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SAGENAP Meeting Summary Report

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The Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) met on March 12-14, 2002, in Ballston, VA at the Arlington Hilton, with NSF as the host agency. The group members for this meeting were Janet Conrad (Columbia), Priscilla Cushman (Minnesota), Jordan Goodman (Maryland), Giorgio Gratta (Stanford), Francis Halzen (Wisconsin), James Musser (Indiana), Rene Ong (UCLA), Steven Ritz (Goddard), Hamish Robertson (U. of Washington), Robert Svoboda (Louisiana State), James Yeck (DoE), James Stone (DoE), and Eugene Loh (NSF). The meeting was co-chaired by Eugene Loh and James Stone, with Steven Ritz serving as report coordinator. Janet Conrad was absent from this meeting. Jordan Goodman and Hamish Robertson left the meeting early.

Five proposals were considered at this meeting: XENON, OMNIS, 3M, Super-Kamiokande repair, and Solar Neutrino TPC. Written versions of the proposals were available to members of SAGENAP. The proponents made oral presentations during the meeting (see the Appendix for the agenda), followed by questions and answers with the group members. Individual written reviews of the proposals by SAGENAP members have been provided to the DoE and NSF. At least four individual reviews by different SAGENAP members were written for each proposal. Two additional projects were considered: the Nearby Supernova Factory, based at LBNL, and a Letter of Intent for further work on ICARUS. Status reports from ten ongoing projects (Auger, CDMS II, Drift, EXO R&D, HiRes, KamLAND, Milagro, SNAP R&D, STACEE, and Whipple/VERITAS) were also presented. Each of these projects was asked to provide a one-page written status summary. In total, seventeen projects at various stages of development or operation were presented to SAGENAP at this meeting.

This report summarizes the meeting discussions and individual reviews of each of the seven proposals, along with summaries of the status reports for the ten ongoing projects. It presents a balanced summary of the conclusions of the SAGENAP membership.

XENON

The XENON proposal is for R&D toward the eventual design of a large (1 ton) liquid xenon dark matter detector. The proposed work would proceed over a period of two years, and would involve groups at Columbia, Rice, Princeton, Livermore, and Brown. The use of liquid xenon for dark matter detection is already being explored by others in the U.S. through participation in the ZEPLIN collaboration. The technique cleverly exploits the good ionization and scintillation properties of LXe coupled with the different ratio of responses for recoil-like and gamma-ray-like (background) events.

The major goals of the proposal are the construction and characterization of a 7 kg prototype module and a complete design for a 100 kg module. The group is currently carrying competing readout schemes that will be investigated as part of the proposed R&D. If the prototype is successful, the next step would the construction of the first 100 kg detector. The longer-term plan is to construct more 100 kg modules to reach a total of one ton. In its full size the detector will have a sensitivity of about 10^{-46} cm^2 in three years of operations, assuming that the projected backgrounds are indeed verified. This would be more than an order of magnitude better than CDMSII, probing substantially deeper into relevant and interesting SUSY parameter space.

Members of SAGENAP were uniformly very enthusiastic about the science case for such an R&D effort. One member wrote, "*The science that XENON is pursuing is first-rate. The question of dark matter, perhaps the most outstanding problem in astrophysics and cosmology during the last two decades, combined with the origin of electroweak symmetry breaking, the most outstanding problem in particle physics over the same time period, present an unbeatable science motivation. Even after 10 years of hard work, we have only just begun to probe the important range of parameter space in the search for the neutralino as a component of the galactic dark matter. The possible discovery of SUSY in accelerator experiments at the Tevatron or the LHC will probably not reduce the importance of looking for WIMPs, but instead very likely strengthen the need to find it, depending on what form SUSY takes. Thus, the science questions addressed by XENON are crucial ones facing modern physics and astronomy, and they urgently need addressing during this decade. The idea of using a liquid Xe detector for dark mater searches has been around for some time and is also being developed by other groups. The idea is very good, and it has been generally acknowledged that the major questions are technical ones (i.e., not related to fundamental limitations of the method).*" The member continued, "*…the attractiveness of Xe has markedly increased….as people begin to address the problems of scaling moderate sized detectors to the large (100's of kg) detectors required to carry out very sensitive searches for dark matter. Thus, it appears to be a very sensible time for the U.S. to ramp up the R&D on Xe dark matter detectors.*" Another member concurred, writing, "*I find the physics motivation for the experiment very compelling, indeed the detection of dark matter is a very central topic in today's particle- and astro- physics. The initial searches using existing technologies (mostly NaI(Tl) and Germanium) have now been superseded by detectors, such as CDMS, that were designed specifically for the task. While it took some time to bring these new techniques to life, these efforts have now been rewarded. It is likely that a* *substantial "upgrade" of these techniques will be needed to move the field beyond the sensitivities of CDMS 2 at Soudan. Fiducial masses of order a ton will be needed and cryogenic detectors will face substantial engineering challenges. It is then sensible to also explore alternative techniques and liquid xenon has been considered a good candidate since some time.*" A third member wrote, "*This detector is clearly a contender in the race for the next generation large device.*"

There was similar enthusiasm for the capabilities of the proponents. One member wrote, "*The group unquestionably has the expertise to build such a prototype device. Professor Aprile and her colleagues have built similar detectors for balloon experiments and have an excellent record of accomplishment……I am impressed with the breadth of experience in the group*." Another wrote, "*I think the Columbia group is ideally suited to the job. Dr. Aprile's group is a leader in the field of liquid xenon calorimetry and their expertise will maximize the chances of success of the program. The remaining part of the team, mainly composed of young and enthusiastic investigators, may also work very well and complement the strengths of the main group.*"

While SAGENAP members were very impressed with the capabilities of the proponents, there were widespread concerns about the realism of the proposed research plan. In particular, many members questioned the availability of the collaborators to do the work according to the very aggressive schedule. One member wrote, "*However I am somewhat concerned that some of the investigators may be somewhat over committed*." Another member concurred, "*I am especially concerned given the other commitments that the PI and other scientists have*." That member continued, "*I don't think this proposal should be funded as is. I would like to see a much larger commitment of the senior personnel. The strength of this proposal is their expertise while the weakness is their lack of time to devote to it due to other (equally important) commitments. This is a truly ambitious proposal that as near as I can see has only one senior person and one post-doc fully devoted to it. I am aware of the substantial commitments of the PI to the NASA program and I worry that this is a second major project that, while similar in technology, will require significant attention if it is to succeed. I am also concerned about the strength of commitment at the other institutions like Princeton and LLNL*." A third member wrote, "*The real question is – who will do the work?*" The member continued, "*The total amount of senior investigator time for the entire collaboration totals only 14 months in the first year and 11 months in the second year. The best, and most experienced faculty members….are only peripherally involved. The overall PI, Aprile, is also the PI of another challenging effort – the development of an advanced Compton telescope. It is hard to imagine simultaneously carrying out two challenging R&D projects. The possible solution to the FTE question may involve several things: a slightly slower approach that allows training of a couple of postdocs, the addition of another group (or a higher level of commitment by existing groups), and a clear statement from the PI as to which effort will be her main focus.*" A fourth member added, "*…the other institutions on the experiment are represented by only one person. Each of them is a major player in another experiment. These are people who are major players precisely because they are excellent physicists, yet the worry is that with their other commitments there is simply not enough manpower to be able to finish this* *project….in 2 years*." One member summarized, "*I think the group involved should be ready to make some hard choices, or simply stagger the different programs in time*."

Another concern shared by a number of SAGENAP members is the issue of backgrounds, which is critical for this measurement. Although investigation of low-background material is part of the proposed R&D plan, the plan does not feature the actual use of these materials in construction of the 7kg prototype. Construction of a detector that realizes the full potential of LXe is difficult enough without the added constraint of low-activity materials. Thus, while the goal of the R&D program to produce a prototype 7kg module is strongly supported, members of SAGENAP suggested that use of low-background materials must be included in the design from the very beginning of the R&D to demonstrate the overall feasibility of this approach. One member wrote, *"….all their choices in selecting R&D directions are very appropriate….I would like to comment, however, on the need in this case to include in the R&D the need for low radioactive backgrounds when designing the detector. In many cases this issue is not decoupled from the rest of the design…some of the construction materials discussed (indium, aluminum, ceramics) are usually rather hot and difficult or impossible to obtain in low-activity versions. I think the group should either avoid using these materials already in the prototype or demonstrate that they are available in a low-background version.*" The member added, "*I strongly feel the prototype chamber should already be a low background detector and success will be defined by a working prototype with backgrounds low enough for a full size experiment.*"

Doing a convincing design to achieve the required background level is again tied to the aggressive nature of the work plan. One SAGENAP member wrote, "*The timetable for the project is not commensurate with the inherent challenges. For example, PMTs are generally considered to be very dirty – a concentrated effort will be required to ensure that they are clean and possibly to develop an alternate photon detection technique. The investigators have clearly understood this problem, but have not devoted enough time (and people) to really pursue the very attractive alternate approaches. The GEMs look very promising, but are being attacked solely by a just-starting faculty member (Oberlack) at 1 month/year. Similarly with the LAAPDs (Gaitskill). It is doubtful that the required research, testing, refinement, and retesting of these devices can be accomplished in the timetable proposed, given the personnel devoted to these tasks at the present time.*" On this issue of investigating the readout choices in the context of background considerations, another member agreed "*this is an area likely to fall behind in an aggressive schedule and cost more than expected*."

