-based, and space-based CMB polarization measurements."

A CMB Taskforce panel has been assembled to develop the required roadmap. The goal for the panel is to complete a report by early 2005. The report will make recommendations on technology development, supporting theory, ground-based and suborbital supporting polarization measurements, and supporting observations by other instruments. The report will also work towards coordinating the activities of the funding agencies to most effectively meet this goal.

2.3. Projects in Dark Energy and Cosmic Microwave Background

Five Category 1 and 2 projects were presented to SAGENAP in the areas of dark energy and the cosmic microwave background: Dark Energy Survey, Destiny, LSST, Polarbear, and QUIET. One Category 3 project, SNAP R&D, was presented to the sub-panel. Here we report on our findings for these six projects, arranged alphabetically.

2.3.1. Dark Energy Survey (DES)

The Dark Energy Survey (DES) plans a 5000 deg^2 survey down to 24th magnitude brightness levels with the goal of measuring *w*, the vacuum energy equation of state. The survey would be done in four wavelength bands using a new 3 deg^2 camera on the Blanco 4m telescope at Cerro Tololo in Chile. The DES proponents envision a four-year construction schedule, followed by four years of observations.

The science program proposed by DES, consisting of weak lensing, cluster photometric redshifts, and Type Ia supernovae, is truly excellent. We find many positive features to commend the project. Especially noteworthy is their estimate that the SZ catalog of 30,000 galaxy clusters from the South Pole Telescope (SPT) could be used to measure *w* to a precision approaching that of the SN Ia projects. This would be an important independent measurement of one of the most critical quantities in cosmology with very different systematic errors from measurements made of Type Ia supernovae. Such a measurement is an essential ingredient of any systematic study of the vacuum energy. There was no discussion of the capabilities of DES to probe *w*'.

While the science goals of the project are excellent, SAGENAP has concern that the project might be substantially more expensive than estimated by the team. The team discussed the challenge posed by the detectors and the optics. Both aspects must be well understood in order for DES to achieve its scientific goals in a timely way. The optical performance of the system is crucial to making good weak-lensing measurements from the ground. The requirements on the stability of the system could prove to be a significant challenge.

The plan to acquire ten times the data of the Sloan Digital Sky Survey (SDSS), but spend substantially less on computing than SDSS, was a concern noted by several sub-panel members, although offsetting this concern is that DES can build on the SDSS software base and utilize the significant advances in hardware that have become available over the last few years. Nonetheless, the data acquisition and processing will be a significant challenge. In addition, although the Fermilab team proposing to build the camera has a strong record with silicon vertex detectors, it may need more experience in astronomical instrument development and use.

A strength of this project is that it can combine the experience of a DOE high energy physics laboratory with the astronomical community at a major astronomical observatory. This type of collaboration was seen as an excellent match for this kind of science and the sub-panel felt that it should be encouraged. The details of the operational arrangement between the lab and the observatory will need to be better defined.

In summary, the DES project has considerable scientific merit, and the team appears to be strong and well focused. Questions regarding the real cost (particularly related to software) and the place of DES in an overall multi-agency initiative on dark energy remain unclear. A Dark Energy Roadmap would provide the needed context for this project (and others exploring dark energy).

2.3.2. Destiny

Destiny is a concept for an implementation of the Joint Dark Energy Mission (JDEM) – the collaborative approach put forward by NASA and DOE to explore dark energy via a space-borne instrument. A stated goal of Destiny is to reduce costs for JDEM by having only the minimal, necessary capabilities, including a restricted mission lifetime. The proposed project has similar scientific goals to the SNAP implementation concept for JDEM (see 2.3.6), but the design of Destiny is much less mature than that of SNAP. The scientific objective is to determine the time history of the expansion of the universe via the detection and measurement of Type Ia supernovae in the redshift interval of $0.5 < z < 1.7$. This scientific driver requires observations in the near-infrared spectral range, which in turn requires a space-based mission.

The Destiny focal plane instrument makes use of a transmission grating replicated onto a wedge prism (a "grism") to obtain spectral resolution $R \sim 100$ observations in the wavelength interval of $0.9\ \mu m < \lambda < 1.7\ \mu m$. The telescope concept has an aperture of 1.8 m and an instrument field of view of 0.25 deg^2. Plans call for a 7.5 deg^2 field at the North Ecliptic Pole to be repeatedly and uniformly visited over the 2-year mission lifetime. The total integration time enables a five standard deviation detection of supernovae above noise in synthetic broad wavelength bands at a level of 10 nJy (1 Jy $=10^{-26}$ W m^{-2} Hz^{-1}). This sensitivity will be sufficient to measure 2,500 Type Ia supernova light curves, as well as the rich sample of galaxies and other sources in the field. The redshifts and flux measurements of the supernovae are determined from the same data.

A feature of the Destiny approach is the spectroscopic sampling of the light curves at multiple epochs. A resolution of $R \sim 100$ is sufficient to identify the key spectroscopic features in the SN atmospheres. By following the development of these features as the atmosphere expands, the data can constrain models for the events in ways that may enable investigation of "second-parameter" effects (i.e. systematic effects associated with the luminosity of supernovae as a function of time). Since an accurate measurement of the dark energy parameters requires reducing systematic effects in the supernova distance measurements to very small levels, the dense time-sampling of the spectra may prove to have significant diagnostic power when combined with detailed supernova models. The scientific importance of multiple-epoch spectroscopy needs to be understood by simulations.

Moreover, since spectra are obtained by multiplexing data samples over the entire field of view, all other types of supernovae will be automatically observed as well. Type II supernovae may also be useful as distance indicators and they can serve as a cross-check on the results from the Type Ia's. Another significant result will come from the redshifts of galaxies in the field, if that capability can be demonstrated. Accurate supernova flux measurements must cope with the problem of sampling the spectra in fixed wave bands at different redshifts. With the Destiny approach, the spectra can be compared directly in their rest-frame, and these uncertainties are removed.

The Destiny concept is an elegant, but unproven, approach that is responsive to the JDEM vision. The request is for support for studies over the next two years that will further its scientific potential, establish its technical feasibility, and determine its cost. The scientific potential depends on such things as a proper accounting of the effect of the light from the underlying host galaxy and the precision with which the pixel-to-pixel relative spectral sensitivities can be determined for spectra that appear in arbitrary locations in the field of view. The pixel scale is an example of a trade-off study that is going on now. Larger pixels would enable fewer sensors to be used, but good angular resolution is needed to maintain sensitivity and to minimize the effect of the host galaxy on the extracted supernova spectrum.

The local environments of Type Ia supernovae may be different at redshifts $z > 1$ than at lower redshifts because of galaxy evolution, and this effect might be an important element of the systematic error that needs to be controlled. Tracking such evolution would be more easily accomplished by imaging without the grism in the beam. In comparison with Destiny, which does not have the capability of a direct imaging mode with sufficient angular resolution, the SNAP concept provides more opportunities for control of systematic errors.

The scientific potential of Destiny on an absolute basis, and relative to SNAP, is difficult to evaluate at this time because the performance simulations are not yet sufficiently advanced. The project team has not done sufficient simulations to show whether the science goals can be accomplished with their simpler instrument, or to quantify how well their measurements of w_0 and w' would compare to those of SNAP. The mission philosophy takes advantage of flight heritage. The sensors have the same specifications as the SNAP near-infrared devices. Still, while recognizing the intent to control costs, the sub-panel was concerned that much depended on the grism approach and the particular sensors used.

The Destiny PI has experience with NASA missions of similar scale, but SAGENAP had some concern that he was already over-committed. The current collaboration consists of a partnership between academic institutions, Los Alamos National Laboratory (LANL), and Lockheed Martin. The LANL personnel contribute to ground operations and to a theory component. At least at this stage of the project, it was not clear that the theory component was well motivated. Overall, the proposed DOE participation does not seem to be naturally aligned with traditional DOE strengths – there did not appear to be significant participation from experimental groups supported by DOE.

The mission definition team has expertise in supernova physics and supernova surveys for cosmology, both from space and on the ground. Science and simulation studies are needed to address the issues mentioned earlier. The current team is relatively small and will require substantial strengthening, especially including additional experimentalists, to undertake such a major project.

To summarize, the Destiny concept for JDEM is a relatively simple, and therefore possibly lower cost, alternative for the measurement of Type Ia supernova events. However the simpler experiment may have larger systematic errors, and may have fewer ways to determine them. Simulations and design trade-off studies would need to be completed to demonstrate the merits of the Destiny concept. The team needs to be strengthened in order to carry out a successful R&D effort.

2.3.3. Large-aperture Synoptic Survey Telescope (LSST)

The Large-aperture Synoptic Survey Telescope (LSST) is a concept for a large-aperture, wide-field, ground-based optical telescope. The concept is motivated partly by the potential of astrophysics in the time domain (e.g. moving objects like asteroids and variable objects like supernovae), and partly by the capability for sensitive cosmological measurements (e.g. via weak lensing or SN Ia detection). As discussed in 2.1, weak lensing is an especially promising technique for constraining the properties of dark energy and it complements the supernova technique. LSST's wide field of view (3.5 deg) is needed to cover the accessible sky, and its aperture (8.4 m) is needed to sample high redshift sources and to detect a high surface density of objects. A figure of merit related to sensitivity is the product of telescope collection area times field area: $A\Omega$. Currently, the LSST concept has a much larger $A\Omega$ value than any other operating or proposed project, and it is thus a leading contender for the forefront ground-based experiment to advance the study of dark energy. Large $A\Omega$ enables LSST to survey objects over the entire visible sky, resulting in small statistical errors on the cosmological parameters. The large statistical base enables dividing the data into different sub-samples to study systematic errors, and it enables searching for changes in the parameters w_0 and w' in different regions of the sky.

Investigating the nature of dark energy and dark matter has a high priority in the scientific community and is of special interest to particle physics. The technique of measuring the weak-lensing shear signal should be pursued because it is complementary to supernova and cosmic microwave background measurements. In contrast to these other techniques, weak lensing does not rely on models of supernova explosions or on baryonic physics to interpret the results, but there are, however, other systematic errors associated with weak-lensing measurements. It is very appropriate for the DOE and NSF to support an effort to measure w_0 and w' via weak-lensing technique because the question of dark energy is so important that complementary techniques should be used to ensure that we have reliably characterized the impact of dark energy.

The importance of a large-aperture, wide-field ground-based telescope for astronomical measurements has been appreciated and highlighted in recent reports, as discussed in 2.1. The current LSST project is a particular proposed implementation of the general concept of a large-aperture, wide-field telescope. The presentation to SAGENAP focused on the role and participation of DOE institutions in the LSST project. SAGENAP received a Letter of Intent submitted by the team to the DOE. We understand that a separate R&D proposal has been submitted to the NSF, but SAGENAP did not consider it.

The LSST concept has been carefully developed by a team having an impressive range of expertise. At this relatively early stage, LSST appears to be an excellent concept lead by a strong team. In agreement with previous studies, SAGENAP strongly endorses the scientific importance of the project and the general approach. Given the strengths of the project and its proposed implementation, funding for research and development (possibly followed by funding for design and development) is warranted – note, however, that a number of issues considered by the sub-panel should be addressed in future proposals that are submitted by the LSST group. These issues are presented below in no particular order.

Science Management

The science management plan presented by the collaboration was relatively rudimentary. We suggest that the plan should show how the science priorities are established, and to indicate ways in which the scientific scope can be changed, if necessary. The science management plan should show in detail how the science drivers flow down to the project design. The arguments for the minimum acceptable AΩ are difficult to evaluate without more detailed specifications of how the input numbers were derived from the scientific requirements. Expressing the science gains based on the value of AΩ would allow for evaluation of the trade-offs for a more, or less, capable telescope and camera system. For example, the project is now considering a larger camera because it appears to be technically feasible; this will increase the cost, and the team should evaluate whether such a direction is necessary and whether it is cost-effective. It will help if the scientific goals are prioritized and the design optimization is restricted to best achieve the highest priority goals.

We encourage the development of a science collaboration plan. Such a plan would show how the lead scientists interact with the broader community in terms of specifying the science goals. The plan would also show how the science working groups function inside the collaboration, and how the funding for scientific research will be managed and coordinated. In the short term, the science teams should make linkages to the groups undertaking faint-galaxy redshift surveys. Such an activity would help evaluate errors (both random and systematic) in the photometric redshifts.

The large AΩ of LSST gives it excellent capability for the discovery of supernovae and the characterization of their light curves. A relatively shallow survey by LSST would sample redshifts around $z \sim 0.45$, extending up to $z \sim 0.7$. This is a key redshift interval, as it is at these redshifts that the effect of dark energy on the dynamics of the expansion of the universe is expected to become dominant. The practicality of spectroscopic follow-up of these supernovae by other, large telescopes should be considered; such partnerships would potentially increase the scientific impact of LSST.

Simulations and Systematic Errors

The team's assertion that the final weak-lensing results will be limited by random statistical error drives several design features, including the ten-year data taking period. A careful study of the potential systematic error floor and its effect on the project should be presented. The project needs to develop end-to-end simulations, including those for the camera optics and detectors. The need for an extremely stable point-spread function means that an advanced mechanical design for the telescope is required, including many control elements. If the simulations allowed a flexible range of input assumptions, the simulations could be tested by ongoing experiments.

Simulations are needed to provide the tools to 1) estimate systematic errors in the experiment so that controls can be designed, 2) examine the trade-offs between different capabilities, 3) optimize the experiment (construction cost, observing cadence, time to completion, etc.) for weak-lensing measurements, 4) explore what factors contribute to the irreducible shear and estimate the value of this shear, 5) show how well the LSST concept competes with other weak-lensing surveys, 6) show how the error budget for shear and for various operational parameters (e.g. exposure time per field, the size and stability of the point-spread function, and area of sky) relates to errors on w_0 and w', and 7) provide a convincing demonstration that the net errors on cosmological parameters scale inversely as the square root of the survey duration.

