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Report of Scientific Assessment Group on Experimental Non-Accelerator Physics (SAGENAP)

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James Stone, DoE, Co-Chair
Eugene Loh, NSF, Co-Chair
Robert Lanou, Report Coordinator

SAGENAP Meeting
Summary Report
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The Scientific Assessment Group on Experimental Non-Accelerator Physics (SAGENAP) for this meeting was composed of Thomas Cline (NASA-Goddard), Janet Conrad (Columbia), Robert Lanou (Brown), James Musser (Indiana), Rene Ong (Chicago), Joel Primack (UC Santa Cruz), Steven Ritz (NASA-Goddard), Hamish Robertson (U. of Washington), Leslie Rosenberg (MIT), Bernard Sadoulet (UC Berkeley), Henry Sobel (UC Irvine), James Stone (DoE), P.K. Williams (DoE), Eugene Loh (NSF) and Marvin Goldberg (NSF). The meeting was co-chaired by James Stone and Eugene Loh with Robert Lanou serving as report coordinator; it was held during March 29-31, 2000 in Gaithersburg, MD (Holiday Inn) with DoE as the host agency. All members were present.

Six proposals were under consideration at this meeting. They were SNAP, ICECUBE, Milagro, Axion, EXO and Zeplin-II. In advance of the meeting written versions of the proposals were available to each member of SAGENAP. Additional material was provided at the meeting in some cases. Oral presentations were made by the proponents during the meeting days (see Appendix) followed by discussion with the group members. Subsequent to discussions by SAGENAP in executive session, each group was given further questions to which they responded in open session. Individual written reviews on the proposals by each SAGENAP member were provided to the DoE and NSF. Each one of the proposals was reviewed by at least five SAGENAP members.
As in the past, this SAGENAP report summarizes the meeting and reviews of each proposal, highlighting the areas of agreement and concerns, and some individual comments for each proposal. The report presents a balanced summary of the conclusions of the SAGENAP membership. These summaries for each proposal are contained in the body of the report.

Following recommendations made in a previous SAGENAP report there were also scheduled, in addition to the proposals being reviewed, presentations of an informative and general nature directly related to the directions, potential and priorities in the field. This has been a very valuable resource providing a common and broadly based, informed input to the group and the agencies from the several areas of the field. At this meeting were heard progress reports on three recently constructed and successfully operating experiments. They were on CDMS by Bernard Sadoulet, SNO by Hamish Robertson and the Sloan Digital Sky Survey by John Peoples. Additionally, presentations were heard on the work of two groups providing initiatives toward furthering the development of the field. Rocky Kolb reported on the activities of the "Connections" group. The group is composed of particle/nuclear physicists, astrophysicists and astronomers from universities and national laboratories whose research is supported variously by DoE, NSF and NASA. Their goals, as the name suggests, include urging that advantage be taken of the unity and complementarity of the science and the technologies (accelerator and non-accelerator), as well as the capabilities of agencies supporting these areas in order to enable new scientific initiatives perhaps not possible without this broader cooperation. A second significant component addresses issues of science education at the interface of high energy physics, astrophysics and astronomy. Additional details of their activities were provided from http://www.quarkstothecosmos.org.

The presentation on the second initiative was given by Michael Turner who is Chair of the study committee recently formed by the National Research Council of the National Academy of Sciences. Its formal title is the Committee on the Physics of the Universe, and it is broadly charged to carry out an assessment of an area of science at the intersection of physics and astronomy. Among the specific goals will be the identification of new science opportunities, objectives, and key questions, as well as to set priorities for the development of this area and to address mechanisms for evaluating future opportunities and cooperation across agencies. Its work is expected to take two years and will evolve through a series of presentations and public forums in cooperation with professional societies (APS and AAS) and the funding agencies (DoE, NASA and NSF).

These additional presentations, when taken together with the experimental proposals before SAGENAP at this meeting, served ideally to emphasize the variety and unity of the fundamental science being done as well as the tasks ahead to insure that it is done well. The members of SAGENAP expressed their interest and support of these efforts and thank the presenters for providing them with valuable background.

SNAP

The Supernova / Acceleration Probe (SNAP) proposal requests DOE/NSF funding for an ambitious satellite experiment to discover and measure the properties of over 2000 Type Ia supernovae (SN Ia) per year at redshifts $0.1 < z < 1.7$. The main goal of SNAP is to try to determine the nature of the "dark energy" that is apparently causing the expansion of the universe to accelerate. The satellite would have a ~ 2 m telescope with an optical imager having a very large field of view of 1 square degree, an infrared imager with a small field of view, and a three-channel spectrograph covering wavelengths from near-IR to near-UV. The present request is for $17.4 million of DOE/NSF funding for conceptual design and R&D on the SNAP project. No detailed cost estimate for the entire project was given but may be expected to be in excess of $200M. The PI of the SNAP proposal is Saul Perlmutter with Michael Levi as Co-PI. The group is centered at LBNL and the University of California (Berkeley) with collaborators from several other US and European institutions.

The Supernova Cosmology Project (SCP) led by Dr. Saul Perlmutter of LBNL initiated the program of measuring cosmological parameters by discovering many high-redshift SN Ia and determining their maximum luminosities as a function of redshift. Under the assumption that the (corrected) maximum luminosities of type Ia supernovae are independent of redshift, these data allow measurement of the luminosity distance as a function of redshift, a classic cosmological test. The SCP results imply that the expansion of the universe is accelerating, rather than decelerating as expected due to gravity. A competing group, the High-Z Supernova Search project, has reached very similar conclusions on the basis of independent analysis of a largely different set of high-redshift SN Ia. The goals of the SNAP proposal are

- to control the systematic uncertainties in the SN Ia measurements of cosmological parameters so that they are comparable to the final photon counting statistical uncertainties (each to be less than about 0.02 magnitudes), and

- to obtain data on many additional SN Ia in order to study the nature of the "dark energy" by measuring w(z), the ratio of the dark energy's pressure to its density, as a function of redshift.

The dark energy accounts for most of the energy density of the universe, and explaining it may lead to progress in fundamental physics. Measuring w can certainly help characterize it. For example, a cosmological constant implies that w = -1, while alternative physical models for the origin of the dark energy such as a rolling scalar field ("quintessence") predict w $\geq$ -0.8, possibly varying with redshift.

The characteristics of the proposed SNAP satellite were chosen to enable both goals. The large mirror and large optical field of view are intended to permit discovery, within two restframe days of the explosion, of approximately 2000 SN Ia per year at redshifts greater than 0.3 and less than 1.7. The optical design is also intended to provide for photometric follow-up and the multiband spectroscopy would permit determination of the redshift of each supernova, confirmation that it is of type Ia, and the control of various systematics. The proposed satellite includes several novel features: new high-resistivity CCDs, the largest CCD mosaic ever used for astronomy (~ 1 Gigapixels), "integral field units" in each of the three spectrographic channels to obtain an effective 2 square arcsecond field, and possibly the use of an actively controlled secondary mirror to stabilize the image. Part of the requested funding is for R&D on these novel technologies, which will in turn improve the technical basis for the spacecraft design.

The proposed SNAP satellite will have many capabilities that go beyond those of any other current or proposed astronomical observatory. For example, the repeated imaging of approximately 20 square degrees will produce data comparable to the "Hubble Deep Field" but covering several thousand times the area and going two magnitudes deeper. The SNAP team proposes to exploit these data for alternative measurements of cosmological parameters via the technique of weak gravitational lensing. The SNAP team also plans to have Science Working Groups for each science topic, and to invite Guest Investigator and Guest Survey proposals for topics that can be addressed by SNAP both during the three years of the SN Ia survey, and during the several years of useful lifetime anticipated afterward.

The panel members agreed that the physics addressed by the proposal is extremely important. . The evidence for a cosmological constant Ω_Λ (or, more generally, dark energy) is strong, and comes from two independent directions:

(1) CMB anisotropies implying that ($\Omega_m + \Omega_\Lambda$) = 1 ± 0.12 at 95%CL plus combined galaxy cluster measurements implying that $\Omega_m \approx 0.4 \pm 0.1$, which together argue that $\Omega_\Lambda \approx 0.6 \pm 0.2$; and

(2) ($\Omega_\Lambda - \Omega_m$) $\approx$ 0.4 from high-redshift supernovae.

The dark energy thus represents most of the energy density in the universe, but the nature of this dark energy is entirely unknown.

One SAGENAP member states: *"Clarifying it is surely one of the most important challenges that observational cosmology presents to fundamental physics, one which is likely to have repercussions in many other areas of physics and astronomy."*

Another member says: *" The accelerating universe is a fundamental mystery in cosmology and particle physics and experiments to follow up on the initial exciting observations must be done."*

The main thrust of the SNAP proposal is to control the systematics of SN Ia by direct observational comparison of many high redshift and low redshift supernovae, and to use observations of several thousand SN Ia at redshifts z = 0.3 - 1.2 (with a few at higher and lower redshifts) to measure various cosmological parameters. An important goal of SNAP is to measure the equation of state parameter w(z) which can certainly help to clarify the nature of the dark energy. The most serious weakness of the present type 1a SN results is their sensitivity to systematic errors such as evolutionary or environmental effects. The proposing group, in their oral presentations as well as in the proposal itself, presented the case for why this program requires: 1) a satellite in a high orbit, 2) a large-field camera, and 3) spectrographic capabilities from near-UV through near-IR. While the panel members were not observational astronomers, they were largely persuaded that this approach might well be superior to a combined ground- plus satellite-based approach and a range of opinions was expressed.

In support of this approach one panel member suggests: *"They argue persuasively that these effects are being addressed ….. [and that] ….. the large data set available through the SNAP measurements will allow subsets of the data with similar properties to be analyzed, and to be checked against each other for evidence of systematic effects due to environmental differences. It is quite clear that the proposers are aware of the critical importance of reducing systematic errors in the proposed experiment, and have to a large extent based the design of the instrument around addressing this issue."*

In the same vein another says: *" The proposal argues successfully that the various components that have been included in the design are necessary to reduce the systematic errors and extract the physics."*

Suggesting that the approach may not be unique, another panel member observes: *"The technical issues are certainly well addressed, although I have lingering concerns about the systematics: the issues of evolution, absorption, and absolute calibration."*

However the panel was concerned, given the design data in the proposal, whether or not the stated signal to noise level for higher red shift spectroscopy could be achieved within the observing strategy proposed. Subsequent to further questions from the panel, the proposers acknowledged that this aspect of the proposal as well as the projected performance of other major detector systems would be thoroughly re-investigated during Phase I.