A third member agreed, "*Another major concern is the backgrounds. The key to success of the full detector is getting the backgrounds down. Many an experiment has failed to reach its design sensitivity because of an unanticipated background. This prototype will not add significantly to the understanding of the backgrounds since it will be at the surface (and not very large). I would feel much better about the project if the prototype were to be constructed in as low a background way as possible and tested underground where the assumptions of the proposal can be tested*." A fourth member concurred, "*In general, I think the prototype effort is strongly focused on the LXe detector* *design without enough consideration given to crucial backgrounds. The stated goal of 10^{-5} kg^{-1}day^{-1}keV^{-1} for the full detector (after a factor 200 rejection) is about an order of magnitude greater than what is claimed for ZEPLIN III. How will the prototype answer the question of whether or not backgrounds can reach the required levels?*" The member continued, "*It would be prudent to operate the prototype in a clean, underground environment until background levels are understood and shown to be under control before moving on to a ten times bigger module, where changes are likely to be much more costly and difficult.*" A fifth member stated, "*Since their goal is to produce an operating LXe detector that could actually do some physics in an underground site, it would be worthwhile to plan on radiopurity from the start. This is perhaps the weakness of the group, since this is not an issue that LXeGRIT has to worry about in its balloon flights.*" The member concluded, "*It is disingenuous to suggest that a 100kg module simply scaled from the 10 kg could reach a better sensitivity than CDMS II at the end of a 3-year development period without addressing this issue.*"

SAGENAP members recognized that the driver behind the aggressive schedule is the competition with other LXe dark matter experiments. As one member wrote, "*The time scale is driven by the need to compete favorably with other LXe approaches, since ZEPLIN II and III are about to take data in the Boulby mine and the 1 kg XMASS detector is also underground now. If their approach is to be the nucleus of the ton-scale world LXe detector, they need to show its advantages soon.*". This competition is a delicate matter for other important reasons, however, since it involves other U.S.-funded groups. Disproportionate funding of R&D efforts might skew the results of the competition, be fundamentally unfair, and not allow the best design to emerge. A member contemplated, "*Since the U.S. is already supporting experiments in the U.S. and is contributing to the Boulby effort, should an independent effort be started to parallel research on LXe? I think as far as the prototype stage is concerned the answer is 'yes', as the group here has some innovative and independent ideas that could significantly improve backgrounds.* Another member added that, "*…at the end of the R&D and if the technique proves successful, the two groups will need to converge on some sort of common plan. But I think it is healthy to have two parallel developments at this stage.*" SAGENAP members felt that the competition resulting from worldwide interest in LXe dark matter detectors should eventually help form a critical-mass collaboration. One member wrote, "*Clearly the final tonne scale detector (and probably the 100kg one as well) will require a major collaboration. This group should be actively seeking to join forces with others to form such a collaboration. They could add immensely with their expertise and they still might be able to fulfill their other commitments.*" Another wrote, "*…cooperation should be encouraged as the final step (1000 kg) is likely to be quite expensive and require many more people to build and operate than the small detectors built to date.*" The member added, "*At the prototype stages, their approach is significantly different from the ZEPLIN design to justify a separate study*."

Despite the major concerns of schedule realism and key personnel time commitments, there was uniform support for an R&D effort by this group, but only if the concerns are adequately addressed. One member concluded, "*My overall evaluation is that this is a design that shows great promise to attack a very important problem. I* *believe the group has the expertise to design and build a prototype of this kind. I am just not convinced they have a good plan for how to proceed…I am confident they can do it, but I think they need to move more deliberately.*" Another member wrote, "*This project is too good to turn down, despite manpower questions.*" A third wrote, "*In conclusion, I would like to see this proposal funded. The physics is first rate, the technique is promising and the PI is one of the best people available to do the job.*" Another summarized that, "*XENON is a very promising approach that deserves R&D funding. The difficulties come from technical and manpower issues associated with the overall challenging nature of the project, the rushed timetable, and the inadequate number of completely committed investigators to carry out the project".* The same member added, *"I find the science and technique of XENON compelling, but that the project as laid out in the proposal is too thin on senior management and somewhat unrealistic in schedule to proceed at the present time. I would encourage the group to reformulate these management issues for a review to be organized by the NSF."*

OMNIS

The Observatory for Multi-flavor Neutrinos from Supernovae (OMNIS) is intended as a laboratory for studying the ν_μ, ν_τ, and high-energy ν_e emission from type II supernovae. The technique used in OMNIS involves the detection of neutrons produced in the charged and neutral current interactions of these neutrinos with the rock surrounding the detector or the passive target material (either lead or iron) which constitutes the majority of the detector mass. In the case of a lead-based OMNIS detector, the group proposes in addition to use the measurement of the ratio of one-neutron to two-neutron events as a means to verify the presence of neutrino mixing. Since SN rates are low (it could be many decades between supernovae in our galaxy), it is essential to design an experiment that requires very little maintenance over a very long period of time and that is inexpensive to operate. Indeed, simplicity and expense minimization are the prime motivations for basing the detector principle on neutron detection instead of one that would provide some form of spectroscopy.

The expected event rates resulting from a supernova at 8 kiloparsecs (kpc) are roughly 2000 muon and tau neutrinos in a 4000 ton lead detector, and 300 events of this type in an 8000 ton iron detector. In addition, a lead-based detector will observe roughly 8000 electron neutrino-induced events in the case of MSW mixing, and 400 such events in the absence of mixing. It should be noted that the majority of the observed neutrons are produced from excited states near the neutron production threshold, resulting in large uncertainties in these neutron flux predictions.

The ultimate goal of the OMNIS project is the deployment of multi kiloton lead and/or iron-based detectors in the WIPP facility. The proponents estimate that the hardware cost for the ultimate 2kTon OMNIS would be about $8M.

The proposal submitted for consideration by SAGENAP describes a two-year R&D program centered on the optimization of detector parameters and development of the neutron detector technology for OMNIS. The collaboration proposing this R&D program includes members from UCLA and UT Dallas, a subset of the groups ultimately interested in the OMNIS project. The requested level of support is about $361k. The proposed program will include Monte Carlo detector optimization studies, optimization of the optical readout system, and light yield and aging studies of the active detectors. In particular, the proposed R&D program evaluates detector options that use either ^{10}B or ^{6}LiF and a light readout scheme adapted from the MINOS technology. Monte Carlo optimization studies will be carried out for a first 500 ton Lead OMNIS module using the proposed detector options along with moderator materials.

The proposed R&D program has the following objectives:

<u>Year 1</u>

- Constructing a working prototype detector consisting of a 1.5m x 0.5m x 0.03m module with three detection planes (Li, ZnS, plastic) with approximately 130m of wavelength-shifting fibers, and associated test stand;
- Performing detailed Monte Carlo calculations of SN signals in the detectors;

- Optimizing the geometric allocations of the neutron detectors;
- Testing alternative neutron detectors and *in situ* study of background;
- Developing and testing extremely stable neutron counter design; and,
- Developing and testing the alternative neutrino detector technologies.

Year 2

- Testing the new neutron detectors at a cyclotron and at WIPP;
- Measuring background levels; and,
- Designing the first version of the OMNIS detector.

A report containing the simulation results, hardware prototype performance, and *in situ* measurements would be produced at the end of year 2. The co-spokespersons for this research effort are Kevin Lee (UCLA) and William Burgett (UTD). The spokespersons for the overall OMNIS project are Richard Boyd (OSU) and David Cline (UCLA).

SAGENAP members noted the single-purpose nature of the experiment -- it is unfortunate that there is not much physics that can be done between occurrences of supernovae -- but many members were also quite positive about the science that might be achieved. One member wrote, "*It is now quite clear that neutrinos play a central role in the supernova explosion mechanism, and measurements of the time evolution of neutrino emission in the various neutrino species would be tremendously illuminating*." Another added, "*Such a detector has a place in the scientific program of the next half century since it is complementary to Super-K, observing the time spectrum of ν_μ and ν_τ from a galactic supernova…rather than mainly anti-ν_e….the physics can certainly be supported on the supernova work alone.*" A third member concurred, "*It seems that there is sufficient scientific motivation to design an experiment that will have a very long lifetime, low construction and operating costs, and sensitivity to both NC and CC interactions and the ability to separate them. Although there are several experiments now operating that can be expected to last a decade or longer, it may be necessary to wait a very long time for the next SN. In addition, it is desirable to view the SN through several different aspects through the earth in order to measure the effect of traversing different amounts and densities of matter. Currently, the U.S. has no neutrino detectors capable of any significant SN measurement.*"

Although SAGENAP members were convinced by both the science case and the overall approach, there were some concerns about the technical and programmatic components of the current proposal.

The first technical concern had to do with the neutron yield uncertainties, along with the ability of the detectors to distinguish different event types. One member wrote, "*The proposal justifies the study of new neutron-sensitive detectors by referring to the 2-neutron spallation channel in ^{208}Pb. This idea comes from a paper by Fuller, Haxton and McLaughlin. The proponents….did not provide us with any proof that the principle itself is sound.*" The member added, "*the cross-section calculations are complicated since the states are close to the neutron production threshold and can be off by a factor of two.*" Another member agreed, "*The one and two neutron production cross-sections in these low energy neutrino interactions are subject to significant theoretical uncertainty. A measurement of these cross-sections is not in the OMNIS R&D plan proposed here, but it* *seems that it should be. Their claim that the time profile of 1 and 2 neutron events can be used as an indication of the evolution of neutrino energy with time rests on the assumption that these cross-sections are relatively well known – this has not been demonstrated. I would not recommend the OMNIS project proceed much further until they obtain through measurements a better handle on these critical cross sections.*" A third member agreed, "*It is crucial to know the uncertainties in the cross-sections for both NC and CC to interpret OMNIS data*", adding that "*the cross-section uncertainties do not completely cancel in the ratio 1n/2n since the thresholds and energy dependence for these processes are significantly different*". The member continued, "*Therefore, it would seem highly advisable to measure the cross-section for 1n and 2n spallation averaged over a neutrino spectrum similar to that expected from SNe in order to have more confidence in the interpretation of the OMNIS data. A spallation neutron source with short spill time would be an ideal location. In fact, it wouldn't really make sense to build OMNIS without a parallel program to do this measurement at some point.*"

The second technical concern was that of aging of the detector over the timescales necessary to operate the experiment. Although aging studies are part of the proposed R&D plan, some SAGENAP members warned that this warrants more attention than it was paid in the presentations. One member wrote, "*Measurements by the MINOS collaboration on the scintillator being contemplated for use by the OMNIS group indicate that the light yield drops by 30-50% over a 25 year period. It is not clear from the OMIS proposal that this level of light yield reduction would be acceptable. Loading the scintillator with Gd, one of the options under consideration by the OMNIS group to increase neutron sensitivity, would undoubtedly accelerate the aging process…. Aging studies should be a focal point of any OMNIS R&D plan.*" The same member continued, "*Although the OMNIS R&D proposal states that the MINOS group at Caltech will help with these aging studies, …the OMNIS group should verify that they will have the support they need from the Caltech group.*" Another member agreed: "*Lifetime tests similar to those performed by the MINOS group would be used to characterize the aging properties of the sandwich. Many issues regarding aging need to be addressed more carefully. The MINOS scintillator went through an intense 3-year development period before it could pass the aging requirements for a 10 year, much less a 50 year, experiment. Contaminants…all contributed to unacceptable degradation. In addition, the MINOS accelerated aging studies depend on a linear relationship between heat and exposure years. This is not yet proven for their 10 years and is most likely not scalable to 50 years. Presently MINOS heats their scintillator to achieve one MINOS year in a few months. At this rate, the OMNIS tests must either monitor their detectors for 6 years at the MINOS temperature or crank up the temperature to achieve a few month test. It should also be recognized that the MINOS scintillator, in its final form and subjected to the assumed equivalent of 10 years of aging, is degraded to half its original performance. A 50-year experiment will require MUCH BETTER scintillator aging performance than MINOS was able to achieve. It might therefore make more sense to look into cheap, replaceable components…. Purification may also be necessary.*"

There were two main programmatic concerns. The first is the relationship between this proposal and the other OMNIS efforts. Listed in the presentations were

