Department of Energy

National Science Foundation

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

April 12-14, 1999

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SAGENAP Meeting
Summary Report

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The Scientific Assessment Group Non Accelerator Physics (SAGENAP) which consists of Barry Barish (Caltech), Janet Conrad (Columbia), Tom Gaisser (Bartol), Jordan Goodman (Maryland), Bob Lanou (Brown), Steven Ritz (NASA – Goddard), Leslie Rosenberg (MIT), Bernard Sadoulet (U.C. Berkeley), Hank Sobel (U.C. Irvine), J. Stone (DoE), P.K. Williams (DoE), M Goldberg (NSF) and E. Loh (NSF) met from April 12-14 at the National Science Foundation. J. Stone (DoE) and E. Loh (NSF) served as co-chairs and B. Barish as report coordinator. Tom Gaisser was absent from this meeting.

Proposals for VERITAS, Axion, KamLAND, SAGE, Fluorescence Detector Development, CDM-TPC, Iodine, GLAST (NSF), and NuBE were presented. SAGENAP reviewed the written materials and the oral presentations, had a question-answer session with each proponent and had interactive discussions in executive session. Individual written reviews for each proposal have been made by at least three SAGENAP members and have been submitted to the DoE and NSF. This report summarizes the meeting and reviews of each proposal, highlighting the areas of agreement, concerns and some individual comments for each proposal. The report represents a balanced summary of the conclusions of SAGENAP members. These summaries for each proposal are contained in the body of this report.

In addition to the reviews, SAGENAP heard a very interesting presentation by Vernon Jones (NASA) on a possible NASA interstellar probe mission. Such a mission could possibly determine whether cosmic ray acceleration occurs via a one shot process or by a series of successive encounters with supernova shock waves. In addition, the acceleration of anomalous cosmic rays and other particle species at the solar wind termination shock may be investigated. SAGENAP members found this presentation very informative and urges the DoE/NSF to include more informational presentations and time for general discussions of the field, its directions, potential and priorities at future SAGENAP meetings. The group believes this will both help the members of SAGENAP judge individual proposals in better context, as well as provide the agencies with some informed input into the scientific directions and priorities for this emerging field.

Finally, in the SAGENAP discussions it was noted that many projects in this field are highly technical and require significant R&D in order to develop the techniques. Although many projects have been enabled by technologies that have been developed for accelerator experiments, other projects have unique technical requirements that require dedicated developmental work. In order to fully exploit the physics potential in non-accelerator experiments, the agencies need to be responsive to proposals to develop the enabling technologies.

VERITAS

The study of high energy gamma ray astronomy has provided a glimpse at extreme astrophysical processes in the Universe. The highly successful EGRET mission in space has catalogued a large number of point sources that produce gamma rays up to the GeV range. The ground based Whipple Observatory has established that there are sources continuing beyond the TeV range, some seen by EGRET and some that have not been seen in EGRET. The combination of GLAST in space and VERITAS on earth comprises the proposed second-generation projects in this field. Each will have greater sensitivity and the combination will overlap in the energy range covered, allowing the possibility of systematically studying the features of sources at these high energies, including how and where the sources cutoff. This can be important information in understanding the sources and acceleration mechanisms in producing these energetic gamma rays.

Gamma rays are a powerful probe of the Universe since they are not significantly attenuated (except at the highest energy through interaction with the infrared background) or bent from straight-line trajectories. They provide an unobstructed view through much of the plane of the Galaxy and point back to their source. The regions where gamma rays are emitted must have relatively low photon densities (e.g. from active galactic nuclei) or be from regions of magnetic fields less than critical strength (e.g. from pulsars and their associated nebulae). The γ-rays from extragalactic sources can probe the intergalactic radiation fields and the studies of the highest end of the spectrum can yield information about the emission mechanisms.

Of special interest from a particle physics point of view is VERITAS (and GLAST) sensitivity to dark matter Neutralinos. The predicted signal is a monoenergetic gamma ray at $E_\gamma = M_\chi$ and/or $E_\gamma = M_\chi(1 - M_Z^2/4M_\chi^2)$ resulting from neutralino annihilation. If neutralinos comprise the dark matter and are sufficiently concentrated near the center of the galaxy, presently a controversial point, VERITAS has sensitivity for detection for many models over a large fraction of the allowed mass range. The signature is unique, since observations of such annihilation lines are not expected from any other known astrophysical processes.

VERITAS is an outgrowth of the successful technique employed at the Whipple Observatory of using multi-pixel imaging atmospheric Cherenkov telescopes. Specifically, VERITAS is a proposal to build an array of seven imaging optical reflectors, each 10 m in diameter with separation of 80 meters. This detector is designed to detect γ-rays between 50 GeV and 50 TeV with an effective area ≥ 0.1 km^2 (E >1TeV), and an angular resolution of $\leq 0.05^0$ for individual photons. These parameters represent a significant step beyond Whipple and should match on well to the GLAST sensitivity range.

The estimated cost of VERITAS as presented to SAGENAP is \$16.6M, but a detailed cost estimate was not presented and the estimate presented did not contain either manpower costs or contingency. The proposal is to cost share this project between Smithsonian (\$6.1M), DoE (\$8.0M), NSF (\$2.0M), PPARC (\$1.0M) and Enterprise-Ireland (\$0.25M). The equipment construction project is to be built over 4 years and to be ready for operation in 2004. The timing is motivated by the anticipated schedule for the potential competition and for the GLAST mission. Of the other projects (e.g. HESS (German-French), New Cangaroo (Japanese-Australian) and MAGIC (German-Spanish-Armenia)), none are further along and it is hard to judge how fast the various projects will be deployed. It should also be noted that these experiments are also complementary, in the sense that the combination gives better sky coverage (North-South, and East-West, an important feature for intrinsically variable sources).