As was emphasized by the SNAP proposers and discussed by several panel members in their reports, the SN Ia experiments, and this one in particular, are complementary to other approaches in experimental cosmology and can be used together to break degeneracies. This is a key factor in believing that next generation experiments are fully warranted and will lead to new understanding. Among these other approaches are experiments on the CMB and large scale structure such as MAP and SDSS, respectively. Some panel members expressed concern that given the promise of further rapid developments in the field before SNAP could be launched, new results ….. *"might lead to a better understanding of evolutionary effects, which may impact the case for SNAP"*.

Still another panelist says: *" But overall, the group did make a strong case that, given our current understanding of SN Ia's and cosmology, an experiment like SNAP would be a powerful instrument and would make perhaps the definitive measurements of the equation of state of the Universe. But, the group did not adequately address what we will likely learn from other techniques over the next 5-6 years before SNAP flies (and when SNAP is up in orbit)."*

It was generally agreed that the proposed satellite could generate a great deal of data that would be of very wide interest to the astronomical community beyond the SN Ia effort. The SNAP group has proposed Science Working Groups and for making arrangements to involve Guest Scientists. SAGENAP believes it is very important to do so. This will generate wider support for the launch should the experiment go forward, assuring prompt and appropriate analysis of the potentially rich pool of data beyond that of the SN Ia goals. Some SAGENAP members went further in their suggestions for further inclusion of astronomers.

One member observes: *"The size of the group is sufficient to carry out the phase I program being proposed at this time, but will need to grow in order to carry out the actual experiment."*

A second member observes*: " The SNAP group is strong in some areas (science especially), but could use help from the space astrophysics community. The group should be more inclusive of the overall experimental cosmology/astrophysics community. "*

And a third panelist says: *" At the scientific level, I argued above that the potential impact of SNAP is much wider than the supernovae, and in particular the project is very complementary to NGST. It is therefore critical to enlist from the community the full complement of scientific expertise necessary for the success of the project."*

The request for $17.4M for R&D evoked from the panel universal support that a substantial R&D effort is justified. However, SAGENAP felt that not enough information was provided as to its projected goals, scope and management. Consequently, a range of general assessments were expressed by panel members as to how to proceed to an effective R&D program.

One member states: *" The next step in this program is to do a thorough research, development, and design study, and I fully concur with the request from this group to DOE and NSF to support this study. Since the funds requested are large and the justificiation for the budget unclear, a cost and management review is appropriate."*

Quoting a second member*: " One can conclude only by endorsing a support study to continue with experiment definition and to investigate areas of possible optimization and/or descoping, while simultaneously exploring the NASA cooperation time table*."

Another says: *"It is premature at this time to approve the SNAP experiment in toto. However, the Phase I study being proposed should be given the highest possible priority."* This member also continues: *"The group did not present or make available a detailed break-down of the Phase I budget, and it is therefore difficult to assess the reasonableness of its size, other than to say that is about the right size relative to expected overall project costs. One aspect of the phase I study (the development of a commercial supplier for high QE unthinned CCD's) promises to have an enormous impact on observational astronomy as a whole, and should be supported independent of the SNAP project."*

A fourth person observes: *" In general, I am highly supportive of a study for a space-based mission. Perhaps due to my slight unease with the present ground-based results, I favor a more conservative next experiment, perhaps largely ground-based, and would like to see how this would fare as a real option. However, I think studies for SNAP should continue."*

Another member states*: " As a request for R&D, this passes an important requirement: the R&D will be widely useful to other experiments, even in other disciplines. This makes the case for supporting the requested R&D attractive. However, one should realize that the request is quite large. For comparison, just the intial R&D*

funding is on the same scale as the total estimated cost for Veritas presented at SAGENAP 1999. I believe that we should not also proceed with a strong push for the full R&D funding request. Instead, I propose that we recommend a thorough technical review of the R&D plan."

And from a sixth panelist: *" A final technical worry is that the full time equivalent percentage of the participants quoted at the SAGENAP meeting is totally unrealistic for a large number of them. A project of the magnitude of SNAP will need that a significant number of scientists sacrifice their short term scientific output to speed up the definition of the mission and latter the construction of the satellite. This aspect will need to be addressed in the cost and management review of Phase I ."*

Several panelists noted it would be appropriate that, along with experimental R&D, related theoretical work such as modeling supernovae could be included.

Both the scale of the complete SNAP project and the need to bring to it technologies and experience from the several fields of particle physics, astronomy and space science for its success argue forcefully that it be a multi-agency project. The role for the expertise traditionally supported by DoE and NSF was made clear in the proposal and in the presentations; however, no definite plan or probable timetable for launch was presented. SAGENAP members acknowledged that the group is still in the early stages of inter-agency negotiations and are appreciative of its "path finding" role; nonetheless, many members expressed concern that it is imperative to have a clearer picture soon of launching opportunities or constraints since long delays might require significant changes in the experiments.

One SAGENAP member says: *"The NASA connection, while crucial, has not yet been made. I believe that discussions with NASA have to indicate a willingness to cooperate on this experiment and its timetable before any funding is approved."*

A second member adds: *"I don't see this as a particularly serious concern at this time, as the group is working hard to understand it themselves, and as I am confident that the agencies will act as facilitators. However, this matter must be resolved prior to approval of a Phase II effort."*

In summary, the SAGENAP discussions indicate enthusiastic agreement by the panel that the science goals are on questions of great importance to physics and cosmology. Further, it was considered that at the present stage in the measurement of the cosmological parameters, new experimentation is fully warranted and that the SN Ia technique will continue to play a crucial part. The quality of the document presented was felt to be impressive, particularly for a project in its early stages. The panel members were favorably impressed with the proposers' consideration of the sources of systematic error and were largely convinced that a fully satellite-based experiment is likely to be the preferred approach. The panel noted that the requirements for spectroscopy in space are stringent and the demands on CCD performance and utilization are severe. Consequently, a thorough investigation of the technical risks of this and all other detector systems should be re-evaluated in Phase I. The excellence of the abilities of the present SNAP group and the outstanding work done so far was cited; however, it was strongly felt that further augmentation in two collaboration areas, with astronomy and with NASA personnel, could benefit the future of the project. There was unanimity on SAGENAP that a substantial R&D program is required soon to insure a successful SNAP experiment. There was also agreement that the entire project should not be fully endorsed until a more complete R&D program and its management is presented and reviewed by an appropriate technical group. It was also widely supported that, if the DoE and NSF decide to conduct such a review, SAGENAP suggests that interim funds be provided to speed the preparations for a review and to enable the forward movement of this important experiment. Such movement should also include efforts to clarify and facilitate the opportunities for launch of the satellite.

IceCube

The IceCube proposal calls for construction of the largest neutrino detector yet attempted, using 10^9 tons of natural Antarctic ice. The detector consists of 80 strings of 4800 optical modules placed deep in the ice, and reconstruction of upward-going muon tracks is accomplished by timing and charge measurements. The physics goals of Ice Cube are many. Perhaps of the greatest importance is that it would open a new window in the astrophysical spectrum, using ultra-high energy neutrinos, where essentially no data exist. History has shown that new windows of this sort almost invariably produce unanticipated discoveries.

In addition to the possible discovery of new physics and new phenomena, there are important known physics questions that IceCube can address. It can contribute to efforts aimed at distinguishing between a hadronic and an electromagnetic origin for TeV gammas from AGN's such as Mrk 421 and 501. If the primary source is hadronic (e.g. decays of pi-zero's), then one would expect neutrino fluxes from charged pi decays as well. Little is known at present about Gamma Ray Bursts (GRB) other than that they are at cosmological distances for the most part and are sources of immense power. IceCube could provide data on possible neutrino fluxes from GRBs.

Making measurements of diffuse neutrino fluxes from AGN's and testing for a direct signal from GRB's are among ICECUBE's greatest strengths; however, it may have sensitivity to address more speculative topics. These include sensitivity to TeV cold-dark-matter candidates via their annihilation to neutrinos. Also IceCube can provide a measure of astrophysical fluxes of tau neutrinos if sufficient fluxes of these neutrinos exist. Energetic tau neutrinos produce a distinctive "double-bang" signature as two decay muons pass through the detector. If both photons and neutrinos from distant sources can be detected in terrestrial observatories, it becomes possible (with certain assumptions) to carry out neutrino oscillation searches over Mpc scales, with sensitivities down to 10^{-17} eV^2, and tests of the weak equivalence principle for neutrinos.

The IceCube proposal rests on a solid foundation of proven technology, the existing AMANDA detector. A fortunate additional factor is the upgrade of South Pole Station beginning shortly. The size and remoteness of the detector lead to a total cost of $140.3M (for design, test, operation and analysis) spread over 5 years. The spokesperson for the proposal is Francis Halzen (Wisconsin) with Steven Barwick (UC-Irvine) as co-spokesperson; the collaboration is composed of scientists from a large number of US and European institutions. Significant other foreign interest and support are likely. With the knowledge gained from AMANDA, construction can begin almost "immediately", i.e. in 3 years given the time it will take to secure NSF MRE funding and to commission the necessary equipment at the pole.

SAGENAP members were uniform in their praise of the pioneering work of the group in conceiving, constructing and operating the AMANDA series of detectors in the Antarctic polar ice. In so doing the group has demonstrated that atmospheric neutrinos coming upward through the Earth can be detected above background and that the products of their interactions can be tracked via Cherenkov radiation in the ice. This result is one essential piece in demonstrating that the technique can be expanded to a size large enough to see astrophysical UHE neutrino sources.

Exactly how large such a detector should be is not possible to specify with precision presently since models for the neutrino fluxes are speculative and no high-energy neutrinos from astrophysical sources have yet been detected, although the AMANDA-series themselves have provided the best limits. Models aside, the detection of VHE gamma-rays from astrophysical sources and the existence of UHE cosmic rays are seen by many to be sufficient argument that a search for their neutrino counter-parts is important to carry out; either to discover sources, set limits or constrain models.