UCLA, UTD, WIPP, LANL/Carlsbad, North Carolina State University, and FNAL, which, the proponents told SAGENAP, integrates to approximately 2/3 of the people interested in OMNIS. Other OMNIS groups include Ohio State, ANL, LANL, and New Mexico State University. One SAGENAP member commented, "*Several institutions identified with OMNIS, including the co-spokesman, were not on this proposal since they are interested in pursuing a different R&D direction. This leaves us in the unpalatable position of choosing the technology direction for a not-yet-approved experiment simply because one faction chose to present their option before this group and the other declined.*" Another member observed, "*In order to be effective, a formal OMNIS collaboration should be formed committed to build a single detector. Optimization of the various components should not proceed piecemeal, as other techniques could have significant impact on the optimization of the other detector components. Milestones should be established on the road to the final detector design to make sure the collaboration converges to a single plan and that R&D costs are obvious and manageable.*" The member concluded, "*If no single OMNIS collaboration is formed and there are multiple efforts, then an external review process to determine which avenue to pursue would be advisable, as the motivation for more than one OMNIS-like detector in the U.S. is less convincing from a scientific point of view.*"

A closely related concern is the overall guidance of the R&D effort. As already noted, the OMNIS spokespersons are Cline (UCLA) and Boyd (OSU) but they are not the PI's on this R&D proposal. One SAGENAP member stated, "*The spokespersons should take responsibility…for the R&D effort rather than a third person.*" Another member added, "*Considering the level of other commitments on the part of the senior UCLA staff, Dr. Lee will be very much on his own here.*" An underlying concern is the relationship between the R&D program and the ultimate goal of producing the OMNIS facility. If successful, this preliminary R&D would presumably be followed by proposals for a pre-construction program of appropriate scale and, eventually, the full OMNIS project. One SAGENAP member commented, "*The scope of the R&D program will contribute additional information that is relevant to the OMINS detector, but it is not clear that the program is adequate or is directly relevant to the expected pre-construction activities. The presentations…did not provide a clear picture of the status of the detector planning, the current construction schedule, or the needs for an R&D program.*" That member concluded, "*The OMNIS R&D is worthy of support, but it would be easier to support if this program was directly tied to an existing pre-construction design and engineering program. Until this is done, one should assume that construction of the detector is still a long way off.*"

The second programmatic concern voiced by several members of SAGENAP was the viability of the WIPP site. This experiment fundamentally has a much longer time horizon than any other experiment in all of HEP. One member warned, "*WIPP is a national defense related site and its openness to non-US physicists is something that can change. More importantly, there would be benefits to a site that has additional operating or planned experiments.*" Another wrote, "*Compared to other possible underground sites, WIPP is stated to be preferable because of the positive political implications of using a nuclear waste repository for a scientific goal and because it is likely to be* *maintained for the duration of the project, which might be 50 years or more. After the Hi-Res experience with Dugway, one should perhaps be more cautious about this!*"

In summary, SAGENAP members were convinced by the physics case and the overall OMNIS concept that R&D in some form toward a possible full-scale detector is worth pursuing. There were significant technical and programmatic concerns about the present proposal, as outlined above. A representative sentiment was expressed by one SAGENAP member, who wrote: "*I support a modest R&D program for OMNIS centered around detector optimization, aging, and a measurement of the interaction cross-sections which lie at the heart of the technique.*"

3M

This proposal from Al Mann and Ken Lande describes the beginning of an extremely ambitious program to be carried out at a National Underground Science Laboratory (NUSL) in the Homestake mine. The detector would consist of an array of ten water-Cherenkov detectors, each of 100 kilotons (50m diam × 50m high in volume). When completed the total detector would be a megaton. With a detector of this size many important physics goals would be within reach. Even with the first module many important results could be obtained. Specifically, as stated in the proposal, the physics goals are:

- Proton Decay – The proposal claims the detector when finished would have a reach to 10^{35} years. This is an extremely important physics goal as many models of supersymmetry and grand unification predict that nucleon decay will be observed by this level. However, the proposal does not go into details on which modes would be rate limited and which would be background limited.
- Long-Baseline Neutrino Oscillations and CP Violation in the Neutrino Sector – This proposal describes a program to send a beam from BNL to the NUSL site. The long distance (2540Km) provides the opportunity to see a clear oscillation signature by observing multiple nulls in the neutrino signal. This signal could be observed with the first module after a few years. Also by studying the difference between $\nu_\mu \to \nu_e$ and the antiparticles that CP violation could potentially be seen in the neutrino sector. The proposal does not give details on how the potential reach of this experiment compares to the already operating or under-construction long baseline experiments.
- Supernova neutrinos – The proposal describes how one could use the multiple detectors (by adding salt to one of them) to look at both the antineutrinos and the neutrinos that come from neutronization.
- UHE neutrinos from space - This detector, when completed, would be a megaton. This could potentially see some neutrinos from astrophysical sources. At completion it would have a geometric factor between one and two orders of magnitude smaller than that of IceCube.

The presentations at the meeting focused almost entirely on the long baseline neutrino oscillation experiments.

The proposal is for funding for a program make a detailed cost estimate for a module in Homestake. The current construction cost estimate is about $45M per module for 7.5% photocathode coverage.

The proponents are well known in the physics community and have a long and excellent record of achievement. As one member wrote, reflecting the views of many, "*The PI's both have long and distinguished careers in this field and are highly capable of being major players in a new experimental program. They both have been pioneers in the field*."

On a general level, SAGENAP members strongly agreed the physics that could be done with a large detector at an underground facility is potentially of great interest. As one member noted, "*Other topics addressable by 3M, including proton decay, atmospheric and solar neutrino observations…remain exciting areas for future work.*" Another member wrote, "*The case for an underground laboratory hosting a 'flagship' detector of the type described in this proposal is, in this reviewer's opinion, very attractive and scientifically compelling*". Other members expressed similar levels of interest, but also felt that further details would be necessary to make the science case compelling.

SAGENAP members were less convinced by the case for the long baseline neutrino oscillation component of the experiment, and they noted that this also falls largely outside the purview of SAGENAP. One reviewer wrote, "*At the SAGENAP review the PIs chose to only talk about one aspect of the science proposal – the very long baseline aspect. This was a strange choice given that this is intrinsically an accelerator experiment and SAGENAP is a group that evaluates Non Accelerator Physics (NAP). Even so, a clear case was not made as to why such a large detector was needed for this aspect of the experiment.*" Another member agreed, "*The case made for observing the L/E structure characteristic of atmospheric oscillations in a long baseline experiment is weak. (SAGENAP) was presented with the possibility of making a similar measurement with the refurbished SuperK detector in the atmospheric beam for a very moderate investment.*" A third simply stated, "*This work falls outside the scope of SAGENAP's charge, and it is not appropriate for SAGENAP to comment on this aspect of 3M science*." That member continued, "*The very low photocathode coverage in the proposed detectors is clearly optimized for the long baseline component of the 3M science program. However, the adverse effect of such minimal coverage on other types of studies….has not been presented. Therefore, a complete review of the science capabilities of 3M is impossible….*". A fourth member added, "*The proposed PMT coverage would not make 3M as good a SN detector as Super-Kamiokande*". There were likewise concerns about the political reality and technical aspects of the proposed Brookhaven neutrino beam, which might cost around $50M.

The organizational and programmatic aspects of the proposal caused the most concern among SAGENAP members. There was a strong belief that the efforts by this group alone are sub-critical and are not sufficiently coordinated with similar efforts by others in the community. One member wrote, "*There is no collaboration. Adequate scientific and technical personnel and superb management will be required to deliver this challenging project. There is insufficient information to judge this critical aspect of the project.*" Another wrote, "*3M is one of several proposed large megaton-scale detectors. UNO and Hyper-Kamiokande are two others that have attracted much attention and many bright young physicists.*" A third member wrote, "*There is now a rather large community of physicists with years of experience in this area, and it is important as well that this pool of knowledge and talent be fully exploited. It is clear to me that the 3M detector, as proposed, does not satisfy these conditions. I believe that the 3M group should be discouraged from proceeding along the path they are now following*." A fourth member wrote, "*The proposal puts forward a vision that is perhaps more appropriate for a conference report rather than an NSF proposal*." That member continued, "*I would be happy if the NSF supports at some level their continued efforts in* *this direction, but even given the modest request the PIs are making this proposal is way too ambitious to be given serious consideration for funding as is*." Another member remarked, "*There is nothing substantial enough here to approve or disapprove*."

In summary, SAGENAP members agreed that, despite problems with this particular proposal, the physics that can be done with a large detector at an underground laboratory – neutrino oscillations, proton decay, and several important topics in neutrino astrophysics – is well worth pursuing in a manner that incorporates the talents and interests of the community. As one member wrote, "*In general, given the scope of the detector and possible beam, I would recommend that the 3M group try to cooperate with the other groups seeking to establish a large underground detector program for the U.S. They have an invaluable contribution to make due to their experience and familiarity with the Homestake site and can in return get a boost from other researchers familiar with construction of large neutrino experiments underground*." Once this cooperation has been more formally established, the physics case has been worked out in more detail, and the detector concept has been optimized for those physics priorities, the proposing groups should return to SAGENAP for a review, according to the procedure for new large-scale projects outlined in the recent HEPAP subpanel report.

Super-Kamiokande Repair

This proposal requests funding to repair the outer detector of SuperKamiokande (SuperK), following the accident of November 12, 2001. As discussed in the proposal, the accident was triggered by the implosion of one of the 20'' PMTs at the bottom of the detector, and the subsequent shock wave destroyed 6,777 out of 11,146 20" PMT's in the central detector. The reverberating shock wave also destroyed 1,160 out of 1885 8" PMTs in the outer veto, with glass fragments seriously damaging the light-proof membrane separating inner from outer detectors and the wavelength shifters used to increase the light collection efficiency of the outer detector. This catastrophic failure followed the first repair of the detector, carried out in the Summer 2001 to replace a number of malfunctioning PMTs.

The proposal extensively and clearly discusses the mechanism of the accident, quoting a detailed *post-mortem* carried-on by a special committee appointed by the University of Tokyo, the leading institution in SuperK. While some conjectures are advanced to explain the failure of the first PMT, the proposal correctly concludes that the structural failure of at least one PMT in a detector containing over 10,000 is essentially unavoidable, and the detector should have been designed in such a way that the implosion of one PMT could not trigger more implosions. Indeed the collaboration has now developed a very effective encapsulation shell that will slow down the water rushing to a tube in case of failure, damping the shock wave to a level that is harmless to the neighboring tubes. The collaboration has decided to rebuild the detector in two phases:

1. Redistribute the remaining central detector 20" PMTs, obtaining about 50% of the original photocathode density in the central detector, and replace with new tubes *all* the broken PMTs in the veto
2. Order about 7,000 20" PMTs and install them in a shut-down as soon as they become available.