Camera Research and Development

The Letter of Intent to DOE did not include a research and development plan with an associated schedule outlining which critical measurements need to be made to provide the basis for a proposal with a reliable cost estimate. The team informed SAGENAP of current work studying the benefit/risk ratio for CCD versus CMOS technologies. A process is needed for subsequently choosing the detectors. The team should choose between the 2.3 Gigapixel and 2.8 Gigapixel options, and evaluate the costs and trade-offs of implementing the very fast read-out time. A plan is needed for characterizing the detectors, including spelling out the role of the various institutions in device testing. There must be a detailed prototype camera plan that provides real-world environment testing at the relevant integration scales to demonstrate that scaling up to the full complement of sensors will work as envisioned.

Project Time Scale

It may be difficult to hold a scientific collaboration together for as long as ten years since individuals will be attracted to pursue other projects in the meantime. DOE is being asked to support a substantial fraction of the operations costs. This begs the question of what science could be achieved in, say, five years. The proposal should make the specific argument why the last five years of a mission with static capabilities is cost-effective. In principle, LSST could be productive beyond a five or ten year mission duration, but this potential raises issues such as the expected mean time between major failures and on-going operations funding.

The Time Domain

The large AΩ also makes LSST ideal for other purposes, such as searching for moving objects and transient events. Discovery of near-Earth asteroids, trans-Neptunian objects, supernovae, and afterglows from gamma ray bursts comprise an important part of LSST's science program. This diverse program implies a diverse set of design constraints on the telescope, instrumentation, and data pipelines. For example, the search for asteroids requires that the telescope slew, settle, and read out in a very short time (~ 5 secs). The planned 10-sec exposure required for some transient searches implies a very large (~ 20 TB/night) data rate. Since the settle time is significant compared to the exposure time, the requirement for short exposures suggests that there may be substantial dead-time. A completely different constraint on the system capabilities comes from the desire to disseminate alerts of transient objects within 30 seconds.

The requirement to read out quickly and to slew quickly (and thus cover a large amount of sky per night) appears to be partially driven by the asteroid search. Since it will take ten years to meet the goals of completeness in the census of asteroids, that part of the program also drives the total survey duration. If the requirement to discover and follow near-Earth asteroids did not exist, it is possible that a different camera (and telescope) could be built that would enable the weak-lensing survey to be accomplished faster and less expensively. The PanSTARRS (Panoramic Survey Telescope & Rapid Response System) project in Hawaii will make in-roads in the time-domain science. It is conceivable that a coordinated approach could off-load some of the burden of undertaking time-domain science with LSST.

Data Handling

The nature and cost of the software development program are potentially the most uncertain aspects of the project because of the exceptionally high data volume and data rate, and because of the stringent requirements on data reduction (e.g. keeping systematic errors below statistical errors, and enabling the fast identification of transient objects with few false alarms). The expected data rate for LSST is substantially higher than any previous astronomical project – the team compares their data rate with the two major particle physics experiments at the LHC. Yet the money and manpower allocated for computing and software are substantially less than those of other projects with large data rates. Much more work is needed to provide a reliable cost estimate for the data pipeline and software development.

The proposed software development encompasses some, but not all, of the needed software. The DOE software tasks include developing pipelines for processing and calibrating the images and carrying out the weak-lensing analysis. These tasks are spread over all of the DOE institutions, as shown in the Letter of Intent. The plan should show who will actually do the work, and how it will be coordinated. The plan should show who will do the non-DOE parts and who has responsibility for the overall integration. (As an example, how is the development of the near-Earth object pipeline budgeted, and who is responsible for this task?) There should be a plan for distribution of data, and for determining which data products are saved. The plan should describe the requirements on the system for delivering data and other science products and show how one can estimate the operating costs of these activities.

Relation to DOE's Mission; Relation to Other Projects

It would be useful to have a clear sense of the interfaces between the proposed DOE element of LSST and other DOE objectives. For example, the requested DOE in-kind support could advance other elements of DOE's high-energy physics (HEP) program. Moreover, parts of the LSST research and development program, such as the simulations, could be shared with other DOE efforts. DOE is being asked to support the development of other large cameras (SNAP and DES) at different laboratories; a coordinated plan could help these efforts reinforce each other.

The project will be helped with additional contributions of senior scientist time, both in the sense of greater commitments from existing key personnel and of additional individuals who are encouraged to join the project and to undertake specific tasks. The project should ensure that there is an adequate mix of HEP-trained individuals and those with an astrophysics background. SAGENAP is concerned that DOE appears to have less of a stake in aspects of the time-domain part of the LSST mission, yet the costs and design of the project appear to be strongly influenced by this capability.

LSST Summary

SAGENAP strongly endorses the weak-lensing approach of a telescope such as LSST: the capabilities for constraining the nature of dark energy are very significant and are complementary to other approaches. Other implementation schemes for a large-aperture, wide-field telescope may exist that address the dark energy science more cost-effectively; if good alternatives were to be proposed, they should be considered. In the meantime, the particular implementation scheme that was presented to the sub-panel appears to be well crafted (albeit requiring significant technological development and an aggressive time line). The DOE team is strong and well organized, and, with possible enlargement, it should successfully complete the DOE components. We recommend that the team address the concerns made here in their full proposal for funding to DOE.

As noted earlier, the Letter of Intent for the DOE element of LSST did not appear to have an explicit research and development phase, whereas a proposal has been submitted to the NSF for such a phase (including aspects relevant to the DOE element). SAGENAP encourages the agencies to define a coherent process for LSST to move forward through the R&D phase. As mentioned earlier (2.1), we also recommend that the agencies develop a Dark Energy Roadmap that would provide the overall context for how the various approaches to exploring dark energy (e.g. ground-based and space-based) fit together.

2.3.4. Polarbear

Polarbear is a proposed experiment designed to study the polarization of the CMB. The distinguishing feature of this experiment is the plan to use Transition Edge Sensors (TES) coupled to twin-slot antennas that are several wavelengths across. This monolithic detector design has many advantages and is considered one of the main contenders for future satellite missions. The proposed Polarbear instrument will have 60 dual-polarization pixels at 90 GHz and 150 GHz frequencies and 40 pixels at 220 GHz. The antenna pairs of different pixels are rotated relative to one another to allow for full coverage of the Q and U Stokes parameters without requiring the use of a half-wave plate. At this time, the proponents have demonstrated that they can build a single pixel. A fully populated array must be produced to meet their scientific goals.

The Polarbear design calls for a custom 3 m diameter off-axis telescope to be sited at the White Mountain Research Station (3,500 m elevation) in eastern California. The size of the telescope and number of detectors determine the resolution and mapping speed of the instrument. The frequency coverage provides powerful information needed to identify frequency-dependent systematic effects and foreground sources. The baseline plan is to achieve a sensitivity on the tensor to scalar ratio of the polarization signal of about 0.01 in several years of operation. This sensitivity would correspond to a GUT energy scale of approximately 10^{16} GeV. A possible upgraded experiment in the future with many more sensors is expected to have significantly improved sensitivity.

The Polarbear experiment encompasses several critical steps to measuring and interpreting the polarization of the CMB. The development of a new generation of millimeter-wave focal plane arrays is generally seen as a necessary step along this path. The Berkeley team is a leader in this area. While the jump from a single detector to hundreds is a big one, the team is well placed to succeed. It is possible that a separate effort concentrated in the area of scaling-up will be needed prior to funding the full experiment. Likewise, a telescope specifically designed for these measurements with dedicated observing time will also be needed to reach the sensitivity limits. This is similar to the plans for the South Pole Telescope and the Atacama Cosmology Telescope. It may be that some coordination of the use of these facilities will obviate the need for the additional telescope (envisioned here by the Polarbear group) by the time an appropriate focal plane and receiver are implemented.

The CMB Roadmap study (see 2.2) is intended to address the issues of technology development, supporting observations, ground-based and suborbital observations, and theory. It is clear that the NSF and DOE will be in a much better position to evaluate their potential investments in this area after this report is received in early 2005. The coordination of the resources of DOE, NASA, and NSF will be required to reach the challenging scientific goals in a timely and cost-effective manner.

In summary, Polarbear represents a novel and highly promising technique to attack the important scientific questions addressed by measurements of the polarization of the CMB. The group is strong and it has a good track record. The only significant technical question concerns the ability to scale-up from a relatively small number of detectors to a much larger array. We believe that Polarbear is a project that merits funding and it should be considered by the agencies in the broader picture of the future of CMB measurements to be provided by the CMB Roadmap study.

2.3.5. QUIET

QUIET (Q/U Imaging ExperimenT) is a proposed experiment to measure the polarization of the CMB. The QUIET detector technology is based on compact and self-contained Microwave and Millimeter-wave Monolithic Integrated Circuits (MIMICs) being developed at the Jet Propulsion Laboratory (JPL). These circuits provide an output proportional to the polarized input signal, and they represent a significant advance in the use of heterodyne technology for polarization measurements. Combining all the features of a complex microwave device into a single package reduces the cost and volume to enable the implementation of large arrays of sensors. QUIET proposes to implement 91 devices operating at 90 GHz, with a possible upgrade to 900 elements. At the present time, they have a proof of concept for the MIMIC device and are preparing to go into large-scale production. This expansion should be a reasonable step, but it is not without some uncertainty. The group must also develop large stacked-horn arrays to keep the costs of the receiver down. Again, this development is technologically feasible, but it does involve some schedule risk.

The QUIET team plans to move the 7 m diameter Crawford Hill (NJ) telescope to a site in the Atacama region of Chile (5,000 m elevation). The cost of moving the telescope (donated by Lucent Technologies) is far less than building an entirely new telescope. The project will use a staged approach. First, they may operate the 91-element receiver from the current site at Crawford Hill, and in 2007 they hope to implement the 900-element receiver at the site in Chile. With the final system they will map 4000 deg^2 to achieve a sensitivity on the tensor to scalar ratio of 0.01, and possibly better.

While the scientific goal of the QUIET experiment is the same as the Polarbear experiment, the approach is very different. The QUIET team has come up with a sound and cost-effective way of producing large numbers of MIMIC detectors. This approach is likely to succeed for a ground-based instrument. However, it is unclear that it will be appropriate for a future space mission if one is required. This possibility should not be considered a negative evaluation for the present plans for a ground-based experiment. On the contrary, a ground-based measurement made this way will have significant advantages over alternate high-frequency measurements, such as those proposed by Polarbear. These advantages include ease of implementation, alternate foreground issues, and robustness of the beam profiles. As was seen with the success of the HEMT-based WMAP satellite, these issues should not be understated. The team is strong and it has significant experience with this type of ground-based observation on the Crawford Hill telescope.

In summary, the QUIET instrument, as proposed, would be at the cutting-edge of planned experiments measuring the polarization of the CMB. Given the experience of the team, there are relatively few technical difficulties associated with building an instrument such as QUIET. As with the Polarbear experiment, the agencies should give serious consideration to QUIET, but they should wait for the CMB Roadmap report so that they can evaluate the roles of these experiments in the context of the larger DOE/NASA/NSF program.

2.3.6 SuperNova Acceleration Probe (SNAP) R&D

The SuperNova Acceleration Probe (SNAP) is a proposed implementation for the JDEM mission to study dark energy. SNAP has been in serious development for several years, with several significant developments taking place in the last two years. First, NASA and DOE announced they would support a Joint Dark Energy Mission (JDEM), and the SNAP team responded by focusing their efforts on preparing for the upcoming competition. In addition, the SNAP collaboration has expanded to more than 100 members from a dozen institutions. SAGENAP received a report from SNAP on the status of its R&D effort.

The scientific goal of SNAP is to characterize the dark energy by measuring its equation of state, w, and its change with time, w', by detecting and following 2,500 type

Ia supernovae out to redshift values of $z = 1.7$. In addition, SNAP plans an ambitious weak-lensing survey that will measure the same dark energy parameters in a complementary way. The supernovae will be studied by repeatedly imaging a 15 deg^2 field in nine filter bands, discovering and obtaining light curves by difference imaging, and by obtaining spectra to get the type, redshift, and detailed information for each supernova. For the weak-lensing project the 120 images of each field will be co-added to reach a limiting AB magnitude of 31. In addition, a 1000 deg^2 survey down to a limiting AB magnitude of 27.7 will be done. By detecting more than 100 resolved galaxies per square-arcminute with a small and extremely stable point-spread function, SNAP should also produce a superb weak-lensing data set.

SNAP is a relatively mature and well-developed concept, in comparison with other major missions being proposed to study dark energy. Recent work by the SNAP team includes the development of a sophisticated simulation tool that has firmly established the need for a space mission in order to fully understand supernovae at redshifts greater than one. The simulation also permits quantitative comparisons to be made between various experimental designs. Important areas of focus are on the calibration system and on the control of systematic errors, the latter which the SNAP proponents believe are key to getting believable dark energy results.

The team has completed designs for many aspects of the project including the telescope, focal-plane layout, CCD sensors, custom electronics, and orbit. Substantial progress has also been made on the remaining aspects of the mission, including the spectrograph, infrared sensors, spacecraft, and system integration. An important issue that is being attacked is related to the availability and suitability of the infrared (IR) detectors – it is good news that there might be more than one vendor for these. SAGENAP was impressed at the continual and substantial progress made by SNAP towards a fully-designed mission. The team estimates that in two or three years they will have a detailed mission design, schedule, and cost that will provide the basis for serious evaluation by DOE and NASA.

SNAP remains an extremely well-motivated experiment for determining the nature of the dark energy that is causing the accelerated expansion of the universe. We endorse the team's approach of understanding and minimizing systematic errors. The team is very strong and has grown to include more traditional space and astrophysics personnel. While other techniques for studying the equation of state of dark energy are being developed, so far none are as mature as SNAP or have shown better capability. The science case for performing such a measurement continues to get stronger, as does the team and its mission design.