The IceCube group, in presenting their case for the expansion of the installation to 1 km^3, argued strongly and cogently for the discovery potential and appropriateness of their technique. They further emphasized that there will be no genuine neutrino astronomy until a detector on at least this scale is constructed. SAGENAP was in agreement that, based on the technical evidence from the AMANDA-series so far, they have shown that a detector that size could be constructed successfully in the Antarctic ice. As with any large-scale exploratory project, there is both the potential for discovery or for significant flux limits. A number of questions naturally arise as to the most effective strategy to proceed. Among the questions discussed for this proposal by SAGENAP are: Is there sufficient scientific motivation for a detector of any size? If there is, what would appear to be a reasonable size on the basis of scientific cost-benefit? Has it been convincingly shown that the technique is ready to extract the physics from a 1 km^3 detector if it were built as proposed? Are there alternative, competitive experiments? Can the continued operation of AMANDA-II provide essential information which will affect the nature of IceCube? Is this group of adequate strength to carry out both the arduous construction and the operation of AMANDA-II? Is the appropriate management structure in place?

On the questions of scientific motivation the majority of SAGENAP members felt that there was sufficient scientific motivation but they often diverged on specifics for the size of a detector and the appropriate time scale for reaching that size. A sampling of members' comments on the first of these two related issues gives some of the flavor of concerns.

One member states: *"IceCube promises to be the first experiment with sufficient sensitivity to observe neutrino sources outside our solar system, and is a reasonable next step towards the goal of adding neutrino astronomy to the list of observational windows on energetic astrophysical sources. It is clear, however, that the models on which neutrino source intensities are based are highly speculative, and a null result (at least in terms of neutrino point sources) is not completely out of the question."*

A second panelist says: " *I find the physics motivation for IceCube compelling. This experiment addresses a broad range of interesting neutrino physics topics. I am particularly interested in possible point sources for very high energy neutrinos. However, observation of diffuse high energy neutrinos would also be quite interesting. A null result would be hard to accommodate within models -- so even that would be important. From this point of view, this is a 'can't lose' experiment."*

Another panelist observes: *" The science is exploratory, but important, and it should be pursued. It is difficult to come up with good estimates for astrophysical neutrino fluxes, but this problem should not deter or prevent the construction of major high-energy neutrino telescopes."*

A more skeptical opinion was also expressed: *" The problem with approving the IceCube proposal now is that AMANDA has not yet given a clear indication that a kilometer cubed detector will indeed produce important new data. We have neither* *experimental nor theoretical reasons to believe it is likely that such a detector will discover high energy neutrinos from identifiable astrophysical sources."* However, this same reviewer adds: *"It is true that we usually find interesting new phenomena when we open new observational windows. Even discovering a background of high energy neutrinos or setting upper limits on such a background would be useful. But if these were the expected scientific outcomes, it might be more appropriate to expand AMANDA more slowly, rather than commit now to spending $150 million* [sic] *on a dramatic expansion."*

A fifth SAGENAP member says: *"It [AMANDA B-10] has reached a rejection of down going muons commensurate with the needs of IceCube. In doing so, the collaboration has been able to place an interesting preliminary limit on the diffuse flux of high energy neutrinos. This important physics result excludes most of the first generation models of hadron acceleration in active galactic nuclei. This impressive process clears the way for an extrapolation beyond AMANDA II, which has just been deployed."* This person also adds, however: *" Gamma Ray Bursts represent, at the moment, the best chance for an observational proof that this approach is worth the large investment. The team may be lucky with Amanda II: in that case a lot of the resistances may disappear."*

Also expressed by another panelist was: *" The AMANDA group has already ruled out the more optimistic estimates for AGN neutrinos based on the extreme assumptions by Stecker et al. It certainly may be possible to see a fair number (i.e. hundreds) of neutrinos from a diffuse collection of AGN. To this reviewer, such an observation would be a significant achievement. One can not think of this as real astronomy, but the detection of a diffuse neutrino source would clearly motivate the continuation of the program to even larger scales. A similar situation exists for GRB: the flux uncertainties are large, but seeing even just a few neutrinos from several GRB would really impact on our understanding of high energy astrophysics."*

Concerns about what would be an appropriate size to aim for and on what time scale to achieve it were extensively discussed by the panel. Several issues merged in the discussion on those two points: While the Antarctic Polar Station facilities are under rehabilitation by the NSF there will be no new detector installations for ~2 years. The AMANDA-II detector has just been completed and is in operation; it is significantly larger than the earlier versions (-B4 and -B10) and is $\sim 1/30$ the size of IceCube. It is expected to continue operating in the austral winters until the end of the 5-year period when IceCube is constructed during the austral summers. Because installations can only occur during austral summers, there is a natural set of breaks in the time frame for completion of IceCube. The data presented to SAGENAP for which reconstructed upward going neutrino events were analyzed was taken with the more restricted B-series configuration. In these data, reconstruction efficiencies of ~ 10% were shown and confined largely to tracks approximately aligned with the strings of optical modules as might be expected from the aspect ratio of the B-series. In IceCube it will be essential to have good efficiency for more nearly horizontal tracks for background rejection. SAGENAP members were impressed with the analysis carried out so far but some reservations were expressed to the effect that the B-series data may be too limited in scope to give assurance for the expected performance of a full-scale IceCube.

The IceCube group has made valuable studies of a digital electronics system as a candidate for replacement of the current analogue system for the optical modules (OM). They had not yet made their final choice for subsequent installations. In response to direct questions about the possibilities for a different or reduced or more evolutionary scale to the installation the IceCube group expressed strong preferences not to do so and argued in favor of the most rapid construction to completion which the seasons and funds would allow. Among their concerns was the threat of competition particularly from the ANTARES detector under construction in the Mediterranean Sea by European groups.

The general sense of the SAGENAP members' view on these issues of scale and timing is that the most rapid construction schedule is not in the best interest of achieving the largest and most effective detector for VHE/UHE astrophysical neutrinos. Rather, that a more modest rate in the initial installation period or two is to be preferred; this will fit better with the new knowledge and experience which will be gained from analysis of the recently inaugurated AMANDA-II. SAGENAP feels that the rapid analysis of AMANDA-II data will be very important. Information gained from AMANDA-II may imply desirable changes in the final configuration. The present size and past experience of the group in trying to maintain the arduous construction simultaneously with data analysis suggests that a slower initial installation schedule will benefit both efforts. Some quotations from a few SAGENAP members' reports illustrate the range of concerns bearing on this conclusion:

One member states: *"The Amanda collaboration has done a wonderful job in demonstrating that the physical construction of a neutrino telescope is possible at the South Pole. Unfortunately, I do not believe that they have shown that they are ready to expand to the IceCube size array. The present Amanda analysis of the Amanda-B results is only efficient for neutrino detection in a relatively narrow cone centered on directly upward going events. This is due to the cuts that are put on the data to eliminate cosmic ray muon background, in particular the cut requiring a track path length of 100 meters. The aspect ratio of Amanda-B is too large to have any efficiency away from the vertical. This means that the muon background and the cuts that would be required to eliminate it have not been fully investigated."* [Since this might imply a need for different spacing]….*" One simply does not know this now, but with analysis of Amanda-II data it should be possible to understand this important question. For this reason, I cannot support going ahead with the IceCube proposal before analyzing the Amanda-II data."*

Another panelist says*: "The AMANDA project, carried out by members of the IceCube collaboration, has demonstrated the experimental technique in a 10,000 m^2 scale installation. By using the ice at depths > 1 km, where the optical properties improve considerably, AMANDA has successfully reconstructed atmospheric neutrino events. The simulations appear to match the data adequately, and the analysis for rejection of backgrounds is continuously improving. There are now physics results from the group, including an interesting limit on the diffuse high-energy neutrino flux, limits on point sources, and limits on WIMP annihilation signals and magnetic monopoles. The group is obviously actively engaged in physics analysis, and this work should continue to grow as the instrument is understood better and the data from AMANDA II accumulate. It is important that results continue to be published. The excellent progress with AMANDA demonstrates that the technical case for taking the next step is very strong."*

A third member observes: *"In spite of the importance of exploring a new frontier, the scale of the project is not well justified. As proposed, IceCube is an enormous undertaking. It would be ten times larger than the proposed ANTARES. The construction would greatly stretch the resources of the collaboration and there are real concerns about whether the collaboration could pull off such a project. IceCube would also dwarf most of the other efforts in particle astrophysics (e.g. in solar neutrinos, dark matter, gamma-rays, UHE cosmic rays etc.). Given the fact that the science is uncertain, the proposed scale of expansion is difficult to understand. A somewhat descoped version of IceCube, perhaps of size 250,000 m^2, would be almost an order of magnitude increase above AMANDA-II and would still be substantially larger than*

ANTARES." The same reviewer adds: *"At face value, these arguments [of the proponents against size reduction] make sense. It does not make sense to simply shrink the IceCube detector by removing strings or by reducing the number of OM's/string. The key point, however, is that this argument is based on assumptions for the energy threshold for the detector and thus for the OM spacing. The argument for a particular energy threshold is weak because we don't know what the flux levels (and their energy spectra) are anyway."*

A fourth panelist says: *" The technical design which has been chosen as the baseline by the collaboration has been demonstrated by the series of AMANDA experiments. It is wise for this group to chose the proven technology and to have high standards for any changes to this plan, such as implementing the digital OMs. The ability to Monte Carlo events in the ice has been demonstrated by the AMANDA results, and so the predictions for Iceube are reliable. The physics analysis strength of the group has been demonstrated by the very nice results which are coming out of the AMANDA experiments. It is important that this strength be maintained while the IceCube experiment is being built. In my opinion, the group needs to address more clearly how this will be done. …. In my opinion, the group needs to enlarge in order to be able to actually perform the IceCube experiment."*

Another member points out: *" Finally, it is possible that real progress in understanding neutrino production sites, beyond the non-trivial fact of their existence (implying VHE proton acceleration and a target) may have to wait for a detector with sufficient acceptance to collect more than the handful of events that IceCube is likely to get. Having said all that, it would seem that I should oppose the construction of IceCube. However, I do not. Not proceeding with IceCube would be tantamount to giving up on neutrino astronomy (at least in the U.S.) If neutrino astronomy is to be pursued, and I believe that it should be, IceCube is at the right scale, and uses the right technology, for the next step. It is very important that the next neutrino telescope be large enough to have high confidence that a signal will be seen. Building a marginal instrument runs the serious risk of killing neutrino astronomy for the forseeable future. The available guidance as to what detector scale size provides this level of confidence is scanty, but not zero. For example, Waxman and Bahcall have pointed out that the neutrino flux from sources contributing significantly to the ultra-high energy cosmic rays is bounded. The characteristic event sample at this bound for a detector of IceCube's size is of order 10's of events per year, validating the rough scale of IceCube as a first viable neutrino observatory."*