This staged approach is due to the long lead-time needed to procure the 20" PMTs and the necessity to restart data taking as soon as possible at least for supernova detection and for the K2K experiment. In both phases, all the 20" tubes would have the damping encapsulation discussed (no encapsulation is needed for the smaller veto detector PMTs). The US groups are traditionally responsible for the veto counter and are requesting $2.1M to replace the 8-inch tubes ($24M in funding is requested from sources in Japan to reconstruct the inner detector). Full reconstruction is necessary because of its critical role in the K2K long baseline experiment. Funding on a short schedule is critical because the KEK lab that provides the neutrino beam is being phased out with a reduction of 20% of operating funds every year after 2003 to build the JHF. For 2003, KEK will extend the run from 6 months to one year for SuperK. Thus, any delays in the repair will result in lost beam time.

SAGENAP members uniformly agree the science output from SuperK has been superb. As one member wrote, "*it is this reviewer's opinion that SK has to be regarded as an astonishing success. SK produced more physics than any other experiment in particle physics in the last 10 years and a large fraction of such physics has turned out to be unexpected. Indeed it is fair to say that the only clear indications we have of physics* *beyond the standard model come today from neutrino oscillations and SK is the single most important experiment behind them.*"

The main scientific issue of the present proposal is whether continued operation of SuperK in its repaired configuration is worthwhile. SAGENAP members enthusiastically agreed it certainly would be. In particular, the factor of two in statistics for the K2K long baseline experiment will increase the present 2.5σ neutrino oscillation significance substantially; the ν_τ appearance from the atmospheric flux could become significant, along with a possible observation of the oscillations as a function of L/E; sensitivity to theoretically interesting proton decay channels will improve significantly; and SuperK will still be the world's premier detector for supernova neutrinos. As one SAGENAP member wrote, "*Although this is a historic experiment that has undoubtedly already generated a Nobel, the question should be addressed if reconstruction is justified….there can be no doubt that the answer to this question is definitely yes*". Another member wrote, "*The physics case for repairing SuperK is very strong, essentially overwhelming. SuperK will clearly be the premier experiment in three major science areas over the next decade: neutrino oscillations, proton decay, and supernova neutrino detection.*" For both proton decay and supernova neutrino detection, the member continued, "*SuperK is essentially the only game in town for these two high-profile science topics.*" A third member agreed, adding, "*While a large chapter of SK physics is concluded, there are at least two very important experiments that are left unfinished. SK was the largest detector of neutrinos from supernovae and its contribution in this arena is essential, not only for their own sake, but also in combination with the other supernovae detectors around the world. While some of these detectors take advantage of different types of interactions to pinpoint the property of supernovae neutrinos, SK being the largest detector (by far) was counted-upon for accurate timing and flux normalization. A supernova explosion with no SK data would substantially reduce the effectiveness of other detectors. In addition, as extensively discussed in the proposal, the K2K experiment seems at present to hint to oscillations, in the region of parameters indicated by the atmospheric neutrino anomaly. Yet the significance of this observation is definitely too weak and substantially more data is needed. While better experiments will eventually come on-line for this purpose, it appears to us that SK may still have a chance if they manage to re-build in the time scale discussed. The physics reach of the partially re-instrumented detector is surprisingly good".* A fourth member joined the chorus of support: "*The K2K program is vital to disentangling the neutrino oscillation question. It is still not understood why the allowed regions from Kamiokande and Super-K just barely agree. The K2K near detector data combined with beam-ν_μ disappearance and atmospheric data provides an important crosscheck on systematics. With only half of the anticipated statistics, another year represents a sizeable proportion of the program. It will improve confidence in Super-K and inform on the upcoming MINOS result as well. In fact, it could also be argued that the JHF project itself also depends on the best understanding of the efficiencies and backgrounds validated and understood in the interplay between Super-K and the K2K near detectors. Positive proof of oscillations from ν_μ requires an analysis sensitive to energy-dependent spectral distortion, but at present, the spectral analysis is statistics limited. Stopping now would give only a 2 σ significance for the single-ring quasi-elastic spectral analysis compared to 3-5 σ after another year of running.* On the issue of the impact on the reduced photocathode coverage, that member continued: "*Only the very low energy physics will suffer, with the solar neutrino threshold moving from 4 MeV to 6.5 MeV and degradation in one of the proton decay channels ($p \rightarrow \nu K^+$, $K^+ \rightarrow \mu^+ \nu$) which relies on background rejection via the 6 MeV gamma tag from $N^* \rightarrow N$. However, they will still be able to resolve the oscillation minimum for quasi-vacuum solutions with enough statistics, provided they can supplement the degraded energy resolution with some independent system. Since they are installing a new system of in situ lasers to determine the scattering and absorption as a function of position, wavelength, and direction, they will be able to restore the energy calibration to former levels and do a good job on this physics as well.*

SAGENAP members were equally well convinced that the team has understood the cause of the accident, and that an appropriate and reliable plan is in place for preventing a recurrence. As one member wrote, "*The work done since the accident has been phenomenal. A clear understanding of the basic mechanism has been achieved and small-scale experimental tests at several depths have both verified the phenomenon and tested solutions. The acrylic container solution is conservative and assured of success.*" Another member wrote, "*SuperK has done an outstanding job of understanding the nature of the accident and of ensuring that such an accident is very unlikely to occur in a refurbished detector.*"

While SAGENAP members shared with the proponents their enthusiasm for proceeding quickly, there was some concern about some of the details of the manpower and management of the repair. One member wrote, "*The U.S. contribution to the repair job is sensible and appropriate. It will allow U.S. groups to continue to participate in this flagship experiment and reap the future scientific benefits. There are only a few small concerns regarding this proposal – they are related to the relatively sketchy information provided about the actual plan for the repair.*" The member added, "*Here we will assume that there will be a relatively quick and moderate-scale technical review of the repair plan. These issues will presumably be resolved at that time.*"

In summary, SAGENAP members strongly endorsed the SuperK repair proposal, and agreed with the need to move forward as quickly as responsibly possible. The future science prospects easily justify the resources. As one member wrote, "*The $2M being asked for this proposal is a one-shot request which is again less than 1/10 of the total repair cost…and roughly equal to one year of US-SuperK operating funds.*" That member concluded, "*If the next galactic supernova occurs before 2007, everyone will agree that not funding SuperK repair will have been a very great mistake*." Another member summarized well the feelings of many members, stating, "*Because of all these reasons the funding of $2.1M to the US groups must be one of the historic bargains in science funding. I simply cannot see any reason not to fund this request with the highest priority*."

Solar Neutrino TPC

This is a proposal for research and development work toward the construction of a ten cubic meter prototype helium-methane, high-pressure Time Projection Chamber (TPC) to be located at an underground site. The purpose of the proposed research is to test the technical design choices with the prototype to enable the ultimate construction of a very large (4000 to 6000 cubic meters) TPC to study solar neutrinos to very low energy threshold (~100 keV). Such an energy threshold would be low enough to detect the pp and ^{7}Be neutrinos that compose about 99% of the flux from the sun. A measurement with a TPC would enable extraction of the neutrino energy spectrum via reconstruction of the kinetic energy and angle of the recoil electron from ν-e elastic scattering. A significant improvement in background rejection (factor of 10) would also be achieved by using the solar direction as a constraint. In addition, systematic errors would be reduced by a precise measurement of the background in the anti-solar direction.

The major motivation for such an experiment is the fact that previous radiochemical experiments (SAGE and Gallex/GNO) measured only an integral reaction rate in this energy range and could not separate out the neutrinos emerging from various processes in the sun. In addition, current experiments to measure the low-energy solar neutrinos (Borexino and KamLAND) have no directional information (and thus have significant backgrounds) and will not have a low enough threshold to detect pp neutrinos. The scientific payoff of a successful measurement of the very low energy solar neutrinos would be: (1) a compelling observation of neutrino oscillations in the mass and mixing regions still allowed by previous solar neutrino experiments; (2) knowledge of the temperature and mass mixing conditions inside the sun via determination of the relative branching ratios for major reactions; (3) possible independent confirmation of results from KamLAND, an antineutrino reactor oscillation measurement in a different energy range using neutrinos; and (4) significant sensitivity in the small mixing angle region (SMA) should existing results from Super-Kamiokande and SNO not be otherwise confirmed.

The proposed prototype would address several quite significant technical issues: (1) electron lifetime, (2) U/Th radiopurity of the inner shield, (3) ^{14}C:^{12}C ratio in deep-well methane, (4) radiopurity of other materials used in construction, and (5) determination of optimal operating parameters for design of the ultimate detector. In addition, calculations of cosmogenic background rates could be confirmed, and various technical schemes for construction of the endcaps and other components could be tested. Such a prototype should be built at depths of 2000 mwe or greater, allowing use of either the Homestake Mine or WIPP as a test site.

The proposal is from an international group consisting of physicists from the U.S. and Europe, notably the College de France, which has worked in this area for many years. The spokesperson is G. Bonvicini from Wayne State University. The U.S. group consists of physicists with experience in the building of drift chambers, calorimeters, and electronics for experiments at Cornell, FNAL, and Jefferson Lab. The major development effort would be based in the U.S. The proposal is for funding from the NSF at a level of 1.9M$ over three years. Educational opportunities are also discussed, in connection with existing outreach activities at participating institutions.