3. Dark Matter

3.1. Dark Matter Overview

Research into the nature of dark matter has become increasingly important, exciting, and active in the past few years as the technology has improved and as it has become clear that the dark matter, whatever its composition, comprises about 25% of the energy density of the universe. Since baryonic candidates and compact objects in the mass range of 10^{-7} to 10^{2} solar masses have been eliminated as significant contributors to the dark matter, an undiscovered elementary particle remains the prime dark matter candidate. Of the possible elementary particles, the axion and some form of Weakly Interacting Massive Particle (WIMP), especially the lightest Supersymmetric (SUSY) particle, are the most prominent candidates.

While little definite is known about the elastic scattering cross section of the dark matter particle with ordinary matter, the minimal SUSY model and arguments based on the required relic abundance have been used for guidance. The current generation of direct-detection experiments has sensitivities that are just beginning to probe the most interesting areas of SUSY parameter space for viable particle dark matter. Thus, it is quite possible that SUSY will be discovered first by these dark matter searches, before it is detected by experiments at particle accelerators. However, it is also possible that the dark matter particle interacts too weakly with ordinary matter to be detected by direct search experiments.

The field of direct dark matter detection as a whole consists of more than a dozen experiments throughout the world. In addition to the experiments that were presented to SAGENAP, and discussed below, other major experiments include DAMA, GENIUS-TF, CRESST, and EDELWEISS. Broadly speaking, the field seems to be converging on an effective overall experimental strategy. The most powerful current technologies (i.e. Ge and Si bolometers and NaI scintillators) are being pushed and are giving rapidly better limits. The less developed technologies (e.g. xenon), that may be more scalable and therefore appropriate for the very large detectors needed to reach the ultimate sensitivity, are being actively pursued and prototyped. If a detection of dark matter is made, alternative technologies with other capabilities (e.g. gas phase TPC's having directional capability) may be crucial in verifying that the signal is from dark matter and in exploring the velocity distribution of the dark matter in our galaxy.

3.2. Projects in Dark Matter

Four Category 3 projects were presented to SAGENAP in the area of dark matter: CDMS II, DRIFT, XENON, and ZEPLIN. Here we report on the status of these four projects, arranged alphabetically.

3.2.1. Cryogenic Dark Matter Search (CDMS II)

The Cryogenic Dark Matter Search (CDMS II) employs Ge and Si solid-state detectors to search for WIMP dark matter. These low-threshold (< 20 keV), kilogram-size detectors simultaneously detect phonons and ionization, allowing for very good discrimination between potential WIMP nuclear recoils and the main electron, neutron, and photon backgrounds. In 2002, the CDMS collaboration ran one tower of four Ge and two Si detectors at the shallow Stanford Underground Facility and in a 54 kg-day exposure found 19 nuclear recoil events. These events were interpreted as neutron background resulting from the shallowness of the site. Using a neutron subtraction technique, the data were analyzed to set limits on the WIMP-nucleon cross section that are interesting, but are not quite as strong for WIMP masses above 30 GeV as those set by the EDELWEISS group. The collaboration then moved to the much deeper Soudan site, and currently two towers of seven detectors each are in operation. With an effective exposure so far of 22 kg-day and no nuclear recoils or other backgrounds observed, the CDMS II experiment has now set the best upper limit on the WIMP-nucleon cross section ($\sigma < 4 \times 10^{-43}$ cm^2, 90% CL, for a WIMP mass of 60 GeV), approximately four times lower than EDELWEISS.

Plans for CDMS II in the near future call for the addition of three more towers of detectors, followed by continuous operations to the end of 2005, resulting in a planned exposure of 1200 kg-day and a final CDMS-II sensitivity to a cross section of approximately 2×10^{-44} cm^2. This sensitivity would push the search for WIMPs into prime SUSY parameter space. Given the present status of the additional towers and the smoothness of the current running situation, we see no reason that these goals will not be met. It is likely that over the next several years CDMS-II will continue to be the most sensitive dark matter detector in the world.

The CDMS collaboration also described plans for an upgraded experiment (CDMS-III) that would add two more towers and run until 2008. Besides new towers, the team would like to improve shielding against the rock neutron background that should eventually turn up and to improve the handling of materials and analysis. The stated goal of CDMS-III is to get an exposure of 4800 kg-day and to reach a sensitivity of $\sigma \sim (4 – 7) \times 10^{-45}$ cm^2, deeply probing SUSY parameter space in competition with the large accelerator experiments.

SAGENAP is impressed with the progress made by the CDMS collaboration, both in terms of achieving the world's best limits on WIMP dark matter and in terms of the successful and steady enlargement of their detector. CDMS II will remain a forefront detector for the foreseeable future. The CDMS collaboration is strong and focused, and their efforts should continue to receive support from the funding agencies during the next several years.

3.2.2. DRIFT R&D

DRIFT (Directional Recoil Identification From Tracks) is a somewhat unique concept for a dark matter experiment in that it attempts to measure the direction of the nuclear recoil by the use of a low-pressure gas time projection chamber (TPC). The WIMP recoil directional pattern should be fixed relative to the stars, and thus it will move with respect to the detector's axes as the Earth rotates on its axis and around the Sun. The ability to measure the recoil pattern and to compare with that expected for WIMPs gives a powerful signature for the veracity of the WIMP signal. Typically, other detectors seek to measure only the overall WIMP interaction signal and/or the annual modulation of the signal.

The DRIFT R&D detector is now running at the Boulby Underground Laboratory (U.K.) after several years of intensive development and construction. The initial data consisting of 37 days of live time seems promising, but results are still very preliminary. The DRIFT collaboration faces a number of challenges to prove that their concept can result in a detector that is competitive with the other approaches (i.e. cryogenic and liquid xenon detectors). First, the background rates seen by the DRIFT R&D detector are significantly higher than those for detectors such as CDMS-II; these backgrounds are under investigation. Second, the capability of the detector to measure the recoil direction has not yet been demonstrated. Third, the current effective target mass of the R&D detector is substantially smaller than that of other, operating detectors. There are thus crucial studies that need to be made over the next year or so to understand if the DRIFT technology and approach will be able to contribute in a significant way to the search for dark matter.

SAGENAP congratulates the DRIFT collaboration on their significant progress and recommends that they be supported to run the R&D detector over the next year or so, in order to collect sufficient data to evaluate possible extension to larger sizes and increased gas pressure and in order to work towards addressing the challenges discussed here.

3.2.3. XENON R&D

The XENON collaboration is carrying out R&D towards building a WIMP dark matter detector using liquid xenon. The ultimate goal is a detector with an active mass of 1000 kg of xenon distributed in ten 100-kg modules. The plan calls for R&D at the 1-kg level, followed by 10-kg, 100-kg, and 1000-kg phases. At the final 1000-kg level, with a sufficiently low threshold of about 15 keV recoil energy and an un-rejected background rate of 10 events per year, XENON would be sensitive to a WIMP primary component of the dark matter having an interaction cross section of 10^{-46} cm^2. XENON performs best for heavier WIMPs with masses above 50 GeV. At WIMP masses of 20 GeV, the sensitivity is an order of magnitude less, but is still interesting.

The XENON group is developing a detector employing a two-phase (liquid/gas) technique. Scintillation light produced in a WIMP interaction produces photoelectrons in both a CsI photocathode immersed in liquid xenon and in a photomultiplier array above the liquid. In the case of the CsI cathode, the photoelectrons liberated migrate through the liquid to the top surface, where they are extracted into the gas above the top surface and avalanche-multiplied on a grid. The detector would reject background by taking advantage of the difference in relative yields of scintillation light and ionization for electrons and recoils. Electrons ejected by Compton interactions form the primary background, and these can be rejected at the 99.5% level. Neutrons are a more serious concern because they produce xenon recoils just as WIMPs would. Shielding and sufficient overburden are the defenses against neutron backgrounds.

The XENON collaboration has a record of achievement in the use of liquefied noble gases for particle detection. Since the beginning of R&D in 2002, the collaboration has made excellent progress and has demonstrated many of the basic principles that must be present to build a successful detector at the 10-kg scale. Backgrounds from components, in particular the photomultipliers, are the focus of much of the team's effort. The contamination of xenon with ^{85}Kr is a known and difficult problem for all detectors of this type. The collaboration has implemented a low-temperature chromatographic method to purify the xenon of this troublesome contaminant. A 1000-fold reduction has been demonstrated successfully, and at this level the remaining activity in a 100-kg detector would be acceptable.

Among the most important remaining goals are demonstrating that both the ionization and scintillation signals from nuclear recoils can be reliably extracted and separated from the backgrounds. Scintillation light from 48-keV neutron recoils has been observed. However, the necessary observation of extracted electrons from the surface of liquid xenon has not yet been accomplished. Significant work remains to recover and use the photoelectron signals from the CsI photocathode immersed in the liquid as well. The next phases of the program are a 10-kg detector that would have sensitivity comparable to CDMS-II, provided a suitable underground site could be located and characterized.

SAGENAP congratulates the XENON collaboration for their excellent technical progress. We encourage the agencies to continue to support their R&D effort towards a larger and competitive dark matter detector. (See 3.2.4 below for a summary of the xenon dark matter projects).

3.2.4. ZEPLIN

ZEPLIN is a project led by the UK Dark Matter Collaboration, with participation by U.S. scientists. The long-term objective is a dark-matter detector using xenon with an active mass of 1000 kg. The collaboration has been in existence since the early 1990s and has pioneered many of the basic principles of the liquid xenon method that are now used by other collaborations as well. At present the R&D program is exploring two parallel tracks, ZEPLIN II and ZEPLIN III. The former is a low-field device and the latter is a high-field one. These follow a successful R&D program with ZEPLIN I at the 5-kg level. ZEPLIN I has placed limits for spin-independent WIMP interactions below those of the DAMA experiment and is proof that such xenon-based detectors can work. The ZEPLIN experiments make use of the Boulby Underground Laboratory (U.K.).

The limited discrimination capability of ZEPLIN I led to development of a 30-kg two-phase detector ZEPLIN II in which ionization signals from electron backgrounds (the dominant background) could be extracted and multiplied in the gas phase. Provided that no ionization signal is able to escape detection owing to any geometrical imperfections, this approach is capable of good background rejection. In parallel, the work began in the U.K. on ZEPLIN III, a somewhat similar concept but with high electric fields to improve the discrimination ratio for electrons relative to recoils.

ZEPLIN II will be taken to the Boulby Laboratory when complete. As in any rare event search, background is the central issue. In ZEPLIN II, advantage is taken of the difference in relative yields of scintillation light and ionization for electrons and recoils. The technique of using an ionization signal to tag backgrounds induced by electrons is sound. However, the absence of an ionization signal from dark matter recoils raises the concern that candidate events might in fact be arising from unanticipated dead regions from which a signal could not, for whatever reason, be extracted. In ZEPLIN III, this concern will be addressed by increasing the electric field to extract the ionization signal from recoil events as well.

At the final 1000-kg level, the full-scale detector ZEPLIN IV would be sensitive to a primary component of the dark matter having an interaction cross section of 10^{-46} cm^2. A sufficiently low threshold in the 10-keV recoil-energy range, and a low background rate are essential to achieve this sensitivity. Future plans are likely to include a CsI photocathode immersed in the liquid xenon (as described in Section 3.2.3).

We commend the ZEPLIN collaboration for their continued progress, both towards operating a competitive dark matter detector and in the development of a much larger xenon detector for dark matter research.

Summary of Liquid Xenon Dark Matter Projects:

The progress on liquid xenon detectors, by both the XENON and ZEPLIN collaborations, has been remarkable. It appears to SAGENAP that viable dark matter detectors larger than the present generation can be built successfully, given continued success with the R&D program. We encourage the agencies to continue to strongly support the R&D towards a possible large xenon dark matter detector.

4. Very High-Energy (VHE) Particle Astrophysics

4.1. VHE Particle Astrophysics Overview

During the last decade, very high-energy particle astrophysics has emerged as an exciting area of research. This field employs both ground-based and space-borne detectors to study the astrophysical sources of high-energy cosmic rays, gamma rays, and neutrinos. The primary scientific goals are twofold: (1) to use these multiple messengers to probe energetic processes occurring in cosmic accelerators such as supernovae, pulsars, active galactic nuclei (AGN), and gamma-ray bursts. (2) to search for evidence of new physics by detecting unique signatures of high-energy cosmic particles. As an example of the second goal, particles with energies above 10^{20} eV have been detected, but we do not understand how they could be produced. It may be that these particles signal new physics from a grand unified theory (GUT) mass scale.

Although there is no strict definition, high-energy astrophysics is generally defined by observations above 10 MeV. Experiments sensitive to particles above 100 GeV are generally considered to be in the very high-energy (VHE) regime and those sensitive above 100 TeV are in the ultrahigh energy (UHE) regime. For the purposes of this report, we simply use VHE to refer to the entire non-thermal energy range above 1 GeV. An important feature of non-thermal radiation is that energy spectra produced by astrophysical sources have a power-law form, $dN/dE \sim E^{-\alpha}$, where α is typically in the range of 1.5 to 3.0. Thus, the flux of particles from a particular source falls rapidly with energy and large detector collection areas are required to make meaningful observations at the highest energies.

Most of our direct knowledge of VHE astrophysics comes from measurements made by gamma-ray telescopes. At GeV energies, the EGRET satellite experiment detected over 270 distinct sources, including many AGN and a number of pulsars and gamma-ray bursts. At TeV energies, ground-based telescopes employing the air shower technique, especially the Whipple and HEGRA telescopes, have discovered about a dozen sources, including AGN, some pulsar nebulae and some supernova remnants. An exciting recent result is the detection of TeV gamma rays from the center of the Milky Way galaxy. This result is interesting because the galactic center is not thought to be a very active region (in the astrophysical sense), and thus there may be an unexpected, exotic source for these VHE particles (e.g. SUSY dark matter). Future experiments in space and on the ground with much greater sensitivity should greatly increase the number of VHE sources and expand our understanding of how they work. These experiments will also study the interaction of VHE gamma rays and cosmic rays with diffuse cosmic radiation fields, such as the CMB and the optical/infrared background.