A sixth panelist suggests: *"I am persuaded that this is more of a 'discovery' instrument than a 'special purpose experiment'. The models do drive the size toward 1 km* ^{3} *and the large overall cost but I am reluctant at this stage to say 'go full speed ahead to the complete IceCube' at this time. I fully support the ultimate goal (to have an IceCube-like device) as well as the next 'scheduled' set of installations but there are still some detailed questions to be answered which I feel can come from (and during) the operation of the just completed AMANDA-II and which might show how further improvements or new directions might come."*

And another SAGENAP member states: *" Questions remain about the extent to which upward going neutrinos can be securely disentangled from background, about the calibration of the response of the detector at ultra-high energies, about the management structure for a very large project of this nature, and about available bandwidth for returning data during the austral winter. Moreover, it is very difficult to quantify the cost-benefit balance for a device whose most important application probably is still unknown. While these are serious and important issues, none appears to be fundamental and* *insurmountable. A decision to proceed would in time produce a remarkable detection array."*

A new management for AMANDA and IceCube was presented to SAGENAP. It would be centered at the University of Wisconsin (Madison) and administered within the Antarctic Astronomy & Astrophysics Research Institute (A^3RI) of the university's Space Sciences & Engineering Center. Panel members were not familiar with the prior management experience on large projects by the Center or its sub-unit, A^3RI. Several members of the panel were concerned that SAGENAP does not have a thorough enough understanding of some aspects of the management plan. The panel members emphasized that there are two large, simultaneous tasks ahead --- the need for prompt, continuing analysis of AMANDA-II and the construction of IceCube itself. Before construction can proceed the group still has some technical decisions to make (electronics for the OM's to cite one example). It should be clarified how the management structure will facilitate such decisions as others will surely arise. Although the group is not small, and it has already achieved much, by self-admission they acknowledge the strain on human resources has been substantial; it appears not likely to diminish. It is felt that there should be a clearer set of milestones for the project and a better understanding as to how they will be set. For these and other examples several members of SAGENAP suggest that a management review be conducted by the appropriate office of the agencies involved.

In addressing this issue one member states: *"The IceCube collaboration seems to be roughly the right size for this experiment, and has the technical expertise needed to carry it out. The group has developed this expertise on AMANDA, and should be uniquely qualified to estimate the costs and manpower requirements of IceCube. However, the IceCube project represents an order of magnitude increase in scale relative to AMANDA, and will have to be managed in a completely different fashion. It was not clear from the group's presentation before SAGENAP that the necessary project management is in place. "*

Another panelist comments: *" Before the review, this reviewer had some apprehension regarding the overall strength of the collaboration and its ability to build IceCube. The presentations did not remove these concerns, but in fact highlighted the major difficulties."* The same reviewer adds: *"A scan of the author list for the proposal indicates many people who are heavily committed to other projects. The group did not demonstrate that it had the number of FTE's to carry out the full project."*

From a third member: *" In the short run, a technical and management review of the current proposal may be in order to refine an assessment of the technical, management and financial scope of the overall project. While internal and external negotiations ….. take place, NSF should continue the support of the Amanda program and of the developments necessary for IceCube (in particular the drilling)."*

One panelist says: *" The collaboration that has put together Amanda and made it a success is a relatively small one, and it deserves congratulations for this achievement. The Amanda collaboration is likely too small and its organization structure too flexible to accommodate as it stands a project the size of Ice Cube, but there is little doubt the collaboration will be augmented appropriately if the project goes forward. Simply putting in place the infrastructure to field the array will take 2 years, and in that time the collaboration can be strengthened where necessary."*

Another SAGENAP member adds: *"I was somewhat startled by the discussion of the analog vs digital signal handling; it seemed to me they do not have all their ducks in a row as to how that decision/selection is to be made --- nor by when that milestone must be set. Up to now they have been stretched very thin in building the detector under* *the unusual demands of the Antarctic and doing analysis at the same time; they have to get their manpower/time better organized since those demands of operation/analysis is a constant into the foreseeable future. They no doubt need more collaborators too."*

In summary, SAGENAP members were unanimous in their praise of what has been accomplished by the AMANDA collaboration and recognize that a new method for studying high energy neutrinos has been successfully developed. There was majority support by SAGENAP for the scientific goals of this project and interprets these goals to be the construction and operation of a large detector in the Antarctic ice to search for evidence of, and the subsequent study of, high energy neutrinos from extra-galactic sources. A successful project could be the opening of a new field of high energy neutrino astronomy. While SAGENAP members felt that the project should continue toward enlarging the detector by building on the current successes of the AMANDA-series, they were not convinced that there was yet enough evidence that the largest configuration (I km^3) envisioned by the IceCube group would necessarily function as proposed nor that the group had made a clear set of decision milestones for itself. There was a SAGENAP consensus that it will be essential, in deciding what the final size and configuration should be, to exploit the data currently being collected by AMANDA-II. It was also felt that the highest priority should be given to analyzing the AMANDA-II data. The 2 year period during which the Antarctic Polar Station facilities are being worked on and in which no IceCube construction can occur provides a window in which new information from AMANDA-II confirming or causing revision of design or milestones can occur. A final decision on what the approved size should be can be made during that period. SAGENAP also suggests that a review of the new management and decision making structure would be a valuable service to the project.

MILAGRO

MILAGRO is a ground-based, very high-energy (>500 GeV) celestial gamma-ray detector. In contrast to air shower Cerenkov telescopes (ACT) such as Whipple, which follow the photon-induced shower via the generated Cerenkov light, MILAGRO is an air shower array, which directly detects the shower particles as they hit the ground. Although air shower arrays only detect the tails of the showers, and hence have relatively poor energy resolution and pointing resolution, they offer some complementary features: ACTs have excellent pointing resolution (for gamma-ray detectors) and decent energy resolution, but they can only operate on cloudless, moonless nights and they have very limited field of view (FOV); whereas an air shower array can take data at all times and cover a sizable fraction of the sky. Since the gamma-ray sky is highly variable, these features can be quite important.

The performance of an air shower array depends on the spacing of the sensors in the array. For a fixed number of sensors, a relatively close spacing will provide sensitivity at lower energies, but will have compromised sensitivity at the high-energy end due to a relatively small collecting area. MILAGRO, which is located ~35 miles west of Los Alamos in the Jemez Mountains, is an unusual and clever air shower array. At its heart is a 60mx80mx8m pond of water instrumented with 450 20 cm-diameter PMTs at 1.5 m depth, forming a continuous, wide-area water-Cerenkov detector. MILAGRO thus has a very low energy threshold for an air shower array. Co-located in the pond at a depth of 7 m in a second layer of 273 PMTs, used for detecting the muon content of the shower for purposes of background rejection.

The primary gamma-ray energy is inferred from the number of particles detected, and the incident direction is found by timing across the array. For these techniques to work effectively it is essential to locate the core of the shower, which often falls outside the relatively small pond. Thus, to achieve its fullest capability MILAGRO must be surrounded by a supporting sparse array of detectors, called outriggers.

The initial MILAGRO proposal was submitted in 1991. The original plan for the outriggers was to use scintillator counters from the old CYGNUS array, but this was later found to be an inadequate match to the low energy threshold of MILAGRO. Construction of the pond detector began in 1994, but was stretched from 3 to 5 years due to funding. To gain interim experience, a sub-array of 228 PMTs, dubbed 'Milagrito', was deployed in 1996 and was operated for 15 months. Milagrito had a live time of approximately 80%, and produced results. These include a ~4σ detection of Mrk 501, an intriguing hint of TeV emission accompanying GRB970417, and detection of a ground-level event associated with a solar coronal mass ejection. There are no science results yet from the full MILAGRO array, but the shadow of the moon in cosmic rays has been detected. The full MILAGRO array was completed in 1999, with physics runs starting in December.

The present proposals are for equipment to complete the outrigger array, and for continued support of the collaborating groups. The PI for the upgrade is Jordan Goodman (Maryland) with co-PI's from several of the collaborating academic institutions. The outrigger design is a ~200m x ~200m array of 175 8ft diameter x 3ft high water tanks, each read out with a single PMT, spaced ~15 meters apart. Eleven test tanks have already been deployed and appear to work well. The PMTs for the outriggers were purchased as part of the main MILAGRO construction, so the total request at present for the outriggers is $434K.

The collaboration consists of groups at UC-Irvine, UC Santa Cruz, George Mason University, Los Alamos National Laboratory, University of Maryland, University of New Hampshire, New York University, and the University of Wisconsin-Madison. Operating and computing funds are also requested for these groups ($1.5M/year at universities, ~$240K/year at the detector site and continued support of the LANL group; $540K for computing at the site and at universities).

The present configuration of MILAGRO has been built on a very tight budget and brought to its present successful operating level by a resourceful and dedicated group. These circumstances have contributed to a longer construction phase than would be optimum since the field of high energy gamma-ray physics continues to develop rapidly. In their presentations to SAGENAP they showed analyzed data and Monte Carlos to support the request for improvements to the detector as well as the data on preliminary physics results taken with the detector before it was fully instrumented. The MILAGRO detector is now running and collecting data. SAGENAP wishes to acknowledge this impressive achievement and is concerned that no momentum be lost so that the detector may be exploited rapidly in those areas of its strongest capability.

SAGENAP members agree that MILAGRO has a valuable and nearly unique role to play in high energy gamma-ray astrophysics. As has been noted, as an extensive air shower array it complements the air Cherenkov telescopes and the future GLAST satellite. It has a nearly 24 hour duty factor and a very large sky coverage at any one time with a relatively low threshold. These features give it the potential to discover powerful, new transient sources and to provide a real-time alert to the rest of the gamma-ray community. SAGENAP believes that the exploitation of this capability should be given very high priority by the MILAGRO groups.