SAGENAP members agreed on a general level that advancing solar neutrino measurements with new detectors is well worth pursuing. In particular, the measurements of the pp neutrinos were found to be the most compelling for understanding the sun itself. As one member wrote, "*In this worldwide perspective the requirements for new initiatives are very demanding and it is indeed appropriate to ask whether new studies of solar neutrinos are justified at all. We believe the answer to this question is clearly affirmative, provided that the detectors of this newest generation have the ability of observing a large fraction of the pp neutrinos in real time and that they are designed to take data for very extended periods of time (e.g., 20 to 30 years). The ability of observing the pp neutrinos is essential because they are produced in conjunction with the reactions that generate most of the energy in the sun. These same reactions are the first step in the fusion process in our star and provide a very important normalization in the study of neutrino production and solar physics in general…It should be realized that the new breed of detectors will probably be as important in studying neutrinos as they will be in observing and monitoring the sun.*" Another member concurred, "*This input to the solar model is invaluable*." A third member noted, "*This is an area that has been overshadowed by the oscillation measurements, but which has immense, broad-based scientific interest. This was pointed out by the 2001 NSAC long-range planning white paper on Astrophysics, Neutrinos, and Symmetries*." That member agreed "…*there is a general consensus in the nuclear physics and astrophysics community that such experiments are of the highest priority for understanding stellar evolution and solar nuclear physics. Thus I consider this proposal to be very well motivated scientifically.*"

Astrophysical motivations aside, the case made by the proponents for the neutrino particle physics that might be done with the TPC, given what we now already know, was less convincing to SAGENAP members. One member wrote, "*The presentation emphasized the additional benefit of measuring the mixing angle to excellent precision, but this tended to obscure the main objective…because it was not at all clear why such precision was important, once the LMA/SMA confusion is eliminated*." Another member analyzed the situation this way: "*In consideration of the effectiveness of a low energy solar neutrino effort as a neutrino oscillation experiment, there are really two scenarios to consider: (1) KamLAND rules out the LMA region for electron antineutrinos, leaving open the possibility for more exotic neutrino properties (e.g., CPT violation) and/or solar models to explain the measurements; (2) KamLAND obtains a compelling positive results for electron antineutrino oscillations in the LMA region, reconciling all the solar neutrino measurements with the SSM and extracting the mixing parameters…..In the first instance, the case for measuring the spectrum of the solar neutrinos below ^{7}Be would be crucial to untangling the solar from the neutrino physics to determine if there is a difference in the oscillation of ν_e and ν_e-bar, or if the actual solution lies in the so-called LOW region, where one might observe day night effects. In the second instance, a low energy neutrino measurement can only confirm an overall deficit of the pp flux due to an LMA solution, as there will be almost no spectral deformation in the low energy region…..There may be some advantage in the precision to which $sin^2(2\theta)$ can be* *extracted from the data, but since the typical error on the reactor flux is about 3%, as compared to 1% for the pp flux, the gain in precision is not compelling.*" That member concluded, "*For neutrino oscillations, the scientific justification for a low-energy solar neutrino experiment depends crucially on the results from KamLAND.*"

SAGENAP members were also skeptical about the additional physics topics mentioned in the proposal. As one member wrote, reflecting the opinions of other members as well, "*The statement that the detector could be used for other physics (such as dark matter searches) was not fleshed out*."

There was a general consensus that a TPC of the type proposed is an appropriate approach with a reasonable chance of success, and that R&D at this time in some form is a good idea. One member wrote, "*While the ideas behind the proposal…were originally put forward by T. Ypsilantis about 10 years ago, we believe it fair to say that not much R&D work has been done until now and this proposal probably represents a good opportunity to investigate the practical feasibility of the Ypsilantis idea. Along with TPC, other R&D projects such as Heron, LENS, MOON and Xmas are being pursued in Europe, Japan, and the U.S.*" That member added, "*In case of successful R&D, TPC could well turn out to be the (or one of the) winning technology(ies) selected by the community to bring the solar neutrino investigation to the next phase. So this reviewer believes that it would be generally appropriate to fund some R&D in this direction. While the principles of the detector are quite straightforward, the practical realization of a high pressure TPC of the size and at the pressure contemplated here is indeed extremely challenging*." Another member wrote, "*The idea of the high-pressure TPC has the great advantage of being able to image the sun and thus have good control of systematics both as a function of time and energy. If all experiments worked equally well, the one with the directional capability would in the end be the most convincing. That being said, I think that the actual construction of a high-pressure TPC to the required size and radiopurity specifications is an extremely difficult task.*" That member added, "*It requires the building of a TPC with five times the drift distances than those currently in operation, and in addition requires radiopurity levels in plastics and methane gas beyond those currently achieved in Borexino, SNO, and KamLAND. It also requires the development of construction techniques that minimize materials in the central volume that could potentially outgas oxygen and/or contribute to the radioactive backgrounds. The largest TPC size currently used to detect neutrino-electron scattering is* $1m^3$ *(MUNU) at 3 bars. The final solar design requires roughly 4000* m^3 *at 10 bars. The prototype proposed here would be an order of magnitude step forward from existing designs, but still quite modest compared to the final detector. The step from existing technology to even this prototype is non-trivial and will require several advances in drift technology, materials selection, and construction.*"

While there was a clear interest among SAGENAP members to see TPC R&D of this type, there were concerns about the particular approach as proposed. Several SAGENAP members believed the most useful question to be addressed by the prototype is the electron lifetime, and they were not convinced that a smaller test chamber could not work equally well in this regard. The other critical design aspect to be tested by the prototype is the mechanical design of the pressure vessel. Here, SAGENAP members worried that any prototype might be far too small to be useful. Wrote one member, "*In particular a critical issue is the ability to drift electrons over very long distances and this could be tested with a long chamber of very small diameter (like a pipe). At the same time the chamber for R&D proposed here would allegedly test some of the mechanical issues related to the construction of the pressure vessel. We disagree with this statement. In fact, it is our opinion that from a mechanical standpoint the critical scaling will happen… going from the prototype proposed to the final chamber (that according to a simple calculation would have a vessel weighing, if made out of steel 2000 tons). Again, a pipe-type of chamber would test what needs to be tested and simplify matters for mechanical issues that a prototype can not test anyway.*" The same member further questioned the emphasis of the proposed R&D: "*In general, we feel…the concept of a 2000 ton pressure vessel to be taken deep underground could well result to be a practical show-stopper and should be addressed from the very beginning. At the same time, tasks such as electronics and trigger do not seem to require immediate attention.*" Another member agreed, "*Although WSU has the facilities for checking the integrity of the structure (thermal wave imaging), it was not at all clear that the actual design of the new vessel was viable. Some part of the R&D support should go toward a detailed vacuum vessel design by the appropriate engineering group. Since they are aiming for a final 4000 cubic meter detector, this design should show how it can be scaled to this size. The details of how you assemble tons of fragile equipment underground, what lab infrastructure is required, including water purification and clean room, were not addressed, yet concerns about the scalability of the detector are valid questions.*" Another member wrote, "*Indeed, it may be that this prototype could be made somewhat smaller and still provide convincing evidence of the long drift distances needed for the full size experiment. Since the initial design may not achieve even these drift requirements, it may be necessary to progressively modify or even completely replace parts of the detector. This would be easier if one started with a prototype that is of more manageable size but still large enough to get the job done.*" Another member agreed: "*However the question naturally arises as to whether building a 9 cubic meter TPC, many times the volume of MUNU, is the most efficient way to answer these questions. They should be encouraged to consider the minimum size device (perhaps with a long, thin profile) sufficient to determine the electron lifetime. Electrostatic tests including plastic wire shields, polyethelene TPC material, improved parts design, etc. can all proceed within the context of a smaller prototype, as well as checking He diffusion and electron attenuation length. Background studies including plastic purity tests for the inner shield and radon cold-trapping using He and methane can also proceed in parallel.*"

Related to the judgment of SAGENAP members that the prototype should be more modest in scope at the present time is the belief that the present collaboration requires significant strengthening. As one member wrote, an initially smaller and appropriate-scale R&D effort "*…would have the dual function of allowing the collaboration to begin addressing the outstanding issues while giving them time to build a stronger group. It is especially worrisome that there is no one who has had direct experience in high pressure TPC's or has actually worked on improving radiopurity,* *except the CdF people. However, CdF's major task in this proposal was identified as Monte Carlo work, rather than hardware.*" Another member agreed, "*In addition, the group seems too small to effectively be able to build the prototype to the required precision and radiopurity standards. They need more people with experience in TPC design and construction and also more people with expertise in radioassay and materials selection and control…. Formation of a larger, stronger group before moving ahead with the full prototype proposed here is strongly encouraged.*"

A number of SAGENAP members also encouraged further development of the software reconstruction techniques and better simulations of the TPC's capabilities. The use of the information from the detector and the tracks to be reconstructed are very different from those routinely encountered at accelerator-based experiments. The estimated performance presented at the meeting did not use all the information available from the TPC, and there is great room for improvement. As one member wrote, "*There is a lot of work still to be done in software while the smaller test stand is being prepared. Reconstruction of multiply-scattered tracks is extremely difficult and their reach depends crucially on the resulting average angular resolution, which they assumed to be 15 degrees at 100 keV (down to 5 degrees at 600 keV) for the purposes of this proposal. Background, calibration issues, etc. will all affect this and need to be better studied. Some portion of the startup funds could be well spent in improving their Monte Carlo.*" Related to this, there should be a clearly stated set of target detector performance requirements that are driven by the physics goals of the experiment.

In summary, SAGENAP members agree that, in principle, a full measurement of the solar neutrino flux is scientifically important and that a TPC of the type proposed has a reasonable chance of success. The proposing group is encouraged to do substantial work to make the physics case more effectively for the other potential topics. SAGENAP members believe the current group is subcritical in size, and may lack needed expertise in important areas. While the group is being strengthened, it is important to focus the hardware effort on answering a limited set of essential questions so that concrete results can be achieved quickly. In particular, SAGENAP members see no reason why a high-pressure chamber that is much more modest than the one proposed would be any less effective at addressing key technical questions. Once the physics case has been made; a modest chamber has been built, made to work, and characterized; and the collaboration has been expanded in needed areas, a proposal for a larger-scale R&D program toward the ultimate solar neutrino TPC would be very compelling.

Nearby Supernova Factory

The Nearby Supernova Factory represents an effort of physicists and astronomers from France and LBNL, with LBNL providing the overall management and internal funding for the U.S. component. The review by SAGENAP at this time is mainly to provide additional peer-review advice to LBNL about this project.

The Supernova Cosmology Project, led by a member of this team (Perlmutter) at LBNL, was one of two groups that found remarkable evidence for the accelerating universe by using SNe Ia as a standardized candle. The ultimate goal of this type of observations is to compare apparent brightnesses at near and far redshifts of supernovae with normalized properties exploding into similar host galaxies. While ambitious proposals exist to improve high-redshift observations, *e.g.*, the SNAP mission, this proposal concentrates on nearby supernovae. The goal of the Nearby Supernova Factory is to discover and obtain lightcurve spectrophotometry for ~ 100 Type Ia supernovae per year over 3.5 years in the low-redshift end of the Hubble flow. A wide-field search in the redshift range $0.03 < z < 0.08$ with weekly repeats is proposed. The essential tools are wide-field CCD imagers and an integral-field-unit optical spectrograph. The program uses the existing Haleakala and Palomar I and II telescopes. A pilot program was carried out by the Supernova Cosmology Project in the spring of 1999. Spectra from 11 supernovae were obtained, many of them past maximum light.

The scientific goals of the observing program are: 1) improved accuracy in future high-redshift supernova cosmology studies by statistically anchoring the low-redshift portion of the luminosity-redshift diagram, 2) calibration of the width-brightness relation (now incorporated by the stretch factor) and the intrinsic colors used for correction of extinction by dust (and, in general, exclusion of the possibility of conspiratorial evolutionary effects in the explosions themselves) and 3) constraining Ω_{matter}.