In the U.S. program, Milagro, STACEE, and Whipple are operating ground-based gamma-ray telescopes. Two major, new telescopes are under construction: GLAST, the next-generation satellite instrument and VERITAS, an array of atmospheric

Cherenkov telescopes to be located in southern Arizona. An expression of interest for a possible high-altitude water Cherenkov detector, HAWC, was presented to SAGENAP.

The all-particle cosmic ray spectrum extends from GeV energies all the way up to 10^{20}eV, and possibly beyond. These particles provide strong evidence for VHE cosmic accelerators that are both highly luminous and efficient. Recently, much attention has been given towards understanding the origin of particles with energies above 10^{19} eV. Particles at these energies are not significantly deflected as they traverse our galaxy, and they may therefore be used for astronomical purposes. Perhaps even more compelling is the fact that particles with such extreme energies are difficult to produce in known astrophysical sources. In recent years, the AGASA experiment has presented evidence for the continuation of the cosmic-ray spectrum beyond 10^{20} eV. This result is surprising because such particles are not expected to travel far due to interaction with the CMB at the so-called Griesen-Zatsepin-Kuz'min (GZK) cutoff near $10^{19.8}$ eV. Cosmic rays with energies above the cutoff must originate in the relatively local universe (< 100 Mpc from Earth) where there are few, if any, plausible candidate sources. The AGASA results, if correct, add considerable mystery to the origin of the most energetic particles known.

In the U.S. VHE cosmic-ray program, the HiRes detector is currently operational in western Utah. The southern Auger Project observatory is under construction in Argentina. At this SAGENAP meeting, three possible VHE cosmic-ray instruments were presented: ASHRA, the Auger Project North, and the Telescope Array Low-energy Extension (TALE).

VHE neutrino telescopes offer the possibility of probing the deep interior of energetic sources. Neutrinos interact weakly with matter and they can escape from regions so dense that high-energy photons would be absorbed or degraded in energy. For this same reason, however, neutrinos are difficult to detect and VHE neutrino telescopes are required to have very large collection areas. Current telescope projects employ a sparse array of optical sensors that record the Cherenkov radiation emitted by neutrino-induced muons or neutrino-induced cascades. In the U.S. program, the AMANDA experiment is currently operational at the South Pole. IceCube is a major new observatory with a collection area ~ 1 km^2 that is currently under construction, also at the South Pole. The balloon-borne ANITA telescope, employing a radio technique to detect VHE neutrinos in the Antarctic ice, is under construction for an anticipated first launch in several years. Ultrahigh energy cosmic ray experiments, such as HiRes and the Auger Project, also have significant capabilities for detecting neutrinos using horizontal or nearly horizontal air showers.

4.2. Projects in VHE Particle Astrophysics

Four Category 1 and 2 projects were presented to SAGENAP in the area of VHE particle astrophysics: ASHRA, Auger Project North, HAWC, and the Telescope Array Low-energy Extension. Five Category 3 projects were presented: Auger Project South, HiRes, Milagro, STACEE, and VERITAS. Here we present our findings on these nine projects, arranged alphabetically.

4.2.1. ASHRA

ASHRA (All-sky Survey High-Resolution Air-shower detector) is a VHE astrophysics project that is currently under development by a number of institutions in Japan, Taiwan, and the U.S. The stated goals of the project are to detect and measure the properties of very high-energy astrophysical gamma rays at energies E > 1 TeV, ultrahigh energy neutrinos at energies $E > 10^{16}$ eV, and cosmic rays at energies $E > 10^{18}$ eV. To do this, the group plans to construct an array of telescopes capable of detecting extensive air showers via the signatures of atmospheric Cherenkov radiation and nitrogen fluorescence. The eventual array might comprise several stations, separated by 10-30 km, with each station consisting of multiple telescopes. The single telescope concept is based on a moderate sized (~3 m diameter) reflective optic and a wide-field camera using a solid-state detector at the focal plane. The group has some initial funding from Japan, where the prototype development has been carried out. The group presented a heads-up to SAGENAP regarding their plans to develop a proposal for U.S. funding that would aid in the design of ASHRA and in its eventual construction and operation. Mountain locations on the island of Hawaii are being considered as potential sites for ASHRA stations.

The general science goals of ASHRA are meritorious and worthwhile to pursue. Relatively little of the VHE gamma-ray sky has been surveyed with good sensitivity. We have yet to detect any sources of VHE neutrinos but we have very good reasons to believe that they exist. Finally, the mystery of the highest energy cosmic rays is extremely perplexing and it requires explanation. ASHRA is still in the relatively early stages of development. No clear detector design was presented to SAGENAP, and it appeared that complete simulations of the detector, the relevant physics, and the backgrounds have not yet been carried out. As such, then, it is difficult at the present time to evaluate the scientific reach and suitability of ASHRA. It must be demonstrated that a single experiment can make forefront measurements in two different areas of research (e.g. gamma rays and cosmic rays/neutrinos), using the same hardware but two markedly different detection techniques (e.g. atmospheric Cherenkov and nitrogen fluorescence, respectively). The ASHRA proponents will either need to make the case that they can, in fact, make valuable scientific contributions in both areas (and that their design is not overly compromised by doing so) or focus on the science in which they can make the greatest contribution.

More concretely, the case must be made that ASHRA will offer unique scientific capabilities beyond that provided by the existing suite of VHE astrophysics experiments. In particular, what new sensitivity to the gamma-ray sky can ASHRA provide that is not provided by GLAST and the new generation of ground-based Cherenkov telescopes? Similarly, can ASHRA make a significant contribution to the understanding of the ultrahigh energy cosmic rays, coming after experiments such as HiRes, the Auger Project, and the Telescope Array?

The ASHRA detector design is still under development. An important strength of the ASHRA concept is achieving excellent (1 arc-min) optical performance over a relatively wide field of view. To carry this out, a high-density camera, with very high (mega-pixel) channel count is required. Good optical performance should allow for good reconstruction of the air shower trajectory using either the Cherenkov or fluorescence signatures, although it is not clear without detailed simulations how much of an improvement in angular resolution can, in practice, be achieved. In the case of the reconstructing the direction of TeV gamma rays, the angular resolution of a Cherenkov imaging telescope is largely limited by the inherent resolution of the image itself from shower fluctuations ($\sim 0.05^o$) and by uncertainty in the position of the shower core. In the case of ultrahigh energy cosmic rays and neutrinos, the main issue at the present time is detector aperture (km^2-sr) and not angular resolution.

In summary, ASHRA is a developing concept for an observatory of VHE gamma rays and ultrahigh energy cosmic rays. Some initial prototype work has been done, but the group needs to carry out complete and realistic simulations in order to demonstrate the overall sensitivity of the experiment. The proponents need to prove that their sensitivity (to either VHE gamma rays or ultrahigh energy cosmic rays) will in fact lead to significant scientific advance. No evidence of this capability was presented to SAGENAP. More programmatically, the role and importance of the U.S. institutions in ASHRA need to be clearly delineated and the role of ASHRA in the broader picture of experiments probing VHE astrophysics should be understood. At the present time, the case for ASHRA, scientifically, technically, and programmatically, has not yet been established.

4.2.2. Auger Project South

The Pierre Auger (South) Observatory, located near Malargue, Argentina, is designed to study the highest energy cosmic rays ($E>10^{19}$ eV) with the sensitivity needed to make a statistically meaningful measurement of the cosmic ray flux above the GZK cutoff. Earlier measurements at these energies by the HiRes group are consistent with the existence of a cutoff at the expected energy (roughly $10^{19.8}$ eV), while measurements from the AGASA array seem to indicate a significant flux of particles above this energy. The sizable energy loss fluctuations experienced by ultrahigh energy cosmic rays in interactions with the cosmic microwave background complicate the interpretation of these existing low statistics measurements. A high statistics measurement of the ultrahigh energy cosmic ray spectrum, such as will ultimately be provided by the Auger Observatory, is needed to definitively settle this issue. Equally important, the Auger Observatory combines two complementary techniques for measuring the cosmic ray energy, a surface detector array and atmospheric fluorescence detectors, allowing for cross checks and control of systematic errors.

The Auger Collaboration has made impressive progress in validating the performance of the detectors using data from the engineering array, as well as in deploying the full array. As of April 2004, 350 out of a total of 1600 surface array stations were deployed, with 250 operational. In addition, ten of the 24 fluorescence detectors were operational. The production and deployment schedule calls for the full detector to be online by the end of 2005, if funding shortfalls are addressed. The collaboration leadership is justified in being proud of the excellent progress on the construction and initial operation of the detector.

Performance of the engineering array has exceeded expectations, in particular in the efficiency for detecting low energy showers. Engineering array data indicates that the array is fully efficient at energies greater than $3x10^{18}$ eV, with sensitivity extending down to approximately 10^{17} eV. To date, roughly 100 events with energies greater than 10^{19} eV have been collected, although energy assignments are still preliminary. At the end of 2004 the total exposure of Auger South should match, or exceed, that of AGASA, falling short at that time of matching the full HiRes monocular exposure.

Plans for deployment of the full array are currently jeopardized by a funding shortfall resulting from economic problems in Argentina, Brazil and Mexico. This shortfall comes to roughly $10M. The collaboration leadership indicates that in making up this shortfall they have secured an additional $2M in support from other participating countries, with an additional $6M likely. Requests for the remaining $2M are pending. The Auger collaboration currently estimates that roughly 1000 of the 1600 surface stations can be constructed and deployed assuming the full $10M funding shortfall, resulting in a detector with 60% of the design sensitivity for 80% of the total design cost.

SAGENAP congratulates the Auger collaboration for their excellent progress in constructing and commissioning the southern Auger observatory. We commend DOE and NSF for providing additional funds to the Auger group, and urge the agencies to continue to work with the collaboration to help resolve the remaining issues.

4.2.3. Auger Project North

The Auger collaboration has always planned to build a second observatory in the northern hemisphere to enable complete sky coverage with the same hybrid technique. For various practical reasons, including funding, the collaboration chose to build the southern site first. The southern observatory is now nearing completion (see 4.2.2), and SAGENAP was given a heads-up presentation and Letter of Intent for a future proposal to build the northern Auger Project Observatory.

The scientific motivation for the northern site should be considered in the context of the primary scientific motivation for Auger as a whole, which can be summarized as follows:

1. Confirm or refute the existence of anomalous trans-GZK events with high statistics and reduced systematic uncertainties.
2. Search for directional anisotropies on all spatial scales, along with composition information, possibly opening a new field of ultrahigh energy cosmic-ray astronomy.

The importance and character of the second will depend, in part, on the answer to the first. If the existence of trans-GZK events is established, there is a high probability that they are a signal of new physics; in that case, it will be of fundamental importance to understand the origin and propagation of these immensely energetic particles. Since such high-energy events will very likely point back to their source(s), the spatial information will be indispensable. The experimental evidence for trans-GZK events has arguably weakened since the start of construction of Auger Project South. The HiRes collaboration now reports a flux consistent with the GZK cutoff, due to an improved understanding of the calibration of their air shower fluorescence measurement. However, the AGASA collaboration maintains an excess of events. The two results, based on two different techniques, disagree at the several standard deviation level.

Because of geography, the hole in the sky coverage of Auger Project South almost exactly corresponds to the location of the AGASA excess events. This poses a problem related to the first motivation for the Auger Project: if the southern observatory were to observe no excess trans-GZK events, it *could* be because the AGASA events are coming only from the direction in the northern hemisphere. Proceeding with Auger Project North prior to learning the results from the southern observatory is therefore a judgment call, and we outline here the issues that should be clearly addressed in any future proposal for Auger Project North.

The proponents argue that the right way to do the experiment is to have complete, uniform sky coverage – a statement that is certainly true, in principle, but the incremental benefits of the northern site must be carefully considered in a broader context. Why is Auger South, with excellent coverage over half the sky, not sufficient to address the key scientific objectives? For the most uniform exposure, the Northern and Southern observatories should be as similar as possible. Small design changes due to geography will likely be necessary; however, there were indications during presentation that the team is considering a re-optimization of the design of the array (size and density). A proposal should show why any changes, including those that offer additional science potential, are also fully consistent with the motivation of uniform full-sky coverage. Regarding the selection of a Northern hemisphere site, other experiments, notably HiRes and VERITAS, have had site difficulties. Development impediments at the Northern Auger site should be studied and addressed as fully as possible in a proposal. Finally, it is important that the development of Northern Auger not come at the expense of the completion, commissioning, and operation of Southern Auger. For example, the pool of experienced members of the group should not be stretched too thin.

To summarize, the scientific case for the Auger Project North has not yet been established. At the present time, SAGENAP is unable to make a statement as to the merits of Auger North without a more complete proposal that addresses the concerns raised here.

4.2.4. HAWC

The High-Altitude Water Cherenkov (HAWC) concept is being developed as a next-generation extensive air shower (EAS) VHE gamma-ray detector. EAS arrays detect the particles from the air shower initiated by gamma rays (or by charged cosmic ray backgrounds). EAS arrays offer continuous operation and a wide field of view, but possess relatively poor energy resolution and relatively high-energy threshold, inherent in the shower "tail-catcher" calorimetric technique. HAWC is a next-generation version of the existing Milagro telescope that uses a pond of water to detect air showers via the water Cherenkov technique (see 4.2.6.). As envisioned, HAWC differs from Milagro substantially in two significant ways: the detector is larger (and it contains many more photomultiplier tubes, approximately ten times the number of Milagro) and it is sited at much higher altitude (~5,000 m vs 2,000 m for Milagro). Two sites, one in Tibet and one in Chile, are currently being considered. The proponents of HAWC are experienced members of Milagro. However, some of the Milagro group has moved onto other projects and SAGENAP has concerns that the present collaboration for HAWC is not sufficient to carry out this relatively ambitious project. As the HAWC concept is now being developed to the proposal stage, a heads-up presentation was given to SAGENAP.