The detector hardware part of the new budget, $434K, is requested to construct the outrigger system. It is estimated by the proposers that the principal gains over MILAGRO without the outriggers would be to improve the sensitivity by a factor ~2, its angular resolution by a factor ~1.4 and supplement somewhat its energy resolution. Using the Crab nebula as a standard candle these improvements would permit a 5σ determination in 120 days if coupled with improvements in their analysis algorithms. While this is a long period relative to Cherenkov telescopes, these latter are virtually incapable of fulfilling the transient discovery role. A majority of SAGENAP panelists favored the addition of the outriggers and saw it as strengthening MILAGRO's value to the field. MILAGRO in its present form accumulates very large data sets and all SAGENAP members expressed a concern that the construction should not interrupt the prompt and timely analysis of these data. Under the present manpower commitments and budget constraints on the involved institutions, it would appear these two tasks --- construction and analysis --- are in severe competition (see also further discussion of manpower below). Recognition of this competition and the possible consequences of not resolving it temper the observations about the value of the outriggers as the following sampling of members' reports show.

As one panelist says: *"The outriggers bring the expected detection significance after 120 days from 2 sigma (assuming an optimized analysis) to 5 sigma. Given the importance of having a source on which to tune the analysis, this factor alone seems to justify the cost of the outriggers. Adding to this the improvement in angular and energy resolution (without the upgrade, MILAGRO is at best a trigger for other experiments, with little if any analysis power of its own) makes a strong case for the upgrade."*

Another member states: *" I favor continuing the experiment at a reasonable level of funding, which would permit both the outrigger tanks and the computing that they seek. I would also recommend keeping the pressure on this group to analyze their data and produce physics results, and to develop the capability to alert other gamma-ray experiments to flaring sources, as soon as possible."*

From another person: *"The outriggers were in the original design of the experiment and do significantly improve the resolution and enhance the scientific interest of the results. In my opinion, with only ~$400k standing between the present stage and the final design, MILAGRO should be finished."* This person also adds: *" If an alert-system were installed, it could perform a very important service to experiments world-wide with its present design. It is clear that the collaboration is stretched very thin. It appears that with the present people and funding (with their ongoing commitments to other projects) the group can concentrate on either the outriggers or the present detector physics."*

A third panelist observes: *" The outriggers are essential for making the project worthwhile. The cost of the outriggers is relatively small, and well worth the investment. Constructing the outriggers should not divert too much attention from doing science with the array. One might wonder if construction of the outriggers would divert scientists from doing science analysis; however, the budget for the outriggers includes incremental labor costs for this reason. It was clear that if funding were squeezed, MILAGRO scientists would themselves construct and deploy the tanks. Thus, fully funding the outriggers will enable scientists to do science. Full funding of the outriggers should be an extremely high priority for this project. Doing MILAGRO only makes sense with the outriggers."*

And from another panelist: *" The outrigger array to define the shower core is also an important adjunct that will increase the resolution significantly at modest cost. One of the most damaging and serious charges that can be levelled at scientific support in the*

US is the failure to follow through. It makes little sense to spend taxpayer dollars to build a valuable experiment only to plow it under the minute it begins to run."

A fifth member says: *"The group makes a good case that outriggers will both improve energy and angular resolution; this is entirely believable given elementary properties of the shower front. The physics motivation for the outriggers is a little harder to understand. True, they improve resolution, but to what end? Perhaps with the demise of EGRET, MILAGRO will have more importance as an "alert" for very high energy gamma ray bursts, in which case the outriggers could make sense. However, this "alert" capability was not directly addressed by the collaboration. (This potential is seen in their probable coincidence seen with GRB970417a.)"*

Another SAGENAP member suggests a similar view on the outriggers: *"MILAGRO is an experiment with a unique capability that should be exploited as much as possible, and as soon as possible. Dragging out the construction of the experiment (e.g. adding outriggers) for a modest increase in sensitivity may actually do more harm than good. The outrigger proposal will improve the performance of the detector, but it may be wisest to simply stand pat at this point and concentrate on detector operations."*

There were concerns about the size of the operating budgets, availability of manpower to add the outriggers, to operate the detector and to analyze the data; consequently, several questions were directed to the proponents about the prospective organization of these tasks and possible other commitments of collaboration members. SAGENAP felt that the operating costs assigned to the aggregate of the university groups seemed high and that the organization plan was incomplete and needed to be tightened-up. It was also seen that a key point in all aspects of any plan will be to clarify the future funding of the LANL-based collaborators who appear to be essential to the experiment. No specific possible arrangement for such funding was considered by SAGENAP. SAGENAP panelists were uniform in citing these concerns about manpower and its organization but differed somewhat in emphasis as the comments selected below show.

One SAGENAP member comments: *" …. it appears that the MILAGRO collaboration is somewhat diffused and not completely cohesive. (There are almost too many institutions, given the typical group size at each institution, but this is not a unique problem to MILAGRO). Another problem is that a number of faculty members nominally on MILAGRO are also heavily involved in other efforts, and thus there may be a problem of the total FTEs actually working on MILAGRO. The FTE numbers presented at the meeting did not mitigate this concern."* This reviewer also adds: *"Each group should be examined on the basis of its committment to the project and its role in the operations of the experiment and in the analysis of data."*

Another panelist states: *" The roles of all the collaborators in the project, particularly for science analysis, are not obvious. The organization needs to be strengthened so that the collaboration can pull together to do the detailed work necessary for useful science. If this can be done, funding these groups would certainly be justified. The Los Alamos and Santa Cruz groups are especially vital to this project, and a mechanism should be found to continue to support them."*

Another SAGENAP member adds: *" ……. Hoffman's group [LANL]is one of the main engines in MILAGRO and I would not want to see them dropped from the collaboration."*

And from a fourth panelist: *" The detector has been built by a core group of Los Alamos physicists working with key collaborators from several other institutions. It is an unusual astrophysical observatory, not falling conveniently into the categories for which funding has traditionally been provided at Los Alamos. "* and also adds, *"It is time for the* *agencies to exercise some creativity in finding a way to support the rather minimal needs of the collaboration, especially the Los Alamos members."*

Finally, a member observes: *"The group has demonstrated through the construction of the central MILAGRO detector that it is capable of mounting the construction effort needed to build the outriggers. The construction costs are estimated at a high level of detail (down to the cable), and seem reasonable. Operating support for the groups participating in MILAGRO is included in this proposal. If the upgrade is approved, the groups should be supported at the level necessary to carry out construction in a timely manner, and to continue aggressively to make progress on the analysis of data as it is acquired."*

In summary, SAGENAP reaffirms the special role that the MILAGRO detector can play in the field of VHE gamma-ray astrophysics by virtue of its complementary nature to other detectors in the field. The panel was satisfied that the collaboration has brought the detector to a high level of operating performance. The effective participation of the LANL group in achieving these and future goals was stressed by several members. On the potential importance to the field, it was emphasized by SAGENAP members that the capability of MILAGRO to detect and rapidly provide an alert to the community (of the early detection of high energy transient sources) should be the highest priority for the group to establish. This implies rapid and timely data analysis at the site. Consequently, it was felt that a first claim on computing funds awarded should go for that purpose. With respect to the request to add the outriggers, the majority of SAGENAP members favored their addition but all warned of the inherent risk to the overall value of the project if the outrigger construction compromised the timeliness of the scientific results. A part of this concern arose because panel members perceived that the organization for construction and analysis needed to be refined, more completely specified and that the collaboration should evaluate more fully their members' FTE commitments to this experiment. It was expressed that a more completely specified organization plan could serve as a guide to the awarding of operating funds among the various collaborating institutions. While reaffirming the unique value to the field of MILAGRO several SAGENAP members suggested that the experiment's major opportunities may well be in the near future and that a timeline of perhaps three years from now may be an appropriate time to evaluate scientific achievement and next directions for the experiment.

AXION

SAGENAP members concurred that experiments capable of discovering or ruling out any or all of the well motivated theories for the axion as a component of dark matter are particularly important for particle physics and cosmology. A key issue in the long development of the field has been whether the technology was available to reach the limits of necessary sensitivity over a wide enough range of masses to include all the models. Axions are one of the three best motivated particle candidates for non-baryonic dark matter. The two other favored candidates are Weakly Interactive Massive Particles (such as the supersymmetric lightest partner) and light massive neutrinos (which are disfavored as a dominant component by large scale structure arguments). Axions have been postulated in order to dynamically prevent the violation of CP in strong interactions in the otherwise extremely successful theory of quantum chromodynamics. There is no guarantee that such particles exist, but the present laboratory and astrophysical limits on their parameters ($10^{-6} <$mass$< 10^{-3}$ eV/c^2) are such that if they exist, they would form a significant portion of cold dark matter. Although the production mechanism of these axions at the Peccei-Quinn phase transition is well understood, a theoretical uncertainty remains in the likely case where the transition occurs after inflation. In that case, global strings are produced and radiate axions. The technically difficult calculations of this axion radiation tend to disfavor the low-mass region, unfortunately the one which is currently accessible by experiments. Such low-mass cosmological axions could be detected by interaction with a magnetic field, which produces a faint microwave radiation at a frequency directly proportional to the axion mass. The spectral feature should be very narrow and detectable in a tunable cavity. This high Q cavity equipped with the best available RF amplifier and maintained at a low enough temperature to limit thermal noise, has to be swept in frequency to cover the relevant mass region. This narrow band method is time consuming but a number of controls are available to the experimentalist, in particular the dependence of a tentative signal on the magnetic field. The expected power depends on the coupling of axions to photons, which is fixed by the specific axion theory being considered as a function of the mass. Fortunately, most of the viable models give axion couplings which differ only by a factor 10, with the KSVZ model being typical of the most favorable ones and DFSZ being a generic type of axion with low couplings. The first two searches for cosmological axions performed a decade ago were missing a factor of 1000 in sensitivity. This is no longer the case as is discussed below. A Japanese experiment, which uses highly Rydberg atoms as photon detectors, should be able to reach a DFSZ type sensitivity, if it can master the technological challenge of such a scheme.

The group involved in the present proposal (consisting of scientists from University of Florida, LLNL, MIT and UC-Berkeley) have shown in their present experiment that it is possible to reach a sensitivity adequate to test the KSVZ model. Using a four microwave cavity apparatus they have been able to speed up the coverage in mass and expect to have covered the range $\sim 3\times 10^{-6}$ to $\sim 1\times 10^{-5}$ eV/c^2 down to the KSVZ limit during the remainder of the year. This successful running commenced about the time of their last proposal presented at SAGENAP, at which time they proposed an extensive ($7.9M) upgrade of the experiment; part of the improvements to have been included were a 100mK dilution refrigerator, a new magnet and introduction of SQUID amplifier technology. At that time SAGENAP's suggestion was to defer any extensive upgrade until the four cavity version has had extensive running and to continue R&D on cryogenic amplifiers (SQUIDs).