The redshift range is chosen to be not so far as to require excessive amounts of telescope time, yet far enough so that host galaxy peculiar velocities will contribute little to the error budget. These observations are complementary to proposed space-based measurements with SNAP. Measurement of low-redshift supernovae requires a wide field of view to collect enough supernova light curves; SNAP would concentrate more deeply on a relatively small patch of sky. There is a dearth of data between $0.1 < z < 0.4$, but this work is not optimized to fill this unmeasured region. Instead, the strategy is to concentrate the measurements at the lowest-redshift end of the Hubble plot to anchor the distribution at one end of the lever arm.

Several SAGENAP members initially had concerns that this project might distract too much attention and staff away from SNAP. However, members were later convinced that the two efforts are, in fact, synergistic. The proposed project will be an excellent opportunity for students, and it will also aid SNAP technology development by proving out the Berkeley CCDs (which are planned for use in the SNAP GigaCam imager) and the Integral Field Unit (IFU) spectrograph in a data taking environment over several years.

SAGENAP members were also happy to hear that the SNfactory team is reaching out to leading supernova theorists, such as K. Nomoto's group in Tokyo, to collaborate on this project.

With the important caveat that there was no optical astronomy expertise in SAGENAP, members were very positive about endorsing this project at LBNL. As one member wrote, "*This effort to address one of the most profound mysteries in particle physics. Its cost is minimal given the significant scientific return and small compared to future large-redshift missions such as SNAP. The Supernova Factory can nevertheless significantly boost the scientific return of the SNAP mission. It represents an excellent program that can be run in parallel with the rather long design effort for SNAP. It will give the LBNL supernova program the opportunity to bring supernova experts into the project who will undoubtedly be interested in the results of the Supernova Factory…. The team already has extensive experience with observations of this type. The construction, data acquisition transmission and handling as well as the observations are thoroughly planned with timelines and deliverables. Appropriate management is in place.*" Another member summarized, "*This is a great idea: this program tests the technology needed for SNAP, further helps to bring the different communities together (also needed for SNAP), and provides an opportunity for young people to make measurements.*"

ICARUS

This LOI from the UCLA component of ICARUS covers the potential continuation of the production of feedthroughs and, in general, the high voltage system for the multi-module expansion of the ICARUS detector. The expected request, if the greater ICARUS project goes forward, is approximately $500k.

ICARUS is a very large liquid argon (LAr) tracking TPC that is able to self-trigger and efficiently record tracks by drifting electrons over distances of meters. The first module, successfully tested in a surface lab in Italy, represents the coronation of a very long R&D program to produce a triggerable "electronic bubble chamber". SAGENAP members believe the results obtained by the group are truly outstanding, and many other programs are benefiting from these pioneering efforts.

Members of SAGENAP noted that the original physics motivation for this effort, the search for proton decay, has been significantly reduced since, in the mean time, the SuperKamiokande detector, with a more crude technique but vastly superior fiducial mass, has raised the bar well above what may be achievable by ICARUS. If ICARUS is to go forward, the remaining physics potential in the area of proton decay should be worked out in detail and in the context of the capabilities of other experiments. There are, in addition, other possible important physics topics to be explored. In particular, a new opportunity will arise if the CERN to Gran Sasso (C2GS) neutrino beam is constructed. Members of SAGENAP found this to be an opportunity that may be worth pursuing. One member wrote, "*Although the SAGENAP presentation did not get into enough details to really assess the power of ICARUS in this arena, superficially it seems to us that this may well be a good match. In any case the Italian INFN seems committed to proceed with the project and provide the great majority of the funding.*" There are uncertainties about the reality of the C2GS beam, but SAGENAP members were told these should be sorted out during the next year. Another physics topic, solar neutrino physics, was met with more skepticism. One reviewer wrote, "*,,,this reviewer is quite skeptical about the ability of ICARUS to do solar neutrino physics (because of the large multiple scattering and the choice of detector materials)*".

There was great appreciation by members of SAGENAP of the role of the UCLA group in ICARUS. One member wrote, "*The small UCLA group has been with the project from the very beginning and, we believe, has given very valuable contributions in many fields, spanning from physics motivations to the construction of very specific and well chosen pieces of hardware. UCLA has maintained a presence at CERN where most of the LAr work has been performed and they were able to bring home their specific construction item, in this case the HV system that includes special cryogenic HV feedthroughs. This task was apparently carried-on professionally and to everyone's satisfaction.* The member went on to add that a "*small financial investment rewards good work done and has potentially high returns in physics."* Other members agreed, saying "*as long as ICARUS happens, support their contributions*", and, "*Give credit to those people who have had the guts to stick through this risky project that is now ready to bear fruit.*" Members added that the actual proposal would have to be judged at the time of submission by the details of the overall physics reach of the ICARUS detector, which include (but are certainly not limited to) the prospects for the C2GS beam.

Auger Status

The Pierre Auger Project will soon begin addressing fundamental questions regarding the nature and origin of the most energetic particles detected. The facility, currently under construction, will consist of an array of surface shower detectors and fluorescence detector telescopes. The experiment will study cosmic rays to energies beyond 10^{20} eV with a statistical reach that is an order of magnitude better than what is possible with any existing facility. There are no credible models for the acceleration and propagation of protons or nuclei to energies above 10^{20} eV, so the nature and origin of these cosmic rays is a mystery. At these energies, charged particles would not be significantly deflected by expected intergalactic magnetic fields and are therefore likely to point back to their sources. In addition, a cutoff in the proton spectrum is predicted above a few times 10^{19} eV from sources further than ~50 megaparsecs (a relatively small distance) due to the interactions between cosmic rays and the cosmic microwave background. Detailed measurements of the cosmic ray spectrum in the region of the Greisen-Zatsepin-Kuz'min (GZK) cutoff will therefore provide important information to constrain models for the origin and composition of the particles responsible for these spectacular events. Recently, events have been observed above the GZK cutoff by all experiments, but at differing rates. The explanation of these events will require either new astrophysics or new particle physics. The devil is in the details of the spectrum and that is where the present experiments seem to disagree, although the statistics is very limited. This has created a great opportunity for the Auger project.

The collaboration has recently made impressive progress with the first data from the completed engineering array of 40 surface detector stations and 2 fluorescence telescopes. The objectives of the engineering array have been met, including testing of all detector components under field conditions, testing and improving deployment strategies in all seasons and terrain, refining cost estimates, and building infrastructure. The construction of an assembly building and central laboratory has been completed. The project anticipates the completion of 12 fluorescence detectors and 100 surface tanks within one year. Operation of the engineering array led to redesigns of the data acquisition electronics and the structure of the tanks serving as water Cherenkov detectors. Nevertheless, the data, including 20 hybrid events (those seen by both the surface array and fluorescence telescopes) per month, are being analyzed by the collaboration, which consists of over 200 scientists from 59 institutions from 16 countries.

Final production manufacturing methods are being validated, and there seem to be no showstoppers. The pre-production run of 100 surface detector stations will be deployed in 2002, followed by production detectors and associated electronics. Commissioning will begin in early 2003. The full 1600-station surface array can be completed in the first quarter of 2005, along with the 24 fluorescence detector telescopes in 2004, funding permitting. Significant scientific results should emerge from the partially completed array in 2003.

CDMS II Status

Direct detection of dark matter continues to be one of the most compelling pursuits in particle physics, astrophysics, and cosmology. The Cryogenic Dark Matter Search (CDMS) has been at the forefront of this research since its inception, using state-of-the-art detectors designed and developed by the collaboration. These detectors, which can be constructed using silicon or germanium to have complementary sensitivity to spin-dependent and spin-independent couplings, detect both ionization and phonon signals. These two signals provide a means to distinguish nuclear recoil events (from WIMP or neutron scatters) from other radioactive backgrounds. CDMS I, sited at the shallow Stanford Underground Facility, has placed important limits on particle dark matter models at low mass. A comprehensive paper containing these results has been submitted for publication in the Physical Review. CDMS II, which is under construction, will consist of 42 detectors installed in the Soudan mine in Minnesota.

The individual detectors are arranged in towers for practical reasons. The first Soudan tower, consisting of 4 germanium and 2 silicon detectors, is currently running at the Stanford Underground Facility. The collaboration reports excellent performance from these detectors: the baseline energy resolution and background rejection capabilities meet or exceed the specifications. More than 50 kg-days of germanium data have been recorded, which alone should represent a significant advance in sensitivity. Production of the remaining detectors is proceeding well, with lessons learned being fed back into the production process, and all detectors should be finished and tested in less than two years. The first towers will be moved to Soudan when the facility is ready.

The Soudan facility readiness has become a significant issue. The project is already some 8 months behind the baseline schedule, and the problems continue to be a major source of schedule uncertainty. In addition to the predictable sources of delays, such as interference with MINOS construction at the site, the cryo-system refrigerator, built in industry, has become an enduring source of problems. The problems include low-temperature leaks and an operational accident that led to structural damage of the fridge. The problems are compounded by the remoteness of the site and the limited past effectiveness of the collaboration to ensure that experts and senior scientists are in residence at all times. The CDMS collaboration recognizes the importance of improving this weakness and, at the time of the SAGENAP meeting, was putting a rotation plan into effect. The current schedule shows towers 1 and 2 moving to the Soudan site in August 2002. All other infrastructure support systems will be installed and operational by that time. The decision to install tower 2 along with tower 1 was made in an attempt to recoup some of the lost time.

The project is also facing some management personnel losses, but it is meeting this challenge well by actively taking steps to minimize the impacts and to identify and hire replacements.

Given the excellent performance of the new detectors, and assuming the nagging startup problems at the Soudan site will now be overcome with increased attention and resources from the collaboration, the CDMS II project shows great promise for delivering great science in the near future.

Drift Status

The Directional Recoil Identification From Tracks (DRIFT) experiment is a WIMP dark matter search using a negative ion TPC detector. The DRIFT 1 detector has an active mass of 170 grams. The rationale for a tracking detector is twofold. First, with all the information provided by the tracking detector (total ionization, track length, event localization, and pulse shape), DRIFT is expected to achieve exposure-limited sensitivity for zero, or near zero, background accepted after cuts. Second, the tracking detector has the unique capability of measuring the direction of the recoiling nucleus, providing information about the direction of the incoming scattered particle. This provides sensitivity to any sidereal correlation between rate and direction, an important signature for galactic dark matter halo WIMP signals.

Funding for the first phase of the DRIFT experiment (DRIFT 1) was initiated in late 1999. The collaboration obtained safety approvals for the vacuum vessel, the underground use of the CS_2 fill gas, the 50-kilovolt-drift voltage and other critical operations. A 150-m^2 lab-grade building has been constructed at a depth of 1180 meters, and the detector is now fully installed in the underground laboratory. Engineering runs to understand the readout system are underway. Several tens of hours of running looking at intrinsic backgrounds have been performed and data taking is expected to begin in a few weeks.

Detailed engineering studies and planning for four kilogram and one hundred kilogram detectors (DRIFT 2 and DRIFT 3) are in progress.