The basic physics case for HAWC rests in large part on the results from Milagro. The instrumentation for Milagro has been successfully completed, and the detector (including outriggers) is operating well. This is a commendable achievement. One of the most important characteristics of the gamma-ray sky is variability, and the ground-based Cherenkov telescopes have small fields of view (typically several degrees across); thus, Milagro offered promise as a TeV full-overhead-sky monitor. While it is true that Milagro has detected well-established sources such as the Crab Nebula and Markarian 421 and the Milagrito engineering array may have observed a BATSE-triggered burst at low statistical significance, Milagro has not yet provided a single, true TeV transient alert. Other results, such as the preliminary evidence for diffuse galactic plane emission and limits on the detectable TeV gamma-ray flux from bursts, are very interesting; however the unique physics contributions of Milagro thus far do not by themselves provide an immediately obvious and compelling case for a follow-on experiment.

A future proposal should address the unique contributions that HAWC would make, in much more detail than was provided in the presentation to SAGENAP. In particular, a number of questions should be considered:

(1) While short-timescale variability of AGN is extremely interesting, it will be studied with much greater sensitivity by the next generation of atmospheric Cherenkov telescopes (ACTs). GLAST will provide a very effective all-sky (not just overhead) monitor, so a HAWC proposal must provide clear arguments why prompt 0.1 – 1.0 TeV gamma-ray measurements of AGN, with poorer resolution and less sensitivity than ACTs, are essential.

(2) What contributions will HAWC provide as a result of monitoring sources discovered by GLAST? Although GLAST will detect thousands of AGN, the vast

majority will be probably be invisible to HAWC. How many sources, and what types, might possibly be seen with HAWC?

(3) Although HAWC may see ~50 GeV emission from GRBs, the delayed emission will be observable from the ACTs with greater sensitivity after a trigger from GLAST and/or SWIFT, along with the prompt emission from the brightest bursts in this energy range by GLAST directly. Why is it necessary to see a relatively small number (perhaps 5-10 /year) of bursts with HAWC?

(4) In regards to distant sources such a AGN or GRBs that can be used to probe cosmology and study possible violation of Lorentz invariance, infrared absorption above 100 GeV limits the visible distance to redshifts $z < 1$, but the sources are known to be at larger distances; thus, what does HAWC offer over GLAST, which will measure GRB light curves and photon arrival times over a very wide energy range, unattenuated by IR absorption, to much larger redshifts and with much better energy resolution? Detailed analysis, beyond the figure of merit shown in the presentation, should be given to elaborate on HAWC's unique contributions.

The technical details for the implementation of HAWC are still in the relatively early stages. Complete end-to-end simulations are required that should show what a detected signal for a gamma-ray transient source would look like, even with modest energy resolution. Given the fact that the Milagro detector required an outrigger detector upgrade to properly reconstruct showers, the simulations must incorporate realistic estimates for the eventual performance of HAWC. Even with an optimistic schedule for construction, and given the experience with Milagro and the more remote site for HAWC, it is likely that observations will not begin before 2011. By that time, GLAST will have been in operation for four years and the next-generation ACTs will have been observing even longer, possibly with incremental upgrades. The HAWC physics case should be put into that context. Finally, the proposal should outline the complete scope of the project, including site-related support. SAGENAP is concerned that the initial cost estimate of $30M was likely to be too low. As mentioned above, we believe that the HAWC collaboration may need to be stronger to be viable in carrying out the project.

In summary, SAGENAP finds that the scientific case for a high-altitude, water Cherenkov detector such as HAWC is not yet very strong. The science case must be justified (in some detail) in the context of the unique physics that HAWC would provide, beyond the detection of some new sources, that would not be provided by other operating gamma-ray telescopes (e.g. GLAST, VERITAS, HESS, etc.). There are also technical and cost issues that should be addressed in a future proposal.

4.2.5. HiRes

The Fly's Eye High Resolution (HiRes) detector is designed to probe the nature of ultrahigh energy cosmic rays, especially the those particles with energies $E > 10^{19}$ eV. HiRes detects cosmic rays via the atmospheric fluorescence technique. It consists of two detectors separated by ~13 km, located on Dugway Proving Grounds in western Utah. Each detector is comprised of banks of mirrors that image fluorescence light produced in air showers onto photomultiplier tube cameras. The fluorescence technique is inherently calorimetric in that the amount of isotropic light produced in the air shower is directly proportional to the energy of the particle initiating the shower. However, accurately determining the energy spectrum of the detected particles requires good understanding and control of the statistical and systematic errors associated with the transmission of the light through the atmosphere and the absolute calibration of the optical components of the detector. HiRes observes showers with either one (monocular) or both (stereo) detectors. The aperture for monocular reconstruction is larger than that for stereo and thus the monocular data sample has the greatest statistical power.

HiRes presented results from approximately four years of data taking with a single detector and approximately two years of data taking with two detectors. The most important result is the monocular energy spectrum that shows evidence for a spectral cutoff at energies above $10^{19.8}$ eV, as expected from the GZK effect and in contradiction to the AGASA result. HiRes reports the detection of 11 events with $E > 10^{19.8}$ eV, where 29 are expected if the spectrum continued unabated – this corresponds to a Poisson probability of ~ 1×10^{-4} that the data are consistent with a constant power-law spectrum. The stereo energy spectrum is consistent with the monocular one, but with substantially weaker statistical power. Another important result from HiRes concerns the search for anisotropy in the arrival directions of the cosmic rays. No significant anisotropy was detected, a result which again is inconsistent with the AGASA data. There is no compelling evidence so far in the HiRes data for the existence of individual point sources. Additional results from HiRes include improved measurements of the cosmic ray composition and spectral measurements at lower energies near 10^{18} eV.

Current plans call for the operation of HiRes until 2007 with the anticipated final data sample being three to four times larger than that used for the present results. All analyses will significantly improve with the additional data sample. SAGENAP congratulates HiRes on its successful operations to date and for the publication of several new, significant results. We believe that the plans for continued operations until 2007 are very well motivated.

4.2.6. Milagro

Milagro, located at Fenton Hill, NM, consists of a large, instrumented pond that detects extensive air showers via the water Cherenkov technique. Milagro provides coverage of the overhead sky for gamma rays at energies greater than ~ 1 TeV. Unique capabilities of Milagro include a high duty cycle and very wide field of view. In 2000, the construction of outrigger detectors around the main Milagro pond was proposed in order to provide enhanced angular resolution and sensitivity. These outriggers have now been deployed by the Milagro group, and they have been shown to improve the sensitivity and event reconstruction of the detector. The Milagro collaboration is to be commended for successfully completing this upgrade to the detector.

To date, the Milagro collaboration has conducted an ongoing search for transient sources, and it has produced a survey of the northern hemisphere sky that indicates that there are, at most, only a few strong, steady-state sources of TeV gamma rays (other than the well-studied Crab Nebula). Recent results from Milagro include the detection of diffuse TeV emission from the galactic plane and the possible discovery of two diffuse sources of TeV gamma rays (one near the Crab Nebula and another in the Cygnus arm).

Although it is unfortunate that very few steady point sources exist at the sensitivity level of Milagro, an important continuing motivation for the instrument derives from its ongoing search for transients. A critical aspect of this work is the formal alert system that Milagro is developing. SAGENAP encourages the Milagro collaboration to ensure that such a system is in place soon and routinely tested through periodic test alerts. The usage and availability of the alert system should also be well publicized throughout the VHE astrophysics community.

Regarding the long-term future of Milagro, SAGENAP expects that Milagro will continue to be fully operational for a period of roughly two years following the launch of SWIFT. Milagro offers unique capabilities in wide-field gamma-ray detection that are not available with any other instrument before the GLAST launch. After that time it would be appropriate to consider whether continued operation of Milagro is justified by its likely future scientific return.

4.2.7. STACEE

The Solar Tower Atmospheric Cherenkov Experiment (STACEE) is a ground-based high-energy gamma-ray observatory, located at the National Solar Thermal Test Facility (NSTTF) of Sandia National Laboratories in New Mexico. A large array of mirrors, called heliostats, was built to carry out solar energy research. At night, the heliostat array reflects atmospheric Cherenkov light from gamma-ray and cosmic-ray induced air showers onto the STACEE detector, which consists of an array of phototubes and associated electronics. The very large mirror area of STACEE results in an energy threshold that is significantly lower than almost all other ground-based gamma-ray telescopes.

The important benefits of the lower-energy capability include: significantly improved statistics (because the sources have power law spectra) for variability studies, greater sensitivity to sources that are further away, due to attenuation of the high-energy flux from photon-photon interactions with the extragalactic background light, and enhanced ability to probe the extragalactic background light using the attenuation effect, thereby testing models of galaxy formation; and raising the possibility of observing O(100) GeV photons from gamma-ray bursts, which are known to occur at cosmological distances.

Installation of the detector and electronics was completed in 2002, and the experiment has been running with 64 heliostat mirrors, with significant sensitivity below 100 GeV. The observations between 2002 and 2004 concentrated on detector studies to improve the energy and arrival direction reconstruction, and on a number of astrophysical sources, including the Crab Nebula, several AGN, plus targets of opportunity that included three gamma-ray bursts. Due to favorable conditions, the collaboration has been able to observe 6-8 sources per year, with a total of about 40 hours on each source.

In addition to detecting the Crab Nebula and rapid flares from the AGN Markarian 421, recent STACEE observations of the EGRET blazar W Comae resulted in new flux upper limits at ~130 GeV that impact current AGN models. The first-stage observations of H1426+428 (which was not detected by EGRET) show promise for constraining EBL absorption models. Another result will follow from extended observations taken on the source 3C66A, a more distant AGN with a nominal redshift of $z = 0.4$.

The STACEE collaboration plans to continue observations at least until the 2006/7 timeframe, so as to have an extended overlap in time with the SWIFT gamma-ray burst satellite. The scientific motivation to operate STACEE beyond 2007, when both VERITAS and GLAST should be operational, will require re-evaluation.

4.2.8. Telescope Array / Telescope Array Low-energy Extension

The Telescope Array (TA) is an experiment to study ultrahigh energy cosmic rays, with the primary motivation being the resolution of the current discrepancy in the measured cosmic ray flux above 10^{20} eV as reported by the AGASA and HiRes groups. The TA group (a collaboration of institutions from Japan, the U.S. and Taiwan) hopes to resolve this controversy by deploying a ground array similar to AGASA and fluorescence detectors similar to HiRes (see 4.2.5) at the same site. Plans for TA include a ground-array of 576 scintillation counters covering 800 km^2, along with three fluorescence detectors located on the periphery of the ground array. Funding for the construction of the ground array has been approved in Japan. The Letter of Intent provided to SAGENAP includes funding for site development and infrastructure in support of the TA project. In the presentation to SAGENAP, there was no substantive discussion of the scientific case for TA or of the role of the U.S. institutions in the overall TA project. Similarly, there was no discussion of the scientific importance of the expected results from the Telescope Array in the context of the general landscape of ultrahigh energy cosmic-ray research during the next 5-10 years.

The presentation to SAGENAP focused on a proposed low energy extension to TA, called the Telescope Array Low-energy Extension (TALE), which would combine TA with two HiRes stations and a new tower detector, with the goal of extending the sensitivity of TA down to energies of roughly 10^{17} eV. The TALE physics program uses measurements of cosmic ray anisotropy, composition, and spectrum to infer the nature of the sources of cosmic rays in the energy range of 10^{17}eV to 10^{19}eV. In this energy range there is clear evidence for a discontinuity in the spectral slope between 10^{18}eV and 10^{19}eV (the ankle), as well as possible evidence for a discontinuity near 10^{17} eV. Such discontinuities have traditionally been associated with transitions from one source of cosmic rays to another.

A definitive measurement of cosmic ray composition in the 10^{17}eV to 10^{19}eV energy range (that could in turn be associated with a specific cosmic ray source class) would be of great interest. However, experience has shown that the power of indirect measurements of cosmic ray composition to elucidate the nature of cosmic ray sources is modest, at best, and it is unclear what additional light will be shed on these sources by an improved composition measurement of the type already available from HiRes data. Separately, although the measurement of anisotropy in the cosmic rays would have an important impact on our understanding of possible sources, the TALE proponents did not demonstrate that such a measurement is feasible given the expected event sample size (and the intrinsic isotropy of cosmic rays in the low energy portion of the TALE energy range in which most events will be observed). There are also questions regarding the effectiveness of the tower detector, which nominally lowers the energy threshold of TALE from 5×10^{17}eV to 10^{17}eV.

In summary, SAGENAP finds that the Telescope Array/TALE group has not yet made a very strong case, both scientifically and technically, for the low energy extension to the Telescope Array. In addition, the scientific merit and overall context of the Telescope Array project was not presented in any detail and was therefore not evaluated. The case for the Telescope Array, and the role of the U.S. institutions in the project, must be justified before proceeding with investment in the Telescope Array and TALE.

4.2.9. VERITAS

VERITAS (Very Energetic Radiation Imaging Telescope Array System) is one of the four major new VHE gamma-ray atmospheric Cherenkov Telescope (ACT) arrays currently under construction. Each of these new observatories represents an important jump forward in sensitivity and capability. The four arrays are distributed around the globe (two in the northern hemisphere, two in the southern hemisphere, at different longitudes) so, together, they provide good sky accessibility. VERITAS is being built by a collaboration of scientists from the U.S., Canada, the U.K., and Ireland, a number of whom were leaders of the pioneering Whipple Observatory gamma-ray telescope on Mt. Hopkins (AZ). The Whipple Observatory has an impressive list of scientific accomplishments, including the discovery of numerous sources in the northern hemisphere (e.g. the Crab Nebula and AGN such as Mrk 421, Mrk 501, and 1H1426), the discovery of spectral variability in sources such as Mrk 421, the establishment of correlated X-ray/TeV gamma-ray emission from AGN, and the recent, intriguing result of VHE emission from the galactic center.