The new proposal envisions a staged approach to increasing the sensitivity down to the DFSV limit and to explore extending the frequency range of the SQUID technology to reach even higher masses. In Phase I they will search for an axion signal in the lower decade of the allowed axion mass window. The intent is to use a new SQUID amplifier that will produce a factor of four or so less noise in its initial operation thus allowing an axion search in this mass interval to the KSVZ sensitivity level. In a Phase II (only R&D funds requested at this time), further development of the SQUIDs and operation at colder temperatures and perhaps higher frequencies would allow a sensitivity increase to the DFSZ level and an exploration of the next higher mass decade (10^{-5} to 10^{-4} eV/c^2). There is no current plan to explore the region from 10^{-4} to 10^{-3} eV. This proposal is for a total of $3.28M for the upgrade and $0.28M for SQUID amplifier R&D. Of this sum, $1.9M would be from DoE and $1.68M to NSF.

In support of their new proposal the group presented impressive results from the four cavity experiment and from the R&D on SQUID amplifiers. In the four cavity analysis the search is conducted through frequency sweeps in the very high-Q, tuned cavities. Each frequency appears in at least 45 sub-spectra which are weighted and added to produce the final spectra where absorption candidate peaks are initially tested for. Twenty-three candidate peaks were found which in final analysis were eliminated as radio noise.

Through its R&D on SQUID amplifiers, the UC-Berkeley group has shown major progress in achieving low noise and advances to higher frequency operation (amplifiers with 100mK noise and 35db gain at >1GHz). This would permit replacement of the HEMT amplifiers and allow 4 times faster scanning in a single re-designed cavity but still using the 1.3 K refrigerator. Thus in Phase-I with the resulting combined reduction in system noise (total noise T would now be at ~1.7K vs. old 2-5K) they can still reach the KSVZ axion and cover the next step in mass range at a higher rate. Expectations of further amplifier improvements and the acquisition of a 100mK dilution refrigerator in Phase II suggest that a full factor 15 improvement in noise could be achieved from both Phases and higher frequency capability would allow a step to higher mass at that time.

SAGENAP panelists were in uniform agreement as to the value of an experiment to search further for evidence of the axion and were particularly favorably impressed by the progress made by this group and its ability to reach the limits as proposed. A few quotes from panel members illustrate the strength of this agreement.

One panelist states: *" Axions are a plausible dark matter candidate, and this proposed experiment would be one of the only experiments in the world searching for it. A key to making the desired measurement is the reduction of system temperature, which is the sum of the cavity temperature and the noise temperature of the amplifier. Tremendous progress has already been made by this group in the development of new low-noise SQUID amplifiers. Though more work is needed, it is clear that they should be able to improve the current limits on axion mass significantly."*

A second one observes: *" The science is excellent- the axion is important enough to merit a substantial search effort. The …. group has established the premier experiment in the world. They have no real competition. This is a testament to their strong technical skills and very hard work. The discovery of the axion would have profound significance. Even the exclusion of the axion over one decade in energy would be an important goal. Thus, this group which has a strong track record should be supported and encouraged to continue their pioneering work."*

Another member comments: *"There is every reason to believe that this group will reach their stated sensitivity. The technological achievements which are necessary appear to attain this goal appear to be well within their grasp. The dedication and capability of the group is impressive."*

A fourth panelist states*: " They propose to do this experiment, and concurrently to do R&D on a new generation of SQUIDs and resonant cavities that could allow this mass range to be extended upward by another order of magnitude. Axions remain, with supersymmetric WIMPs, the only particle physics cold dark matter candidates that are well motivated, in the sense that they were predicted by particle physics independently of their possible role in cosmology. Even though with an estimated cost* of [for both Phase I & Phase II] about *$10 million for the 1-10 micro eV search this experiment is a rather expensive one, this opportunity to do a definitive experiment on axionic dark matter can hardly be passed up. It is worth noting that we are far from knowing how to achieve sufficient sensitivity to do a definitive experiment on WIMP dark matter."*

Another SAGENAP member observes: *" The scientific goals of the collaboration are equally impressive – it appears within the reach of this group to either discover the axion, and failing that, to eliminate it as a significant dark matter constituent. This is truly refreshing – a bounded discovery phase space!"* The same person adds: *"Overall, I see* *this experiment as possessing in abundance all the things you look for in a great project – compelling and achievable science goals, advancement in the technological state of the art, and a group with the necessary competence to ensure a high probability of success."*

Management and work schedule plans for the Phase I of the project were presented. The budget would be spread out over a three year span beginning in Fall of CY2000. The co-spokespersons for the upgrade are K. van Bibber (LLNL) and L. Rosenberg (MIT). There is a project manager sited at LLNL and there is an external review board. The main funds are to go for substantial re-building of the experimental insert including a field compensation coil for protecting the SQUIDS. UCB designed SQUIDS are to be built by industry and packaged by UCB. MIT will build the microwave receiver. The high resolution data channel will be upgraded by Florida. The 4 cavity system would be shut down sometime this Fall. Phase-I running would begin in 2003.

SAGENAP agrees that the management plan appears to be sound and based upon their previous experience. The work plan schedule, while ambitious, is also based on their extensive construction and operating experience with largely the same group of individuals. Some panel member comments an these plans follow:

From one panelist: *" I think that the group is well managed. This is clear from the quality of their past physics results and their work over the last year. They made a very nice presentation of their new WBS structure. It looks sensible and appropriate for an experiment of their size. I have no problems with individuals appearing in several boxes. I would be very sorry to see them bogged down with too many further microsoft*[sic] *management requirements. These requirements are justified for huge collaborations and enormous detectors, but not for small, already-well-managed groups."*

A second member adds: *" ….. the group has responded to concerns with respect to project management by setting up a management structure which seems appropriate to the scale of the project."*

And a third panelist concludes: " *This reviewer sees no problems whatsoever with the current management scheme."*

In summary, SAGENAP members re-affirm their opinion that the continued experimental search for the axion is very important. The panelists also view the work so far by this group to be of the highest quality and fully endorse the Phase I of the upgrade to the existing axion experiment. They believe that the experiment, which this project will enable, is unique and has a high probability of extending our knowledge of axions in a definitive way. The management, work plan and budget proposed appeared to be appropriate to the tasks. Additionally, it was noted by several members that this particular search is unique in the field and that the successful R&D on cryogenic amplifiers for the experiment may well have significantly wider application.

EXO

The EXO project (for Enriched Xenon double-beta decay Observatory) intends to develop a powerful new technique to search for neutrino-less double-beta decay ($0\nu\beta\beta$) from ^{136}Xe. The present proposal is for the initial R&D at Stanford to investigate its feasibility. The collaboration includes scientists from University of Alabama, CIT, IBM, ITEP-Moscow, University of Neuchatel, INFN-Padova, SLAC, Stanford, WIPP, and the universities of Trieste and Torino; the spokesperson is G. Gratta of Stanford. The technique attempts to exploit the uniqueness of a double beta decay signature that includes not only two final state electrons, but also simultaneous event-by-event identification of the stable daughter nucleus of Ba. If successful, background in a ^{136}Xe experiment can be essentially eliminated and the technique could therefore probe for Majorana masses down to the order of 10's of milli-electron volts, well beyond the range of current experiments, and where the real interest in double beta decay currently lies.

The existence of neutrino mass seems now to be almost beyond doubt, with quite convincing data from atmospheric neutrinos, solar neutrinos, and possibly the LSND experiment. Oscillation experiments cannot provide an actual mass scale, however, as they are only sensitive to mass differences. Nor can they provide information about the charge conjugation properties of neutrinos. The Majorana or Dirac nature of neutrinos cannot be discovered in a straightforward comparison of neutrino and antineutrino interactions owing to the intrinsically left-handed nature of the weak interaction. The observation of neutrino-less double beta decay is the one avenue open that can provide both types of information, if nature has endowed neutrinos with a Majorana mass. The occurrence of $0\nu\beta\beta$ decay depends on an effective mass that consists of a sum over mass eigenstates that couple to the electron, with each term multiplied by a CP-non-conserving phase. In addition, other non-standard-model effects such as departures from pure V-A can modify the transition matrix element. Hence, in view of the many possible cancellations, non-observation of $0\nu\beta\beta$ is not particularly informative, whereas a positive signal is extremely definitive. With atmospheric oscillations indicating a mass scale for ν_μ and ν_τ of order 0.05 eV and LSND pointing to masses in the sub-eV range, there is the strongest motivation to discover a way to push the present 0.2-eV double beta decay limits (from ^{76}Ge experiments) down an order of magnitude or more.

There are many groups currently running or building experiments to search for $0\nu\beta\beta$ but in all cases the backgrounds are already non-zero. Three factors are fundamental to a successful double beta decay experiment. These are control of background, large fiducial mass to achieve detectable rates, and excellent energy resolution to separate the $0\nu\beta\beta$ peak from the $2\nu\beta\beta$ continuum with the same endpoint. Two of these factors are in competition, the signal rate and the $2\nu\beta\beta$ background, because the former improves with a high energy release in the decay (as E^5), but unfortunately the $2\nu\beta\beta$ background rises as E^{11}. The most success has been obtained with ^{76}Ge because of the modest energy release (2.04 MeV) and the superb energy resolution of Ge detectors. In a background free experiment, however, increases in the detector mass can translate more directly into sensitivity increases.

The R&D in this proposal is intended to lead to an experiment which would use a 1-ton Xe (enriched to 65% in ^{136}Xe) TPC. The TPC would be about 40 cubic meters and initially be run at 5 atm. Once the design and technique is proven, the project could be extended to10 tons of Xe by adding more TPC modules or increasing the pressure in a single TPC to 50 atm. After a five year run the 1-ton design is expected to improve the current Xe neutrino-less double-beta decay lifetime limit by four orders of magnitude, reducing the upper limit on neutrino mass by two orders of magnitude to between 0.02 and 0.05 eV. This result depends on achieving a close to zero background condition by adding tagging of the ^{136}Ba daughter (via triggered laser spectroscopy) to the gas TPC method previously used for ^{136}Xe double-beta decay. One significant challenge is to pinpoint a single atom of barium inside a 5 meter-long cylindrical tank. The proposal identifies a number of areas of preliminary research to achieve this goal, these include:

- TPC design parameters.
- Energy resolution (The background cannot actually be zero in this experiment due to the finite resolution of the TPC and the consequent mistaken identification of the high-energy tail of the much more frequent $2\nu\beta\beta$ as $0\nu\beta\beta$).
- Electronics and trigger design.
- Ba^+ tagging in Xe:
    - Pressure-broadening effects on the Ba^+ line.
    - Laser power requirements.
    - Laser aiming.
- Exploring the possibility of using solid or liquid Xe.
- Simulation of backgrounds at the WIPP site.