EXO R&D Status

EXO is a new concept to search for neutrinoless double-beta decay and, hence, evidence for lepton number violation and neutrino mass. The technical approach involves observing, in a large volume of ^{136}Xe, the coincidence of ionization with the right energy pattern and the appearance of a ^{136}Ba ion in the final state. The work in the U.S. is being carried out at Stanford University, SLAC, and the University of Alabama.

The EXO proposal was presented to SAGENAP two years ago. A Xenon test purification system has since been built and tested at SLAC using all UHV components and clean-room assembly. A high resolution laser spectroscopy system with a Barium source and quadrupole magnet RF trap was built and operated at Stanford. A test device to investigate the extraction of single ions from liquid Xenon was designed and built at SLAC. A test liquid Xenon ionization chamber with high efficiency charge and scintillation readout was built at Stanford. In fiscal year 2002, additional funding was provided by the DoE Environmental Management program for the procurement of 100 kilograms of 80% enriched ^{136}Xe.

There are two detector options under consideration: 1) High pressure TPC, as originally proposed (20atm, 35m^3 modules, 4.2 ton/module, 2 modules with Xe enclosed in a non-structural bag roughly the size of ALEPH TPC); and, 2) Liquid Xe Chamber, very small detector (3m^3 for 10 tons). The collaboration is currently leaning toward the liquid Xenon option due to its compact nature. The associated R&D includes: single ion Ba+ tagging at difference residual pressures, Xe purification, liquid Xe energy resolution, Ba ion lifetime and grabbing from liquid Xenon, procurement/qualification of low background materials, isotopic enrichment of ^{136}Xe, and finally the construction of a 100 kg ^{136}Xe prototype detector which should be running by the end of 2003. The 100 kg prototype will be capable of measuring the ^{136}Xe two-neutrino double-beta decay mode, which has never been measured and which is the predominant background process for EXO.

HiRes Status

HiRes is an ultra-high-energy cosmic-ray experiment based on the measurement of the nitrogen fluorescence induced by air showers. It is an extension of the pioneering Fly's Eye experiment, sited at the Dugway military proving grounds in Utah. Air showers may be viewed from one location (monocular mode) or from separate locations simultaneously (stereo mode). The primary physics goal is a higher-statistics measurement of the cosmic ray flux around and above the Greisen-Zatsepin-Kuz'min(GZK) critical energy, *i.e.*, a few x 10^{19} eV and above. The previous observation of events above 10^{20} eV has raised a number of fundamental questions in particle physics and astrophysics, and addressing these questions is a very high priority.

HiRes I was completed in 1997, and has been taking data in monocular mode since that time. HiRes II was completed in 1999. HiRes II operations were stable starting in 2000, and terminated on September 11, 2001 when the Army began refusing collaboration access to the site. Approximately one year of data in both stereo and monocular modes have been accumulated by HiRes II.

The collaboration has been working productively on data analysis. One of the critical components of understanding the data, particularly in establishing the energy scale, is the determination of the atmospheric properties and the resulting molecular and aerosol scattering. Steerable lasers are used as calibration sources. The group has characterized and monitored the atmosphere, measuring the aerosol scale height, which is typically about 1 km, and hourly atmospheric corrections are being applied to the data. There are also important preliminary results on the UHE cosmic ray spectrum. The combined monocular spectra show a pronounced ankle structure around 3 EeV (1 EeV = 10^{18} eV), in good agreement with the original Fly's Eye stereo spectrum. Using 4 years of data from HiRes I, there is also some evidence for the expected pileup below the GZK critical energy. There is one well-measured event with energy near 2×10^{20} eV. All spectra from all data sets from HiRes and Fly's Eye are consistent within statistics, however (at least at the time of this SAGENAP meeting) they are inconsistent with the AGASA spectrum both in normalization and in the location of the ankle.

The issue of access to the site is a critical one for the future of the experiment. An agreement has been worked out with colleagues at LANL, who have the necessary level of security clearance, to begin data taking. The collaboration believes they can complete the three-year data-taking plan in this mode of operation, but they also understand that continued lack of access to the site by the majority of the collaboration would make a longer-term experimental program at the Dugway site impossible. Alternatives are under investigation.

KamLAND Status

The KamLAND (Kamioka Liquid scintillator Anti-Neutrino Detector) detector combines a kiloton of active mass with low detection energy threshold. Its primary initial goal is to detect anti-neutrinos from a large number of commercial nuclear reactors. This key experiment should definitively confirm or refute the Large Mixing Angle (LMA) MSW solution to the solar neutrino puzzle in a terrestrial disappearance experiment and improve the statistical measurements of the mass difference and the mixing angle. The LMA solution is favored by the results of SuperK and SNO.

Other scientific goals of KamLAND include the observation of supernovae, the first detection of terrestrial anti-neutrinos, the search for exotic nucleon instability modes and, possibly, the direct observation of Be neutrinos from the sun.

The project is a Japan-US collaboration with the US providing front-end electronics, trigger, outer veto detector, radioactivity qualification, refurbishment of the twenty-inch inner detector photomultipliers and calibration systems. Approved in fall 1999, the experiment is now being calibrated (LEDs, laser, and movable ^{60}Co and ^{65}Zn sources) and is taking its first physics data using 1,325 fast 17-inch photomultipliers (for a total of 17.5% photocathode coverage). The 554 twenty-inch Kamiokande PMTs were refurbished and they will eventually increase the coverage to 30%, once their HV and readout electronics systems are complete. Early results indicate a performance in terms of light yield and backgrounds that is superior to the proposal.

The work of the group in commissioning the detector has been impressive. At present, KamLAND runs with physics triggers about 85% of the time, with the remaining time spent on calibrations and system upgrades. The HV system appears to be constructed of recycled and outdated hardware, divided into three different systems that are not long-term viable. The plan being developed to acquire a new, unified HV system will very likely be an excellent investment appropriate to the large physics potential of the experiment.

First data analysis is in progress, and the US contingent seems ready to use its excellent computer infrastructure and expertise to deliver these important results.

Milagro Status

MILAGRO is a ground-based gamma-ray detector using the water Cherenkov technique. The experiment consists mainly of a large pond of water, instrumented with approximately 750 photomultiplier tubes in two layers (top and bottom). High-energy particles (cosmic rays and gamma rays) create showers in the atmosphere; MILAGRO detects the shower particles and infers a shower direction and energy from the detected PMT pulses. The energy threshold of the experiment is somewhere around 1 TeV for gamma rays, and the angular resolution is approximately 0.75 degrees. The experiment is being augmented by the addition of 170 water tanks (called "outriggers") around the pond that will help in identifying the shower core location and hence improve the energy and angular resolutions. With the outriggers, the energy resolution is expected to be approximately 50%.

A major strength of MILAGRO is its ability to observe sources around the clock and to view a large fraction of the overhead sky at any given time. This ability is crucial to detect new phenomena at very high energies. The disadvantage of the technique is in the relative paucity of information in showers at these energies – one can achieve only a relatively modest level of background rejection and thus the intrinsic sensitivity of MILAGRO is not as good as narrow-field instruments using the atmospheric Cherenkov technique.

The construction of MILAGRO started in 1994, but the schedule was stretched out to five years because of funding availability. An interim experiment called Milagrito, which used a single layer of PMTs, was deployed and operated between 1996-98. The full MILAGRO experiment was completed in 1999, with physics data starting in December of that year. Since that time, the experiment has taken data relatively continuously, with the live time steadily rising to the 90-95% range. The collaboration has done a very good job at commissioning the experiment and bringing it up to good working order. They have also worked very hard at ramping up the data analysis effort done on site to the point where the large quantity of data can be analyzed in semi real-time.

The experiment has made good progress on a number of fronts since the last SAGENAP meeting. Most importantly, it was demonstrated that the experiment is now working. The event reconstruction software is in place and it shows clear evidence for a deficit in the number of cosmic rays from the direction of the Moon - this checks the experiment's absolute pointing, angular resolution, and stability. Background rejection techniques have been developed using the information from the bottom layer of the pond. These techniques apparently increase the gamma-ray signal from the Crab Nebula to a believable level (from 1.6σ to 6.1σ). The quoted sensitivity on the Crab is approximately 6σ in two years or 4.2σ/yr, not far off from the design. Outriggers should improve the sensitivity further. MILAGRO also sees evidence for emission from Mrk 421 during a high flare state in early 2001, but does not have any evidence in the two years of data for any strong transient sources, such as gamma-ray bursts. The collaboration is developing interesting methods to improve the low energy response of the detector (using new trigger electronics), and it is hoped that these will be installed and operating soon. It was not clear from the presentation whether the current energy threshold is actually 500 GeV or 1 TeV.

One significant statement from the 2000 SAGENAP review concerned the importance of establishing a mechanism for MILAGRO to provide alerts to the astrophysical community -- especially to the more narrow-field X-ray and gamma-ray telescopes observing the same sky as MILAGRO -- of strong, transient high-energy sources. It appears that this mechanism has not yet been implemented.

In summary, MILAGRO is now taking data and operating smoothly. The group is commended for improving the live time of the experiment and for setting up full analysis of the data at the site. This is a challenging experiment and the collaboration has worked quite hard to get to this point. The experiment has now detected sources. The outriggers will add needed capability and should be incorporated into the detector and data stream as soon as possible (*i.e.*, this year). The collaboration should set up a burst alert system to the community as soon as possible.

SNAP R&D Status

The SuperNova Acceleration Probe (SNAP) project aims to build and operate a satellite telescope for cosmological and astronomical research. SNAP is being designed to detect and measure the properties of many Type 1a supernovae to investigate the nature of dark energy, the mysterious and hypothetical quantity invoked to explain the apparent acceleration of the Universe inferred from earlier observations of Type 1a supernovae at moderate redshift (out to z~1). These earlier results were obtained by two groups, one of which has helped to spawn SNAP. The technique relies on a phenomenological relationship between the shape of the light curves and the absolute brightness of Type 1a supernovae to enable their use as corrected candles. Because the interpretation of these results implies something profound in fundamental physics, the standard of proof is correspondingly very high. It is therefore essential for better measurements to be made, following the expansion history of the Universe out to z~1.5, with a clear demonstration that the results are not due to some unaccounted systematic effect inherent in the technique. This is the primary purpose of SNAP.

In addition to its primary purpose, it is clear that a space telescope with the characteristics of SNAP (Hubble-sized mirror and relatively wide field-of-view) is capable of carrying out a broad range of astronomical studies, with both cosmological and non-cosmological impact. Thus, the science motivations for SNAP go beyond the already very important question of dark energy. SNAP is currently in the phase of mission definition and R&D. The goal is to develop the complete cost and schedule plan by 2004. The SNAP team reported on a substantial amount of progress accomplished in the last two years (since their initial presentation to SAGENAP in 2000). Substantial work has been carried out on many aspects of this intricate project, especially in the areas of optics, launch, detector testing, mission definition, and simulations. Overall, the technical aspects of the project are progressing well, and there do not appear to be any showstoppers.