As an array of 12m diameter telescopes, VERITAS is the next-generation experiment that is a logical extension of a proven technique. An array design provides much better background rejection capabilities, allowing a significantly lower energy threshold. The design also permits better energy and angular resolution, larger effective collecting area to expand the capabilities at high energy and, with enough telescopes, the ability to study more than one source at a time with sub-arrays when the full collecting area is not required. An important, distinguishing characteristic of VERITAS is the use of a readout employing a deep, yet high-speed, Flash-ADC. This feature should permit the full information inherent in the Cherenkov signal to be utilized for event reconstruction and background discrimination.

As many previous reviews of the project have concluded, the scientific motivation for VERITAS is very strong. Due to insufficient funding, the collaboration is initially building four telescopes rather than the seven telescopes put forward in the original proposal. The major loss in going from seven to four telescopes is one of flexibility – essentially half the number of sources can be observed with the smaller array, but a particular source can be studied with comparable sensitivity to the full array. The four-telescope VERITAS will be completed in 2006, prior to the anticipated launch of GLAST in early 2007. GLAST will survey the whole 0.02 GeV - 300 GeV sky, monitoring for transients, and VERITAS will study sources with greater sensitivity at high energies (E > 100 GeV) and will help resolve the short-timescale variability. VERITAS and GLAST will have, for the first time, practical sensitivity in overlapping energy ranges, allowing complementary searches for dark matter signatures and spectral measurements for a variety of important sources.

The earlier site issues resolved, VERITAS will be located in Horseshoe Canyon on Kitt Peak, AZ, at an elevation of 1,700 m. A prototype telescope has been constructed at the Whipple Observatory base-camp where it demonstrated that the basic design is sound (i.e. encompassing the optical and mechanical structure, the camera, and the data acquisition). The prototype will be completed in 2005 as the first telescope and it should carry out observations at the base-camp for at least one year. It will then move to Kitt Peak. Construction of the remaining three telescopes has also commenced. The management, science collaboration, and advisory structures are all in place.

The collaboration remains firmly committed to the full seven-telescope array, which is not currently funded, viewing the construction of VERITAS-4 as the first stage of the project. Some planning has started towards the development of future, much more powerful gamma-ray telescope arrays using the atmospheric Cherenkov technique. SAGENAP congratulates the VERITAS collaboration on their excellent progress to date.

5. Neutrinos

5.1. Neutrino Overview

Most of what we know about neutrinos we have learned during the past decade. With the discovery and subsequent laboratory confirmation of neutrino oscillations from cosmic rays and from the sun, we now know that neutrinos are massive particles that violate lepton family number conservation by virtue of being mixed lepton flavor states. Such rapid changes in our understanding of this least-explored sector of the particle "periodic table" has provoked intense interest among physicists. Neutrino measurements typically rank among the most cited of recent papers in physics.

Neutrino oscillation studies reveal two basic oscillation length/energy (L/E) scales: (1) the "solar" scale with L/E of around 15 km/MeV with amplitude in the range $\sin^2 2\theta$ = 0.7-0.9, and (2) the atmospheric scale of around 0.5 km/MeV with amplitude in the range $\sin^2 2\theta$ = 0.9-1.0. Measurements of the directional dependence of neutral current (NC) events in atmospheric neutrinos and the absolute rate of NC events in solar neutrinos disfavor oscillation to a non-interacting "sterile" neutrino. Additionally, statistical studies of showering events in the atmospheric data favor oscillation to mostly ν_τ. Taken together, these data imply near-maximal mixing of ν_μ and ν_τ at a mass scale difference of $\Delta m^2 \sim 2\text{-}3 \times 10^{-3}$ eV^2. Additionally, solar and reactor neutrino oscillations imply a $\Delta m^2 = 8 \times 10^{-5}$ eV^2, with ν_e oscillating to a mixture of ν_μ and ν_τ. Evidence for matter effects in the solar measurements favor $m_1 < m_2$, but the sign of the mass-squared difference for $m_{1,2}$ and m_3 is not determined by current atmospheric and accelerator experiments. In addition to the uncertainty in the level ordering, there is also uncertainty in the overall mass scale, which is limited in the laboratory by beta decay to be less than 2.2 eV. Oscillation studies are not sensitive to neutrino mass directly but only to the difference of the mass squared.

For some nuclei, beta decay is forbidden but the process emitting two beta particles (double-beta decay) is allowed. Two-neutrino double-beta decay, an allowed second-order weak interaction process that produces two electrons and two anti-neutrinos, has been observed in nature. The more interesting process where only two electrons appear in the final state (zero-neutrino double-beta decay) requires special neutrino characteristics in order to exist. In fact, the Majorana neutrino mass is directly related to the rate of this process. Hence if zero-neutrino double-beta decay is observed, it would demonstrate that the neutrino is a Majorana particle, that it is massive, and that lepton number is violated.

Neutrino mixing is also not completely understood. The standard formulation of the mixing matrix has three independent mixing angles (θ_{12}, θ_{13}, θ_{23}), a CP-violating phase (δ), and possible two CP-violating Majorana phases (α_1, α_2). Although the values of θ_{12} and θ_{23} are reasonably constrained by current experiments, only an upper limit is known for θ_{13} and the values of δ and the α's are completely unknown. Thus there are still many unanswered questions that require experimental investigation.

The Z_0 decay-width measurements indicate that the number of light active neutrinos is very near 3. However, there is the possibility that "sterile" neutrinos might also exist. (In this context a sterile neutrino is one that does not couple to the Z_0.) In fact, "see-saw" models predict Majorana neutrinos and naturally provide right-handed sterile neutrinos at a very high mass. Interest in the possibility of light sterile neutrinos has arisen lately due to the LSND result. The most common way to accommodate this anomalous result within the neutrino oscillation paradigm is to include one or more sterile neutrinos that mix with active neutrinos.

If light sterile neutrinos exist, they have potential to greatly affect many astrophysical processes, especially if they mix with active neutrinos. For example, the addition of a light sterile neutrino that weakly mixes with active states leads to better fits of the solar-neutrino energy spectrum. Active-sterile neutrino mixing models can produce the neutron abundance required for heavy element production in the supernova R-process. Asymmetries in supernova explosions can be maintained by sterile neutrinos (whereas active neutrinos scatter reducing any asymmetry), and therefore sterile-neutrino models can explain pulsars with large velocity kicks. A sterile neutrino with a mass near 100 keV is a tantalizing candidate for cold dark matter. Models of mass varying sterile neutrinos, tied to the expansion of the universe through a scalar field, can naturally explain the puzzling acceleration of this expansion observed in later epochs.

Several new neutrino experiments are being constructed or proposed in an effort to build on the success of the current program and to look beyond the Standard Model. They typically seek to answer one or more of the following questions:

(1) Is the neutrino inherently a Dirac or Majorana particle, and is lepton number violated?

(2) What is the absolute mass scale of the neutrino?

(3) Is the mass ordering of the neutrino states "normal" (m_1, m_2, m_3) or "inverted" (m_3, m_1, m_2)?

(4) Is the mass ordering near-degenerate (with the oscillations sensitive only to the small differences between them) or hierarchical (in which case the measured Δm^2 represent values close to the absolute mass scale).

(5) What is the value of the angle θ_{13}, which seems to be smaller than the other mixing angles (90% c.l. limit: $\sin^2 2\theta_{13}<0.2$)? How close is the value of θ_{23} to maximal?

(6) Do neutrinos violate CP? What are the values of δ and the α's?

New generation experiments presented to SAGENAP, as status reports or for context, can be generally divided into four different areas by the techniques used: (1) double-beta decay experiments and beta decay endpoint experiments, (2) long-baseline accelerator experiments, (3) medium-baseline reactor experiments, and (4) low energy, real-time solar neutrino experiments.

Beta decay experiments: As discussed above, if zero-neutrino double-beta decay is observed, it demonstrates that the neutrino is a massive Majorana particle and that lepton number is violated. The atmospheric oscillation results indicate that at least one neutrino has a mass greater than approximately 50 meV. Many of the designs for the next-generation double-beta decay experiments will have sensitivity to an effective Majorana neutrino mass at this level. Furthermore, double-beta decay experiments are the only feasible technique to explore the Majorana/Dirac nature of the neutrino. The EXO collaboration presented a status report on their progress towards a xenon-based double-beta decay experiment.

Long-baseline accelerator experiments: Beams of ν_μ with L/E ~ 300 km/GeV are being constructed in the U.S. (NuMI), Japan (K2K, T2K), and Europe (CNGS) to make precision measurements of Δm^2 and θ_{23} and to look for a non-zero value of θ_{13}. The latter measurement would arise from the appearance of ν_e in the originally pure beam of ν_μ. The rate of appearance depends not only on θ_{13} but also on δ and the sign of Δm^2 (because the presence of matter can enhance or inhibit the oscillation depending on the sign of Δm^2). While making the interpretation of the results more difficult, this additional sensitivity offers the promise of combining results from several experiments to deduce the value of all three parameters. Accelerator beams also have the great advantage of being able to run anti-neutrinos in addition to neutrinos to sort out the ambiguities. These experiments offer the only feasible way to look for CP violation in neutrinos. The sub-sub-panel heard presentations on the status of the ICARUS project and a report for context on the long-baseline program.

Medium-baseline reactor experiments: The current best limit on θ_{13} comes from the CHOOZ reactor experiment at L/E ~ 250. The limiting factor for this measurement was knowledge of the absolute flux and the proton fraction in the scintillator. By using multiple detectors and building on lessons learned from CHOOZ, Palo Verde, and KamLAND, it seems that an order of magnitude improvement in sensitivity (or better) is possible. These experiments have the great advantage that the θ_{13} measurement is unambiguous, since it does not depend on δ or on the details of the mass hierarchy. The sub-panel heard a heads-up presentation on future reactor-based experiments.

Real-time, Low Energy Solar Neutrino Experiments: There is a world-wide effort to measure the time-resolved energy spectrum of solar neutrinos produced by both the beryllium-7 and p-p reactions. Measurement of the beryllium neutrino capture line would be a check on the solar oscillation results and would in addition provide information on isotropic mixing and nuclear fusion in the solar interior. Similarly, a precision measurement of the p-p flux has the potential to reduce the experimental uncertainty in the associated neutrino-mixing angle and also provide information on the

CNO component of solar energy generation. Currently, both KamLAND in Japan and BOREXINO in Europe seek to measure the beryllium line, while several concepts are under development as potential p-p neutrino experiments.

At the time of the SAGENAP review of non-accelerator neutrino physics, a number of other studies involving this topic have either been completed or are still in progress. Two activities in particular need to be considered, as their outcomes will influence any planned program in neutrino physics. One is the American Physical Society Multidivisional Neutrino Study, in which a coordinated program of experimental neutrino physics is to be suggested for the DOE and the NSF. A second activity now in progress is a three-stage solicitation by the NSF for proposals for a possible deep underground science and engineering laboratory. If the underground laboratory is built, it would enable a range of scientific and engineering projects, including neutrino detectors, double-beta decay experiments, nuclear astrophysics accelerator-based experiments, and dark matter detectors.

Several other studies have been completed recently in which comments or recommendations about neutrino physics are salient. The National Research Council (NRC) “Connecting Quarks with the Cosmos” report (see 2.1) identified eleven significant scientific questions, of which three concerned neutrinos:

- What is the dark matter?
- What are the masses of the neutrinos, and how have they shaped the evolution of the universe?
- How do cosmic accelerators work and what are they accelerating?

Recent theoretical work raises the possibility that a fourth question, “What is the nature of the dark energy?” is also connected to neutrinos.

A second NRC review, “Neutrinos and Beyond: New Windows on Nature,” was commissioned to examine the roles of a deep underground laboratory and the Antarctic ultrahigh energy cosmic-ray experiment IceCube. The report strongly endorsed the scientific goals of both projects.

5.2. Projects in Neutrino Physics

Two Category 1 and 2 projects were presented to SAGENAP in the area of neutrino physics: ICARUS and LANNDD. Three Category 3 projects were presented: EXO, KamLAND, and Super-Kamiokande. In addition, the sub-panel heard general presentations on activities associated with measuring the mixing angle θ_{13} and associated off-axis neutrino beam experiments. Here we present our findings on the five projects in neutrino physics, arranged alphabetically, followed by a summary of the general presentations on measuring θ_{13}.

5.2.1. EXO R&D

EXO (Enriched Xenon Observatory) is a proposed experiment to search for neutrinoless double-beta decay. As discussed earlier, zero-neutrino double-beta decay is the only feasible process that can discern whether the neutrino is a Majorana or Dirac particle. Furthermore, double-beta decay has the potential to measure the absolute mass scale of the neutrino. The science of double-beta decay is thus extremely well motivated. The EXO project is currently in an R&D phase to develop technology to detect the daughter (Ba-136) of Xe-136 double-beta decay. To detect the daughter, the collaboration intends to extract the ion from the liquid xenon source in real time coincidence with the xenon decay. If successful, this program has to potential to eliminate, or greatly reduce, all background, except that due to the two-neutrino decay of Xe-136.

The daughter identification technique considered by EXO is a very aggressive design because the technology of extracting a lone ion from a large liquid sample and transferring it to a trap operated at vacuum is unproven. Furthermore, good energy resolution, required to reduce the two-neutrino mode background, is a critical component in any double-beta decay experiment. Encouragingly, the collaboration has identified the critical technical developments required for this project and their research over the last few years has yielded results.

The EXO group has demonstrated that by measuring both the ionization and scintillation signals of a charged particle interaction in liquid xenon, one can significantly improve the energy resolution. They have obtained sufficient resolution using this technique, thus eliminating an important technical risk. They have also built and operated the necessary optical trap. Although not a technical breakthrough, building the trap demonstrates that the collaboration now has those necessary skills. In addition, this trap is now being used to study how well lone ions can be detected optically when a background gas is present. These measurements are not yet complete, but they are progressing and the results look promising. Finally, the group has begun to study the extraction of the ion itself. This research is still in the early stages and it will be some time before the important measurement of the extraction efficiency is made.