The present request for funding is limited to work at Stanford on some of the issues of Ba^+ tagging.

In the group's very interesting written proposal and oral presentations to SAGENAP it was emphasized that the project was still in its very early stages and that the ultimate design of the detector is quite fluid. However, two conceptual designs (and variants on them) were illustrated and served as excellent foci for discussion of challenges and opportunities. Also presented were descriptions of preparations for R&D already begun at Stanford and SLAC with seed funds and equipment from those institutions and IBM. The preparations are for work on Ba ion spectroscopy and investigations of solid or liquid Xe as an alternative to the gas TPC. An essential ingredient to even consider an experiment on the scale proposed, involving as much as ten tons of enriched ($\geq$65%) Xe, is the opportunity to make use of ultra-centrifuge isotope separation plants in the uranium facilities of the former Soviet Union. SAGENAP was told discussions to this end are under way and preliminary tests involving ORNL are also under consideration. It is anticipated that no basic-research funds would be required for this purpose.

With respect to the various R&D tasks enumerated by the group and outlined above, no definitive plan was presented (beyond the Stanford request) although the specific areas of responsibility each collaborating institution was expected to have were stated.

SAGENAP members agree with the proposers that a successful experiment to actually observe $0\nu\beta\beta$ or to improve the mass limit by one or more orders of magnitude is one of the most basic in neutrino physics. And further, because of the neutrinos' role in so many natural processes, it would have broad implications beyond particle physics. They were also in agreement that the innovative technique suggested, although noting that the experimental challenges appeared formidable, nonetheless seems sufficiently attractive to begin a serious R&D program which might eventually lead to an approval for a detector project. More specific views expressed by the panelists on the proposal and its R&D can be seen from the following sampling of their reports:

One reviewer comments: *" This is quite a speculative project in the sense that the techniques are considerably beyond the state-of-the-art, but I think it is well worth the effort. The people on the proposal are first rate and the connection with the IBM lab and its expert on laser resonance spectroscopy will allow them to go forward on the Ba+ identification quickly. There is some overlap of personnel with the KamLAND construction and first operation, and I worry that one of the two will suffer a lack of attention. A post-doc at Stanford would help."*

Another states: *" The strategy proposed by the collaboration is, in the opinion of the reviewer, not quite optimal. There is little doubt that with conventional proven techniques in use today, a dramatic improvement in double beta decay effective mass sensitivity could be achieved given a ton of enriched* ^{136}Xe*. The detection of Ba daughters is certainly a nice confirmation of double beta decay (although not* $0\nu\beta\beta$*), but should not be presented as crucial to the success of the endeavor. One should begin in a more conservative vein to assure a measurement with a large quantity of enriched Xe using either liquid or gas to confirm that the energy resolution needed can be achieved. Without that, no amount of Ba detection will help. Development of Ba detection would be appropriate as a longer-term program, especially desirable if a signal should be seen. The lead time for success in Ba detection can thus be stretched considerably, while the justification for enriching and working with Xe right now becomes very transparent.*
The existing collaboration has made a fine start; it will need to be strengthened somewhat and more formally organized as the project goes ahead. The strong support of ISTC and SLAC are very positive factors." The same reviewer also adds: *" With or without Ba detection, the* ^{136}Xe *EXO experiment is likely to be the best hope for reaching the 0.01-eV level in* $0\nu\beta\beta$ *and is distinctly more promising than GENIUS. The reasons are the freedom to modify experimental conditions, to repurify the Xe, and to use a variety of track-recognition and background-rejection techniques that are not available in Ge. R&D should be supported soon with the major emphasis being demonstration of high energy resolution in large Xe volumes as described above. Enrichment should begin as soon as it can be arranged. This is a completely unique opportunity that must not be passed up. The scientific motivation is very compelling."*

A third SAGENAP member says: *" The team is very strong. The combination of Gratta's group at Stanford with outstanding scientists at SLAC should lead to a strong R&D program. There are many, many uncertainties about this effort, but at least there are very good people thinking about them. There may be some concern that Gratta is over-committed (i.e. he is an important component of KAMLand), but EXO is in such an early R&D phase that this should not be a major problem."*

Another panelist observes: *" In my opinion, this idea looks promising. I believe that the request for R&D for locating the Ba+ ion should be approved. In my opinion, the full R&D request should be granted. However, again I believe SAGENAP should be cautious in its statement -- only supporting the R&D at the moment."* Also adds: *"This is an extremely capable group for the R&D effort. We should note that the proposal was well-written and convincing. This group knows what it is doing. I applaud the involvement of SLAC in pushing this proposal forward."*

One SAGENAP members says: *" [The R&D] is largely to look into the efficacy of the Ba tagging. I believe that energy resolution is a much more serious question to be addressed because of the presence of a large rate from two-neutrino double beta decay. While there is still a TPC in the new design the burden of energy resolution is shifted to the TPC and the use of as-yet-unproven read-out technology …. in an unusual kind of* *high pressure environment (50 atm.) of the TPC containing various gas additives. While the initial R&D request (presently only for Stanford) is quite reasonable and perhaps apt, we need a better handle on what the rest of the R&D iceberg is likely to be …"*

Another panelist adds: *" I give the proposers high marks for having the courage to take such a technological challenge on. I should say, however, that their chances of success are greatly improved by the presence on the EXO collaboration of the SLAC group, which includes some of the most technically talented physicists active today. I support the R&D request of this group, but would defer any statement concerning the actual construction project until the basic feasibility of the technique has been demonstrated."*

In summary, SAGENAP members are in agreement that the physics motivating this proposal is compelling. They concur that it will take an experiment on the scale of this one to advance the search for neutrino-less double-beta decay. SAGENAP agrees that the opportunity to acquire very large quantities of enriched Xe for double-beta decay experiments is one that should be actively pursued by the appropriate agency channels and urges that be done soon. SAGENAP members felt it was too early to consider other aspects of the project; but they do agree that it was fully appropriate to go ahead with a well focused R&D program to test the feasibility and to determine what the next step should be. While not in full agreement on what the specifics of the first R&D should be, they do express confidence in the strength and abilities of the group to carry out the proposed R&D.

ZEPLIN-II

This proposal from the UCLA group is for ZEPLIN-II, a WIMP dark matter detector to be deployed in the UK Boulby mine underground dark matter facility in England.
The ZEPLIN-II detector has liquid Xenon as the target. Halo dark matter particles (WIMPS) entering ZEPLIN-II could recoil against a Xe nucleus (even or odd isotope), resulting in a direct scintillation signal viewed by a bank of phototubes, along with recoil-induced ionization. The ionization electrons drift along an applied electric field to a liguid-gas interface; here they are accelerated in the gas and amplified, inducing secondary light emission from a CsI internal electrode. The secondary light signal is detected by the same photomultiplier bank after a time delay. Electromagnetic background rejection is enhanced by drift (inhibiting recombination) and multiplication in the gas, and will be employed in ZEPLIN-II. The presenters argued that this liquid Xe technology held the promise good sensitivity to realistic WIMP candidates at relatively modest cost.

The UCLA and their collaborating Italian group has published results from a 0.5 kg test liquid-gas chamber in which they reported good electromagnetic-neutron separation from, ~10 keV equivalent recoil (primary scintillation) energy to upwards of 200 keV. Their proposal to SAGENAP included a figure showing good electromagnetic-neutron separation down to 10 keV (where still only ~5% of the electromagnetic scatters contaminate the nuclear recoils). The ZEPLIN-II detector is a scale-up of the small prototype chamber to 30 kg LXe target mass. With their planned threshold of 10 keV (or less) they claim a sensitivity at the level of 0.1 events/kg/day in a one year running period (this would be adequate to detect the claimed DAMA signal). They further say: " we estimate that a 30 kg detector based on the new technique proposed here could identify signal events at the level 0.1 to 0.01 events/kg/day in a running period of (less than or equal to) 2 years."

Also presented to SAGENAP was an (unsigned) memorandum of understanding among some of the Zeplin collaborating institutions for hardware deliverables. UCLA (the only requestor in the SAGENAP ZEPLIN-II presentation) would be responsible for the central part of the detector. The memorandum of understanding also contained a set of milestones and outlined the management structure.

The Zeplin-II presentation to SAGENAP were given principally by H. Wang (UCLA) with additional presentation material from D. Cline (UCLA) and N. Smith (Rutherford Laboratory (RAL) and Boulby Mine, UK). Dr. HanGuo Wang is project leader for the UCLA part of the project. The UCLA group, through its R&D with LXe, has successfully brought a new wrinkle to particle identification in LXe. They have shown in their presentation that low energy electrons (potential Compton background) can be distinguished from low energy nuclear recoils (the signal) by measuring the relative amounts of scintillation at two different times. The early scintillation comes from the initial de-excitation of the Xe^* dimers; the later scintillation arises by drifting the ionization to a gas phase region where the charges are accelerated and causing scintillation in the gas. The neutron recoils' primary ionization quickly recombines so that few ionization charges remain to be drifted hence there is little if any secondary scintillation in contrast to the electrons. Previous efforts (mainly by UK groups) to use LXe as a particle discriminator have relied solely on pulse shape discrimination (PSD). The UCLA method has been adopted by the collaboration as the technique for the Zeplin-II detector; the original Zeplin-I having used PSD.