The optical design has progressed significantly in the last two years, and an initial design for the telescope was studied by the NASA/GSFC Instrument Synthesis and Analysis Laboratory and by industry. The detector focal plane design and hardware have also been given significant attention. The optical (350-1000 nm) detectors will consist of radiation-tolerant CCD's, such as those being developed at LBNL. Tests have been carried out on batches of these CCD's and many performance requirements (*e.g.*, dark current, noise, radiation damage) have already been met. Optimization studies for the SNAP camera will be carried out. An exciting milestone was the successful use of such CCD's for astronomical observations at NOAO/Kitt Peak. Another important development has been the increase in the fraction of pixels devoted to NIR (400-1700 nm) detection. This change was strongly recommended to SNAP by the DoE Review panel in January 2001, and the SNAP team reacted to modify their design. Because SNAP will require a fair fraction of the worldwide supply of HgCdTe detectors (and Rockwell is essentially the only vendor), an important milestone over the next two years will be to establish a realistic production, delivery, and test certification schedule for the

NIR detectors. Other aspects of the camera (*e.g.*, filter strategy) will require work and the team is planning appropriately. Calibration is another issue raised during the DoE Review that will require attention in the future. Studies have also been carried out on the spacecraft design and launch parameters. Work with the NASA/GSFC Integrated Mission Design Center has further developed the spacecraft requirements and orbit possibilities. Work has started on the thermal and structural aspects of the payload and spacecraft.

If SNAP is placed on the NASA SEU Roadmap, we assume that DoE and NASA will discuss how to move forward in partnership to carry out these key activities. The overall picture of how SNAP will fit into the NASA and DoE portfolios is also not clear at this time, but this is a large issue that goes beyond the scope of SAGENAP. Again, we assume that DoE and NASA will work together constructively to develop and nurture this project appropriately.

The 2000 SAGENAP report recommended that SNAP broaden its scientific focus, both in terms of its mission definition and in terms of makeup of the collaboration. The reconfiguration of the camera and more community involvement in the broader science of SNAP have been excellent developments. The SNAP team has also been adding more collaborators, largely from the particle physics and cosmic ray communities. A dedicated study group, working in the context of Snowmass 2001, produced the well-publicized "Resource Book on Dark Energy". SNAP has had a growing presence at AAS meetings and, ideally, the project will be even stronger by the addition of more astronomers.

In summary, the science of SNAP is as compelling as ever. The question of dark energy is among the most profound questions in all of science. The SNAP team is strong and is adding further strength. The R&D progress has been steady so far and there do not appear to be any technical showstoppers preventing a successful conclusion of the R&D phase of the project.

STACEE Status

The Solar Tower Atmospheric Cerenkov Experiment (STACEE) is a ground-based high-energy gamma-ray observatory, located at the National Solar Thermal Test Facility (NSTTF) of Sandia National Laboratories in New Mexico. By day, a large array of mirrors, called heliostats, focuses solar radiation onto a tower for solar power experiments. By night, the heliostat array focuses atmospheric Cerenkov light from cosmic ray induced air showers onto the STACEE detector, which is consists of an array of phototubes and associated electronics. Due to recent funding reductions at NSTTF, more operating costs are now being passed onto STACEE in the form of modest usage fees.

The detector can distinguish gamma-ray-induced air showers from those initiated by charged particle cosmic rays, and the residual background rate is measured by pointing the array off source. What most distinguishes STACEE from other ground-based gamma-ray observatories like Whipple is its ability to operate at energies below 250 GeV. This is important for several reasons. First, since astrophysical gamma-ray sources typically have power law spectra, lower energy threshold results in significantly improved statistics. Since gamma-ray sources of interest to STACEE are often highly variable, the increased statistical power allows variability measurements on shorter timescales, and the variability is an indication of source feature sizes. Second, there is an inverse relationship between gamma-ray energy and attenuation length through intergalactic space due to pair creation off the bath of infrared background light: lower energy photons in this regime probe larger cosmological volumes. Third, the infrared background light is itself of great interest, since different models of galaxy formation predict different background light densities, and measuring the energy-dependent attenuation of an adequate sample of sources at an appropriate range of distances can probe the background light density. Finally, the lower energy threshold raises the intriguing possibility of observing O(100) GeV photons from gamma-ray bursts, which are known to occur at cosmological distances. Since the STACEE field of view is relatively narrow, its burst potential will be greatly enhanced after SWIFT is launched in late 2003. Construction of the detector was completed in October 2001, and the experiment is now running with 64 heliostat mirrors viewed by 64 phototubes, each with FADC readout, and an energy threshold below 100 GeV.

The experiment has already produced interesting physics. In 1998, during construction, the physics potential was demonstrated using a portion of the array to detect the Crab Nebula at peak energy of 190 GeV at $>7\sigma$ significance. The flux from AGN Markarian 421 was shown to be highly variable, and during the most intense flaring observed STACEE collected gamma rays at record rate of nearly 1000/hour (5 times higher than Whipple). Flux limits were also placed on two other AGNs.

STACEE recently began its three-year observing program, promising exciting results from its lower-energy capability. The team remarked that it is too early to say whether a second-generation experiment is warranted. This will have to be evaluated in the context of the results of the current observing program and the capabilities promised by VERITAS and GLAST.

Whipple/VERITAS Status

The pioneering Whipple ground-based gamma-ray observatory, with effective area ~100,000 m^2, energy range 0.2-100 TeV, angular resolution ~0.2 degrees, and ~10^{-3} hadron rejection, continues to be the leading instrument of its kind in the world. The Crab Nebula is typically detected at 7σ significance in one hour of observation. The group has an impressive list of scientific accomplishments, including the 1989 discovery of TeV emission from the Crab; the 1992 detection of TeV emission from Markarian 421 (at z=0.031); the observation of an intense, short flare from Markarian 421 in 1996; the months-long flaring detected from Markarian 501 (at z=0.034) in 1997; the discovery of 1H1426+428 (at z=0.129) to be published in 2002; and the very interesting new discovery of a correlation between flux level and spectral index from Markarian 421 during an extended period of flaring in 2001 (paper in preparation).

In addition to great science productivity, the Whipple telescope is also serving as a test bed for the VERITAS electronics and camera systems. VERITAS, which is an array of telescopes, is the next-generation experiment under development by the collaboration. The array design cleverly provides much better background rejection capabilities, allowing a significantly lower energy threshold, as well as better energy and angular resolution, larger effective collecting area to expand the capabilities at high energy, and an expanded field of view (or the ability to study more than one source at a time with sub-arrays when the full collecting area is not required). The scientific motivation for the new capabilities is strong. VERITAS science topics include probing the IR extragalactic background light at moderate redshift, providing important constraints on models of galaxy formation; measuring the short timescale variations in bright AGN flares; characterizing the highest-energy AGN emission during periods of lower flux; confirming or refuting the hypothesis that cosmic rays with energies below 10^{14}eV are produced by shock acceleration in supernova remnants; distinguishing currently viable acceleration models of pulsars by measuring the populations and spectral roll-offs observed in HE and VHE gamma rays; performing a major galactic survey in the VHE gamma-ray band, which has the potential of uncovering important new source classes; searching for gamma-ray "lines" from supersymmetric galactic dark matter annihilations; providing the possibility of observing TeV emission from gamma-ray bursts; and, most important, providing a sizable discovery potential for the unanticipated.

The original VERITAS proposal was based a seven-telescope array, with funding provided by a partnership among DoE, NSF, and the Smithsonian. Unfortunately, due to a reassessment of its scientific priorities, Smithsonian will not contribute its planned share of VERITAS capital construction costs in FY03. A new request will be made for FY04, and Smithsonian will continue to provide major infrastructure and other operating support for the project. As an interim solution, the NSF and DoE quickly approved proceeding with a single VERITAS prototype telescope, and this work appears to be going very well. With the reduced funding, the collaboration is now planning to move forward with construction of four telescopes (VERITAS-4), to be completed prior to the launch of GLAST in 2006 to enable most of the planned complementary observations.

Compared with the full array (VERITAS-7), VERITAS-4 would have a 100 GeV threshold (compared with 50 GeV for VERITAS-7) and ~30% worse pointing resolution. The flux sensitivity at 300 GeV for VERITAS-4 will still be excellent at 300 GeV (6 mCrab, compared with 5mCrab for VERITAS-7), and all critical performance parameters at all energies will be far superior to those of Whipple. Still, the collaboration remains firmly committed to the full seven-telescope array, viewing the construction of VERITAS-4 as the first stage of the project.

An enduring issue has been the selection of one of the two possible sites, again due to issues outside the control of the collaboration. At the time of the SAGENAP meeting, it appeared this issue was, finally, very close to resolution. Atmospheric testing during the past year indicates that both sites are at least as good as the Whipple site at higher elevation. The VERITAS project office is fully staffed, and the unified Whipple/VERITAS collaboration is in place to proceed with the project.

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SAGENAP AGENDA

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March 12, 2002

8:00AM	Executive Session
8:20AM	Aprile - Liquid Xenon DM search Science – an overview Experiment Management (manpower, cost, schedule)
10:05AM	Break
10:15	Bonvicini High pressure TPC Science – an overview Experiment Management (manpower, cost, schedule)
12:00PM	Lunch
1:00PM	Mann - Nucleon decay/neutrino oscillation Science – an overview Experiment Management (manpower, cost, schedule) BNL Neutrino beam for the above experiment
3:00PM	Break
3:10PM	Sobel – Super-K Rebuild
4:10PM	Break
4:15PM	Cline - OMNIS Science – an overview Experiment Management (manpower, cost, schedule)
6:00PM	Executive Session
6:45PM	Questions for Aprile, Bovicini, Mann, Sobel, and Cline
Adjourn	

March 13, 2002
Hilton Arlington,
950 North Stafford St., Arlington, VA.

8:00AM	Executive session
8:15AM	Q&A for the presenters of the five proposals
10:30AM	Break
	New Starts and Status Reports
10:40AM	Cline - ICARUS-2400
11:20AM	Loken - Nearby supernova factory
12:00AM	Executive session - Report writing
12:45PM	Lunch
1:45PM	Milagro
2:25PM	STACEE
3:05PM	Whipple
3:45PM	SNAP
4:25PM	KamLAND
5:05PM	EXO
5:45PM	Drift
6:25PM	Adjourn

March 14, 2002
Hilton Arlington,
950 North Stafford St., Arlington, VA.

8:30AM	Executive session
8:45AM	AUGER
9:25AM	HiRes
10:05AM	CDMS-II
10:45AM	Break
11:00AM	Report writing

Adjourn when a draft of the executive summary is finished