In parallel, the EXO collaboration is designing and constructing an underground laboratory for a 200-kg enriched xenon prototype to be sited at the Waste Isolation Pilot Plant (WIPP) in Carlsbad, NM. Although this apparatus will not contain the ion extraction system, at least initially, it will provide a number of critical tests, including a planned first-time measurement of the two-neutrino rate and a demonstration of the energy resolution on an experimental scale.

The EXO collaboration has assembled a strong team and is making excellent progress in their R&D program. The science being tackled by EXO is important and their approach is one that should be aggressively pursued to understand if it is viable. The size of their collaboration is adequate for the R&D phase, but it appears that it will have to increase if the project is to proceed as a ton-scale, or larger, experiment.

5.2.2. ICARUS

ICARUS (Imaging Cosmic And Rare Underground Signals) is a planned 3-kton liquid argon time projection chamber (TPC), to be located at the Gran Sasso Underground Laboratory (LNGS) in Italy. ICARUS is the culmination of more than twenty years of development by mostly European groups, with small but important U.S. participation. The stated goals of ICARUS include: (1) the search nucleon decay modes that are difficult to discern in water Cherenkov detectors, (2) the measurement of the flux of solar neutrinos above ~5 MeV, (3) the detection of ν_τ appearance in a pure ν_μ beam generated at CERN, and (4) precision measurements of Δm^2, $\sin^2 2\theta_{23}$ and possibly a non-zero θ_{13} in the same beam.

A 600-ton module (T600) has been fully tested and the capabilities firmly established. Tests with the T600 have proven that liquid argon detectors can unambiguously detect and identify interactions such as K^+ and π^0 decay and decays of residual excited nuclear states on an event-by-event basis. This will allow ICARUS to investigate possible nucleon decay modes such as $p \rightarrow e^+ + \nu + \nu$ via nuclear de-excitation and $p \rightarrow K^+ + \nu$. In the case of the first decay, water Cherenkov detectors have a large background of ν_e interactions (they see only single electron events), and in the case of the second decay, the K^+ is below Cherenkov threshold, which forces strong cuts to be made that significantly reduce efficiency and introduce model-dependence. Thus, ICARUS will likely be competitive with water Cherenkov detectors such as Super-Kamiokande in the search for some nucleon decay modes even with a much smaller mass. The argument for solar neutrinos is less persuasive in light of recent SNO and KamLAND results, which makes further measurements of the ^8B solar neutrino flux redundant. Effort along these lines should be reconsidered.

ICARUS is one of three detectors planned for long-baseline neutrino beams (the others being MINOS and OPERA, with Super-K already operational). The importance of these measurements is described in the neutrino overview (see 5.1). The schedule calls for the full 3-kton detector to be operating in the CNGS beam by late 2007 or early 2008. This is an ambitious schedule but it is possible that it will be achieved. While the mixing measurements are redundant with other long-baseline experiments, the ν_τ capability is unique and should be pursued. The idea of reducing the beam energy to try and detect a non-zero θ_{13} would likely degrade this unique capability, and the sensitivity to θ_{13} does not seem competitive with other planned experiments.

The UCLA group has successfully built and installed the HV system for the T600 module and is now requesting funding to go ahead with the HV systems for the four additional 600-ton modules. SAGENAP supports the physics goals of ICARUS as outlined above and recommends that the small, but crucial, U.S. contribution be continued to see the project through. Currently there are uncertainties in the timescale for the deployment of the four modules at LNGS. Funding for the U.S. contribution should be closely coordinated with the actual detector construction and deployment schedule when definitely known.

5.2.3. KamLAND

KamLAND (Kamioka Liquid scintillator Anti-Neutrino Detector) has been a great success by any reckoning. Built in a remarkably short time by a Japan-US collaboration (in which both nuclear and particle physicists participate), it demonstrated reactor antineutrino oscillations over a mean baseline of order 180 km. In this L/E range, one is sensitive to the same values of Δm^2 that also lead to solar large mixing angle (LMA) oscillations.

KamLAND determines the value of Δm^2 more precisely than solar experiments can because oscillation maxima and minima can be measured over this baseline, rather than an average conversion probability. Solar experiments, on the other hand, give a more precise determination of θ_{12}. KamLAND continues to take reactor data and greatly improved precision on Δm^2 can be expected. The combination of KamLAND and solar experiments also provides constraints on θ_{13}. There is potential for about a factor of two improvement in $\sin^2 2\theta_{13}$ over the next few years from the KamLAND-solar sector.

KamLAND has strong U.S. participation, exemplified by the digitizing electronics provided by Lawrence Berkeley National Laboratory (LBNL). However, the US-supplied outer veto has experienced significant failures and may need to be refurbished or replaced. Live time has thus far been excellent, about 95%. A program of modest upgrades to the detector for the reactor measurements is being pursued.

KamLAND II is a future application of the same basic detector system, but focused on the study of solar neutrinos. Carrying out these studies will require a significant reduction in background rates over current levels, and the prospects for achieving these background levels remain uncertain. Notwithstanding the difficulties, the physics motivation of the search for ^{7}Be neutrinos is strong. The collaboration has demonstrated exceptional competence in the execution of KamLAND I, and is well poised to tackle these challenging technical problems. The KamLAND collaboration plans to request support from DoE next year to carry out the R&D needed to achieve the background levels required for KamLAND II.

In summary, SAGENAP congratulates the KamLAND collaboration for presenting what is arguably the most significant result in neutrino physics in the past year. Although challenges lie ahead in realizing the goals of KamLAND II, the collaboration is strong, and SAGENAP looks forward to hearing of progress towards meeting the requirements for KamLAND II once the R&D effort is fully underway.

5.2.4. LANNDD

The Liquid Argon Neutrino and Nucleon Decay Detector (LANNDD) is a concept for a large liquid argon drift chamber in the 100-kton range. Such a detector would have very good track and vertex resolution (few mm) compared to water

Cherenkov detectors (10's of cm). It could be realized by scaling up the design of the existing ICARUS 600-ton modules to longer drift distances and larger module volumes.

The potential capabilities of a liquid argon detector cover a wide range of physics applications. In long-baseline neutrino-oscillation experiments such as T2K and NOνA, a crucial limitation in searching for ν_e appearance is the capability to discriminate neutral-current originated π^0 initiated showers from those initiated by the physically interesting quasi-elastic ν_e interactions. Since Compton scattering lengths are typically 10's of cm in common materials, liquid argon would be able to cleanly separate vertices even at higher energies (where the π^0-decay gammas are close to collinear) and hence discern π^0 from anti-ν_e induced events. The potential gain in sensitivity over other technologies could be as much as an order of magnitude. Thus a LANNDD-type detector, if technically feasible, might be a very desirable alternative for a future generation "superbeam" or neutrino factory long-baseline experiment.

In addition, there are potential improvements in sensitivity in the search for proton decay. For some important modes (such as $p \rightarrow K^+\nu$), the daughter particles are below the water Cherenkov threshold and hence a traditional large-water detector has low sensitivity. In contrast, a liquid argon detector would observe these particles. Also the detection of ν_e's from galactic and relic supernovae, via the charged-current interaction with argon ($\nu_e + {}^{40}Ar \rightarrow {}^{40}K^* + e^-$), would be better in liquid argon.

SAGENAP believes that liquid argon detectors have an important role to play in future neutrino experiments and could make substantial contributions to the effectiveness of a neutrino factory and in the search for proton decay. Although the U.S. made significant contributions in the initial stages of the development of this technology in the 1980's, presently it makes only a secondary contribution as nearly all effort now takes place in Europe. The sub-panel recommends that the U.S. regain technical strength in liquid argon detector technology as potentially very important to the field of neutrino physics and underground science over the next few decades. The sub-panel supports the idea of an R&D effort to build a 5-m test chamber to investigate the technical feasibility of a large-volume liquid argon detector, but with the following reservations: (1) the safety of kiloton volumes of liquid argon in an underground chamber has not yet been established to levels required by WIPP or any other national laboratory, and (2) the current group proposing the prototype drift volume is far too small and over-committed to other projects to carry out such a major R&D effort.

We encourage the LANNDD group to participate in the formation of a dedicated research team of sufficient size and technical expertise to develop a proposal to undertake a major R&D effort in this important area.

5.2.5. Super-Kamiokande

Super Kamiokande (Super-K) is a very large underground water Cherenkov detector designed to study neutrinos from astrophysical and man-made sources and to search for rare processes such as nucleon decay. The experiment, located on the western side of the Japanese Alps, was built by a Japanese-U.S. collaboration. Operations started in 1996 with a central inner detector consisting of a 22.5-kton fiducial mass of purified water viewed by ~11,000 50-cm diameter photomultiplier tubes and an outer detector viewed by ~1,900 20-cm diameter photomultiplier tubes. The primary hardware responsibility for the U.S. group was the construction of the outer detector. The first phase of this experiment (SK-1) between 1996-2001 was extremely successful. Super-K found the first convincing evidence for neutrino oscillations using atmospheric neutrinos. Later observations of solar neutrinos by the Sudbury Neutrino Observatory (SNO), when combined with Super-K's solar neutrino data, showed that solar neutrinos also undergo flavor conversion. These observations are arguably the most important discoveries in particle physics in the past fifteen years. Super-K has also extended the limits on the proton lifetime well beyond those set by previous experiments. In 1999, the K2K experiment, using Super-K as the far detector of an accelerator neutrino beam, started. K2K has now found confirming evidence of muon neutrino oscillations using a man-made beam.

In November 2001, Super-K was seriously damaged in an accident caused by the implosion of a photomultiplier tube at the bottom of the detector tank. In the accident, approximately 55% of the photomultiplier tubes were destroyed. In response, the collaboration carried out a detailed and comprehensive study as to the cause of the accident and how to safeguard against the possibility of a similar occurrence in the future. A long-range plan was developed that provided very strong scientific motivation for rebuilding the detector. The operation of the second phase of Super-K (SK-2) began in 2003 using a fully restored outer detector and an inner detector with reduced photomultiplier tube coverage. Operation of the K2K beam has also resumed. The Super-K collaboration showed SAGENAP clear evidence that SK-2 could again detect solar neutrinos and again see the atmospheric neutrino deficit, albeit with slightly reduced sensitivity from the full, original detector. We congratulate the Super-K collaboration for the careful planning and hard work that permitted a rapid recovery from an unfortunate occurrence.

Looking towards the future, Super-K plans to refurbish the entire detector and to continue operations. The scientific motivations are both strong and broadly based. Super-K will serve as the far detector of the long-baseline T2K neutrino experiment from the JPARC accelerator, scheduled for first operation in 2008. In addition, for the next five years at least, Super-K will remain the world's largest underground detector. It will thus be at the forefront in the study of astrophysical neutrinos, including the watch for supernovae neutrinos, and it will extend the search for proton decay. Starting in 2005, the two-year refurbishment is largely devoted to restoring the full complement of inner detector photomultiplier tubes, but there is significant work to be carried out on a number of other detector components.

There are possible upgrades to Super-K that are currently in the R&D phase. One possibility is the addition of Gadolinium salt ($GdCl_3$) to greatly enhance the detector's sensitivity to antineutrinos from the diffuse supernova background. A second possibility involves the replacement of the electronics for the inner detector with a custom chip design that would be substantially cheaper and much longer lived than the current system.

It is anticipated that the U.S. groups on Super-K will propose to participate strongly in the refurbishment and upgrading of the detector and that they will continue to play a central role in the data analysis and scientific output of the experiment. Over the years, the U.S. has gotten a large amount of very exciting science from a relatively small investment. Given the excellent scientific motivation for continuing the Super-K program through the end of the decade and beyond, we expect that the U.S. participation in the Super-K and T2K programs would be at least maintained and very likely expanded.

5.2.6. θ_{13} Experiments

The physics motivation to measure θ_{13} is compelling. At present we know that two of the three mixing angles are nonzero, but we only have a limit on the value for θ_{13} (commonly expressed as $\sin^2 2\theta_{13} < 0.2$). A measurement of this angle will provide needed data for our understanding of the neutrino's place in the Standard Model of particle physics. If θ_{13} is exactly zero, it may imply some new symmetry. If it is not zero, it may provide a critical clue to the origin of the mixing matrix. Measurements of all three angles are required to perform the valuable tests on the unitary nature of the neutrino-mixing matrix.

Furthermore, a number of pressing questions regarding the characteristics of the neutrino can only be answered once we understand θ_{13}. If, and only if, θ_{13} is nonzero, the CP-violating phase δ in the lepton sector might be detectable. The fundamental question regarding the mass spectrum hierarchy (is it normal or inverted?) can be addressed by studying matter effects in oscillations but, again, only if θ_{13} is nonzero.

The current limit on θ_{13} is derived from the CHOOZ reactor experiment, atmospheric neutrinos, and the solar neutrino experiments. The actual value of the limit is somewhat dependent of the value of Δm^2 with the solar neutrino results placing the most restrictive limit in the critical $\Delta m^2_{Atm} < 2x10^{-3}$ eV^2 region. At an oscillation baseline corresponding to this Δm^2_{Atm}, θ_{13} can be observed by either studying subdominant effects in the disappearance of ν_e, or by studying the appearance of ν_e in a ν_μ beam. Presently operating experiments can only make marginal improvements on this limit. The operating solar neutrino experiments and the soon-to-operate MINOS detector have potential to reduce the allowed region by a factor of two or so in $\sin^2 2\theta_{13}$.

The sub-panel was given an informational briefing on a potential reactor program designed to measure θ_{13}. By placing a large anti-ν_e detector approximately 2 km from a nuclear reactor, one can exploit a strong neutrino source to study oscillation effects at a baseline defined by the known value of Δm^2_{Atm}. Because it looks at the disappearance of anti-ν_e, the oscillation effect is parameterized by only one unknown parameter, namely θ_{13}. Hence this technique measures θ_{13} in an unambiguous way. In reactor experiments, the neutrino source comes for "free" and doesn't require a new beam, and thus the experiments can be fielded somewhat quickly and economically. The sensitivity of this program is expected to be in the range of $\sin^2 2\theta_{13}$ ~0.01. Since the signal is a disappearance of anti-ν_e, the experiment would be limited by systematic errors.