The completed Zeplin-II is to be sited in the UK dark matter facility in the Boulby Mine in England. The laboratory already has a variety of dark matter effort in progress in the facility. The group collaborating on Zeplin-II consists of scientists from UCLA, RAL, Imperial College and the universities at Padova and Torino; they have been actively working together for some time on cryogenic liquid applications. The group has proposed a very aggressive time schedule for Zeplin-II. It would include work at UCLA to construct the Xe chamber including the electrostatic structure and, having been subjected to full cold test by Oct. 2000, then shipped to Boulby for completion as a running experiment. The running to commence in early 2001. The CAD design work is planned to begin in May. The UCLA work will be carried out principally by Dr. Wang, a student and the UCLA shops. The funds requested from the NSF ($405K over 3 years) are to construct the device and to participate in its installation and operation as part of the on-going dark matter experimentation at the Boulby facility. Major funding for the completion of the of the Zeplin-II is expected to come from the rest of the collaboration.

The SAGENAP members agree that experiments for the direct detection of dark matter are of fundamental importance. In addition to achieving an adequate level of sensitivity and background discrimination, it is also seen as important to have a high degree of diagnostic capability within the overall direct detection program. Since neither the mass of the putative dark matter particle(s) nor a definitive estimate for the lowest rate to be expected are known, it is useful to have a range of complementary experiments and techniques. The present disagreement between the Rome DAMA experiment and the very different CDMS experiment illustrates these points. Xenon by virtue of its high atomic mass, liquid density and particle detection properties is therefore an attractive candidate as a detection medium. In their discussions, the SAGENAP panelists were impressed by the UCLA work and the possibility that xenon can be made into a successful dark matter experiment. This point is made in the reports of several members; however, they also expressed concerns about the aggressive schedule, the manpower available for the UCLA portion and whether this impressive work can yet be said to be out of an R&D phase.

One SAGENAP panelist says*: " …. However, a significant drawback of this approach is the absence of an observable secondary scintillation signal from the recoil. This introduces the possibility of a large class of backgrounds that could be quite difficult to deal with. The proposers state that they have not seen background events of this type in their prototype detectors, but it is unclear that these tests have been conducted with the necessary sensitivity. In addition, the background estimates presented in the proposal seem rather crude, particularly those associated with the detector itself. Achieving their sensitivity goal will require residual background rates in the range 0.1-0.01 events/kg-day. As stated in their proposal, the background rates observed in a low-Kr Xenon chamber in the Gran Sasso is < 10/keV-kg-day. Conservatively taking the limit as a measurement, and given the gamma misidentification probabilities quoted in their proposal – ranging from 2 % at 10 keV to 0.2% at 40 keV, it is not completely obvious that they will be able to achieve their residual background rate goals. The participation of the UK group in this effort is a definite plus in this respect, given their previous experience in operating low background experiments in the Boulby facility."*

From a second panelist: *" This technique seems to work, with published instrumentation results. The UCLA and European groups feel sufficiently confident to propose ZEPLIN-II, a WIMP detector based on the liquid-gas Xe prototype. The immediate goal is construction of a 30 kg prototype with sensitivity better than 0.1 recoils per target kg per day. This does not represent a huge scale-up from their large prototype (perhaps a factor or two of 10). Briefly, the prototype results are encouraging and strengthen the case to continue with a larger instrument." The same person later adds: " Since everything comes down in the end to the understanding backgrounds, this whole issue is a big unknown. The proponents are not blind to this, they are after all planning on a big Compton veto system in order to suppress PMT-source backgrounds. And they are of course planning on using low-activity materials, etc. However, the pattern of these experiments is a long struggle at understanding and minimizing backgrounds. I, therefore, do not believe the optimistic schedule presented; it strikes me as incorrectly minimizing the difficulty they will have of reducing backgrounds. I could believe they will take some data, but data at a sensitivity of 0.1 or less per target kg per day I think will come at least a year later than they think, maybe more. They simply won't understand the tails of their background distributions. Briefly, I think the experimenters have underestimated the commissioning difficulty at their target sensitivity. …… overall much too much important detail is missing. The number of milestones is also woefully inadequate; by the time missed milestones add up, the experiment would be very seriously behind. Briefly, the big picture of the project structure (management, institutions, deliverables and funding) is vague and should be clarified before I would feel comfortable supporting this effort. Exactly who is contributing what, in terms of people, time and funding? What are the milestones leading up to delivery of subsystems? How long will it realistically take?"*

A third panelist says: *"The proposal is strong on ideas and plans, but weak on a realistic schedule to complete a working detector. The project appears to be paced by the review process of the U.K. dark matter group. This is not the right way to proceed. Instead, the Zeplin program should be judged as a R&D effort at this stage. A possible schedule would be for the group to continue R&D over a two-year period with the expectation that a significant proposal would be submitted at the end of the R&D phase. The full proposal would more fully address the backgrounds (at the site and from* *detector elements), the achieved discrimination and energy threshold, and would provide a realistic estimate of the expected sensitivity."* The same person adds: *" The science is great. The technique in principle should work. Wang is a solid scientist who should be supported to continue the development of the technique."*

Another member observes: *" The UCLA group proposes to collaborate with the UK group in the Boulby salt mine (UK) on Zeplin-II. This is an excellent idea for several reasons. The UCLA group is very small and they already have a collaborative experience with the UK group on LXe. UCLA appears to have a single very strong person in Dr. Wang. A very aggressive time-scale was proposed to have Zeplin-II as a going experiment in one-year. I believe this is quite unrealistic given the size of the group and the present level of understanding of backgrounds in-situ and their ability to reject them in a scaled up version of their present smaller prototype. The idea is to construct the cryogenic vessel and its field shaping structure at UCLA, ship it to Boulby and install it in the existing active veto shield at Boulby."*

In summary, SAGENAP finds the UCLA work so far to be innovative, excellent and very promising. SAGENAP members feel that the work should continue toward the goal of having a "two-phase"-type LXe experiment searching for dark matter but they are concerned that the proposed schedule is not realistic. They also did not see that the background rejection had yet been sufficiently well established to consider that an experiment would result at that time. They would support the construction of the proposed chamber at UCLA but worry that the UCLA presence and participation in the subsequent experiment has not been sufficiently clarified. SAGENAP would prefer to see this continue in an acknowledged R&D phase until clarification on the questions of backgrounds and longer term participation by the UCLA group occurs.

APPENDIX

1. **AGENDA FOR THIS MEETING.**
2. **MEMBERSHIP OF THE PANEL.**

Scientific Assessment Group for Experiments in Non-Accelerator Physics

SAGENAP Annual Meeting
March 29 – 31, 2000
Gaithersburg, MD

March 29, Wednesday

08:30 AM	Executive Session
09:30	SNAP Presentation
10:30	----Break----
10:45	SNAP Presentation
12:00	---- Lunch----
01:00 PM	ICECUBE Presentation
02:30	----Break----
02:45	ICECUBE Presentation
03:30	SNO Status Report (Hamish Robertson)
03:45	CDMS Status Report (Bernard Sadoulet)
04:00	SDSS Status Report (John Peoples)
04:15	Connections: Quarks to the Cosmos (Rocky Kolb)
04:45	NRC Committee on Physics of the Universe (Mike Turner)
05:00	Executive Session
06:30	Questions to SNAP and ICECUBE (Bob Lanou)
	ADJOURN

SAGENAP Annual Meeting

March 30, Thursday

08:30 AM	Executive Session
09:00	SNAP Answers to Questions/Discussion
09:45	ICECUBE Answers to Questions/Discussion
10:30	----Break----
10:45	EXO Presentation
11:25	Zeplin II Presentation
12:00	----Lunch----
01:00 PM	Milagro Presentation
02:30	----Break----
02:45	Axion Presentation
04:15	----Break----
04:30	Executive Session
06:00	Questions to Milagro, Axion, EXO, Zeplin II (Lanou, et al.)
	ADJOURN

SAGENAP Annual Meeting

March 31, Friday

08:30 AM	Executive Session
09:00	Milagro Answers to Questions/Discussion
09:30	Axion Answers to Questions/Discussion
10:00	EXO Answers to Questions/Discussion
10:15	Zeplin II Answers to Questions/Discussion
10:30	----Break----
10:45	Executive Session
12:30 PM	----Lunch----
01:30	Executive Session
03:00	ADJOURN

SAGENAP MEMBERS
Spring 2000

Professor Hamish Robertson
Department of Physics
University of Washington
P.O. Box 351560
Seattle, WA 98195
Phone: 206-616-2745
Email: rghr@u.washington.edu

Professor Henry Sobel
Department of Physics and Astronomy
University of California Irvine
Irvine, CA 92697-4575
Phone: 949-824-6431
Email: hsobel@uci.edu

Professor Leslie Rosenberg
Laboratory for Nuclear Science
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, MA 02139-4307
Phone: 617-253-7589
Email: ljr@mitlns.mit.edu

Professor Janet Conrad
Fermilab MS 309
Batavia, IL 60510
Phone: 630-840-3266
Email: conrad@fnal.gov

Dr. Steven Ritz
Mail Code 661
NASA/GSFC
Greenbelt, MD 20771
Phone: 301-286-0941
Email: ritz@milkyway.gsfc.nasa.gov

Professor Rene Ong
Enrico Fermi Institute
University of Chicago
5640 South Ellis Avenue
Chicago, IL 60637
Phone: 773-702-7475
Email: rene@hep.uchicago.edu

Professor Bernard Sadoulet
Department of Physics
University of California, Berkeley
Berkeley, CA 94720-7300
Phone: 510-642-5719
Email: sadoulet@physics.berkeley.edu

Professor Robert Lanou
Department of Physics
Brown University
Providence, RI 02912
Phone: 401-863-2632
Email: lanou@physics.brown.edu

Professor James Musser
Department of Physics
Indiana University
Bloomington, IN 47408
Phone: 812-855-9933
Email: musser@bigbang.astro.indiana.edu

Professor Joel Primack
Department of Physics
University of California-Santa Cruz
Santa Cruz, CA 95064
Phone: 831-459-2580
Email: joel@physics.ucsc.edu

Dr. Thomas L. Cline
NASA Goddard Space Flight Center
Mailstop 660.0
Greenbelt, MD 20771
Phone: 301-286-8375
Email: cline@apache.gsfc.nasa.gov

Professor James Stone (Co-Chair)
U.S. Department of Energy
Division of High Energy Physics, SC-221
19901 Germantown Road
Germantown, MD 20874
Phone: 301-903-0535
Email: James.L.Stone@science.doe.gov

Professor Eugene C. Loh (Co-Chair)
National Science Foundation
Particle and Nuclear Astrophysics Program
4201 Wilson Boulevard
Arlington, VA 22230
Phone: 703-306-1895
Email: ecloh@nsf.gov