At the present time, no reactor neutrino proposals exist. However, the various groups involved will most likely soon be asking for R&D money and developing proposals. Since there are a number of strengths to the technique, it would be useful to encourage support of this program. There are at least two groups pursuing a U.S.-led reactor θ_{13} experiment. The differences between the proposals are not dramatic. They differ primarily in the choice of site and the resulting depth available for the detectors. The U.S. program needs to coalesce into one effort. There are also U.S. groups involved in Double CHOOZ, a European-led effort, further diluting the focus.

The accelerator-based high energy physics program is not part of the SAGENAP purview and hence this complementary way to measure θ_{13} was presented only for context. This included a number of presentations on long-baseline experiments using accelerator-produced ν_μ beams including experiments in Japan and in the U.S. In these experiments, one searches for ν_e appearance in the beam, again at a baseline appropriate for Δm^2_{Atm}.

The strength of the long-baseline experiments is their sensitivity to a number of oscillation parameters. With this technique, one could hope to measure θ_{13}, and the CP-violating phase δ. For long-baseline experiments there is sensitivity to the mass hierarchy, which creates an ambiguity in extracting the three parameters, but, if one operates the beam in neutrino and anti-neutrino modes, one might also deduce the sign of Δm^2_{Atm} and reduce the ambiguity. In addition, an improved measurement of θ_{23} would be possible. This richness of this physics, however, can also be a complication because of the ambiguities in the interpretation of the all-important observation of ν_e appearance. Exploiting the results from a reactor experiment could ameliorate these uncertainties.

It is clear that the physics goals of the long-baseline accelerator program and the reactor program have a great deal of overlap. However, a reactor result on θ_{13} would be unambiguous and useful for the interpretations of the accelerator data. Hence it seems prudent to pursue a reactor experiment reasonably quickly. In addition, the long-baseline neutrino oscillation experiment, T2K, is being built in Japan and will likely be contemporaneous with the reactor experiments. A coordinated plan that incorporates the worldwide effort would be necessary. That is, some sort of roadmap is needed that coordinates the accelerator/non-accelerator approaches to this problem. Fortunately a committee has been formed by the APS (see 5.1) to do just this. It is difficult to see how one should proceed with a recommendation here without the APS study analysis. If a reactor proposal is brought before SAGENAP in the future, SAGENAP would consider the context of the APS study.

6. Other Experiments

SAGENAP heard presentations from two groups working on experiments designed to measure the electric dipole moment of the electron. These presentations were abbreviated and we are not in a position to do a careful review of each project. Indeed, the main issue associated with these experiments is to what degree they could be considered as part of particle physics and thus suitable for funding by the DOE HEP (High Energy Physics) and NSF EPP (Elementary Particle Physics) programs. Here we provide general findings associated with this issue.

6.1. Electric Dipole Moment (EDM) Experiment

The search for an electron Electric Dipole Moment (EDM) goes back over forty years. As in the case of the neutron, the existence of a large static electron EDM almost automatically implies new physics. Since the Standard Model prediction (based on the magnitude of CP violation in kaons) leads to values of roughly 10^{-57} C-m, a larger value could be an indicator of new CP-violating processes. An electron EDM would also imply that the new physics is associated with leptons and that it may arise from different sources than processes that might lead to a neutron EDM.

Searches for an electron EDM have typically been made using neutral atoms. The atomic EDM may be significantly enhanced by a non-zero electron EDM, especially for atoms with higher charge Z. Enhancement factors of 100-600 are typical. Most of these experiments use the splitting of atomic states via an external electric field (Stark Effect) and the photon transitions between them to search for an electron EDM. An electron EDM would change the magnitude of the Stark splitting. Since the EDM is T-odd, P-odd while the quadratic Stark Effect is T-even, P-even, a reversal of the electric field changes the sign of the EDM contribution to the energy. Since atoms moving in stray magnetic fields can have the same effect on the splitting as an EDM, work in this area has concentrated on cold atoms and field-free environments. The current best limit is less than 2.6×10^{-48} C-m, about nine orders of magnitude above the Standard Model prediction. New experiments hope to lower this limit by roughly two orders of magnitude using improvements in cold cesium fountains and by using the strong atomic electric fields of dipolar molecules.

Although electron EDM experiments have been used to some constrain some models with potentially new CP-violating processes, in many cases the electron EDM limit can constrain only a few of the many new parameters implied by the model. In the case of SUSY models, the constraints may also depend heavily on the squark (supersymmetric quark) mass or other unknown model-dependent quantities. We note that theoretical activity in the HEP community following the publication of the current best limit on the electron EDM has been steady but somewhat limited, with only roughly a dozen citations in refereed journals over the last two years related to HEP. In addition, we note that the techniques used to make the measurements and the extensive calculations required to deduce the atomic enhancement factors are those of atomic physics. While this does not mean that the results are not interesting to particle physics, it does mean that the particle physics community may not be the best judge of the experimental feasibility of these experiments or the reliability of the calculations. SAGENAP believes that these experiments would be best done in the atomic physics community, where there is also great interest in fundamental symmetries of nature (e.g. parity violation).

APPENDIX A

Charge to SAGENAP

(following two pages)

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

APR 12 2004

Professor Frederick Gilman
Carnegie Mellon University
5000 Forbes Avenue
Pittsburgh, Pennsylvania 15213

Dear Professor Gilman:

This letter is to request that the High Energy Physics Advisory Panel (HEPAP) establish a subpanel to assess projects in experimental non-accelerator physics that may be of interest to both the National Science Foundation (NSF) and the Department of Energy (DOE). This subpanel will be referred to as the Scientific Assessment Group for Experimental Non-Accelerator Physics (SAGENAP).

BACKGROUND

There is a need to convene a subpanel of the High Energy Physics Advisory Panel (HEPAP) to assess projects in experimental non-accelerator physics that may be of interest to both NSF and DOE – SAGENAP.

SAGENAP's role is to provide one view of which projects are worthy of further, in depth, consideration for funding by the agencies. SAGENAP's primary consideration is the scientific merit of the project.

An assessment by SAGENAP is neither a necessary nor a sufficient step for obtaining funding of a project and endorsement by SAGENAP does not automatically result in endorsement of the project by the funding agencies.

SAGENAP will access projects in three categories: 1. Projects in the conceptual phase; 2. Projects that are ready to request agency funding for concept studies, design and development, or construction; and 3. Ongoing projects funded by the above named agencies.

ORGANIZATION

For the April 2004 meeting, SAGENAP will be constituted as a subpanel of HEPAP, to which it will report.

The agencies jointly select the members of SAGENAP, and will ensure that the membership includes members of HEPAP.

The agencies jointly select the Chair (a.k.a. Coordinator).

CHARGE

SAGENAP will assess projects that may be of interest to both DOE and NSF. The projects to be assessed by SAGENAP will be specified by the agencies. For projects in categories, 1 and 2, SAGENAP will:

- Assess the scientific merits of the project.
- Assess the readiness of the project to request funding for concept studies, design studies, or construction.
- Assess the scientific and technical goals of the project in the context of related activities in the field.
- Assess the scientific, technical, organizational, and management capabilities of the project team.

For projects in category 3, SAGENAP will:

- Assess progress and any scientific issues on the ongoing project and identify any areas of concern for agency attention.

<u>SAGENAP will also identify activities that may lead to construction projects in the future, and assess their priorities, readiness, approximate timescales and costs.</u>

REPORTING

The Chair (Coordinator) will prepare a report following the meeting. The report should provide a balanced summary of the assessments of the SAGENAP members for each project on the agenda.

The report will be presented to HEPAP at the first HEPAP meeting following completion of the report.

We wish you well in this important exercise.

Sincerely,

Raymond L. Orbach
Director, Office of Science
U.S. Department of Energy

Michael S. Turner
Assistant Director for Mathematical and Physical Science
National Science Foundation

cc: R.Staffin, SC-20
B.Strauss, SC-20
R.Imlay, SC-20
M.Marsden, SC-20

J.Dehmer, NSF
J.Lightbody, NSF
M.Goldberg, NSF
E.Loh, NSF

APPENDIX B
SAGENAP 2004 Membership

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APPENDIX C

SAGENAP Agenda

April 14-16, 2004
Arlington Hilton Hotel
Arlington, VA 22203

April 14, 2004

08:30	Welcome	J. Dehmer, R. Ong
08:45	LSST Science, Proof of Concept	T. Tyson
10:00	Break	
10:15	LSST Project Organization, Management and Telescope	D. Sweeney
10:45	LSST Data Management	C. Stubbs
11:15	LSST Focal Plane Array and Camera	S. Kahn
11:45	Questions	
12:15	Lunch	
13:40	SNAP	S. Perlmutter
14:20	Destiny	J. Morse, T. Lauer
15:00	Dark Energy Survey	J. Mohr, B. Flaugher
15:30	Break	
16:30	Questions to LSST	
16:45	Adjournment	

April 15, 2004

08:15	LSST Questions and Answers	LSST Group
09:00	STACEE	K. Ragan
09:30	VERITAS	T. Weekes
10:00	Milagro	J. Goodman
10:20	Break	
10:50	HAWC	G. Sinnis
11:20	Auger S	P. Mantsch, J. Cronin
11:50	Auger N	K. Arisaka
12:20	Lunch	
13:40	HiRes	G. Thomson
14:10	Telescope Array/TALE	P. Sokolsky
14:40	DRIFT	D. Snowden-Ifft, J. Martoff
15:10	Break	

15:25	CDMS II	B. Cabrera, B. Sadoulet
16:05	XENON	E. Aprile, R. Gaitskell
16:45	ZEPLIN II	H. Wang
17:30	Adjournment	

April 16, 2004

08:50	Double-Beta Decay Overview	S. Elliott
09:20	EXO	G. Gratta, M. Breidenbach
10:00	KamLAND	K. Luk, Y. Kamyshkov
10:40	Break	
10:55	Super-Kamiokande	E. Kearns
11:15	Off-Axis Neutrino Physics Overview	H. Sobel
11:35	T2K: Beamline, Near Detector Overview	C. Jung
11:45	T2K: 2KM Detector Overview	C. Walter
11:55	Theta(13) Neutrino Physics and APS Study	E. Blucher
12:15	Lunch	
13:15	Theta(13) Neutrino Experiments	M. Shaevitz
13:35	Discussion	S. Freedman
14:05	EDM I	H. Gould
14:15	EDM II	N. Shafer-Ray
14:25	ICARUS	D. Cline, F. Sergiampietri
14:45	LANNDD	D. Cline, F. Sergiampietri
15:10	ASHRA	S. Dye
15:20	Polar Bear	A. Lee
16:00	QUIET	B. Winstein
16:10	Closing	
16:20	Adjournment	

APPENDIX D

Project and Experiment Acronyms

2KM – 2 kilometer neutrino detector for the T2K experiment

AGASA – Akeno Giant Air Shower Array

AMANDA – Antarctic Muon And Neutrino Detector Array

ANITA – ANtarctic Impulse Transient Antenna

ASHRA – All-sky Survey High-Resolution Air-shower detector

BOOMERANG – Balloon Observations of Millimetric Extragalactic Radiation and Geophysics

CDMS – Cryogenic Dark Matter Search

CHOOZ – CH $2_{13}2_{13}$ Z

CNGS – C ERN Neutrinos to Gran Sasso

COBE – COsmic Background Explorer

CRESST – Cryogenic Rare Event Search with Superconducting Thermometers

DAMA – DArk MAtter

DASI – Degree Angular Scale Interferometer

DES – Dark Energy Survey telescope

DESTiny – Dark Energy Space Telescope

DRIFT – Directional Recoil Identification From Tracks

EDELWEISS – Experience pour DEtector Les WIMPS en site Souterain

EDM – Electric Dipole Moment

EGRET – Energetic Gamma Ray Experiment Telescope

EXO – Enriched Xenon Observatory

GENIUS – GErmanium in liquid NItrogen Underground Setup

GLAST – Gamma ray Large Area Space Telescope

HAWC – High Altitude Water Cherenkov

HEGRA – High Energy Gamma Ray Astronomy

HESS – High Energy Spectroscopic System

HiRes – Fly's Eye High Resolution detector

ICARUS – Imaging Cosmic And Rare Underground Signals

IceCube – neutrino observatory in the ice at the South Pole

JDEM – DOE and NASA Joint Dark Energy Mission

K2K – KEK laboratory to Kamioka experiment

KamLAND – Kamioka Liquid scintillator Anti-Neutrino Detector

LANNDD - Liquid Argon Neutrino and Nucleon Decay Detector

LSND – Liquid Scintillator Neutrino Detector

LSST – Large area Synoptic Survey Telescope

MAXIMA – Millimeter Anisotropy eXperiment IMaging Array

MINOS – Main Injector Neutrino Oscillation Search

NuMI – Neutrinos at the Main Injector

OPERA – Oscillation Project with Emulsion-tRacking Apparatus

Palo Verde – long-baseline neutrino oscillation experiment using anti-neutrinos from the Palo Verde Nuclear Generating Station
PanSTARRS – Panoramic Survey Telescope & Rapid Response System
QUIET – Q/U Imaging ExperimenT
SDSS – Sloan Digital Sky Survey
SNAP – SuperNova/Acceleration Probe
SNO – Sudbury Neutrino Observator
SPT – South Pole Telescope
STACEE - Solar Tower Atmospheric Cherenkov Effect Experiment
Super-K – Super Kamiokande experiment
T2K – TTokai To Kamioka experiment
T600 – 600 ton module for the ICARUS experiment
TA – Telescope Array
TALE – Telescope Array Low-energy Extension
VERITAS – Very Energetic Radiation Imaging Telescope Array System
Whipple – Whipple Observatory gamma-ray telescope
WMAP – Wilkinson Microwave Anisotropy Probe
XENON – Xenon experiment
ZEPLIN – ZonEd Proportional scintillation in LIquid Noble