The SAGENAP members are in agreement that the scientific objectives of VERITAS are well motivated and the complementarity with GLAST is an extremely important feature. The details of the detector optimization where not presented, so SAGENAP is unable to assess whether a more modest proposal could achieve most of the goals. However, this proposal builds off the very successful experience of the Mt Hopkins collaboration. There was some concern voiced about whether the 'management style' of this group is capable of implementing this ambitious project.

Individual comments of note from SAGENAP members include the following:

Regarding the proposing group and technique, SAGENAP acknowledges the proposing group's pioneering work in this field. One member states, "*The Whipple group has single-handedly developed the field of Gamma Ray astronomy in the energy range around 1 TeV. They have spent many years learning how to discriminate against hadronic background and have slowly improved their technique.*"

SAGENAP finds the scientific potential of VERITAS to be very high. One member comments, "*EGRET and Whipple have shown us the tip of the iceberg -- together with GLAST, VERITAS will provide a large and essential step forward, answering many of the current questions and providing a reasonable expectation of uncovering new directions of research.*"

Several members stressed the complementary nature of VERITAS and GLAST and the need for a multiwavelength approach to understand the underlying physics. For example, one member states, "*It is essential to have a new instrument available to overlap with the next generation space detector, GLAST.*" Another member says, "*This lower energy threshold will give a significant overlap with the operating range of the proposed GLAST satellite experiment with similar flux sensitivity. Given the new low threshold of about 100 GeV, its data is directly in the region of maximum sensitivity to determine the high energy end of the AGN spectra and thus to put constraints on the time at which galaxies first formed. In this application, the data from VERITAS will be easily as important as that from GLAST.*"

SAGENAP was impressed with the care the group has taken to be able to run with a lower threshold. For example, the design incorporates expensive FADC electronics. But, as one member says, "*The background also sets the trigger rate, and this is the primary reason given for the FADC electronics: this architecture helps keep the trigger rate manageable, which in turn allows the gamma-ray energy sensitivity threshold to be kept low. This is important, because the overlap in energy between GLAST and VERITAS should be kept as large as possible.*"

SAGENAP found the VERITAS proposal to be built on experience and is confident they can reach the stated experimental goals. To quote one member, "*It seems* *clear that the "upgrade" of the 10 meter telescope to the VERITAS array will succeed.“* The enhanced ability of VERITAS, relative to Whipple was acknowledged and felt an appropriate step. For example, one reviewer comments, *“The proposed designs touts both a much bigger area and lower threshold which is obtained, in part, by observations with multiple mirrors.”*

However, SAGENAP had some questions concerning how the detector has been optimized for the physics. As one member states, “*It was not as clear to me whether the particular choice of a seven telescope array was the optimal configuration to achieve the maximum sensitivity at the minimum threshold; as the low threshold operation seemed to me to be the critical aspect for new physics.”*

The SAGENAP members are uniformly quite positive about this project from the perspectives of our scientific assessment. As one member states, *“It will produce the best instrument in the world for Gamma Ray observations in the 100 GeV to 10 TeV energy range. The observations have a significant expectation of producing new and exciting physics.”* A second reviewer states succinctly, *“In summary, I strongly urge the funding agencies to support the proposal in a timely fashion”… “The team is excellent, their track record is outstanding, and the physics is compelling.”*

In conclusion, SAGENAP endorses the proposal, but feels that a technical and cost/management review should be conducted before the exact funding profile is determined.

KamLAND

This proposal is for a U.S. group to join KamLAND (Kamiokande Liquid Scintillator Anti-Neutrino Detector) and to enhance the technical and scientific goals. The physics goals are to search for reactor neutrino oscillations down to $\Delta m^2 \sim 7 \times 10^{-6}$ eV^2 and to make a measurement of the ^{7}Be solar neutrino flux. The reactor experiment promises to be the first completely terrestrial experiment sensitive in the region of neutrino oscillations indicated from the solar neutrino experiments. The sensitivity goal encompasses the large mixing angle solution.

The detector will be located in the Kamiokande cavern and will contain 1 kton of ultrapure liquid scintillator, surrounded by a 2.5 m thick water Cerenkov shield. The volume will be viewed by 1920 phototubes with 30% coverage and will employ multihit, deadtimeless electronics. Low energy electron antineutrinos will be detected through the inverse β decay process, which are detected in a two step process – a prompt positron followed by a delayed γ ray of 2.2 MeV from the neutron capture process. The detector will be sensitive to antineutrinos emitted from a number of reactors in the general vicinity in Japan at distances of 150 km or more.

The low energy neutrinos from the sun will be detected though elastic ν-e scattering. This is a one step process that is less constrained than for antineutrinos (described above), and therefore requires additional steps in background suppression. In particular, the U/Th/K contamination in scintillator must be at a level $< 10^{-16}$ g/g, as well as the same contamination from the photomultiplier tube glass, surrounding rock, etc. The scintillator needs to be very pure in terms of ^{14}C contamination. The design goal of the KamLAND detector is to obtain an ν-e event rate of ~ 300 events/day, compared to the ~ 50 events/day in Borexino at Gran Sasso. Since the experiments are not background free, the comparison also depends on relative background levels. High statistics measurements are necessary to distinguish seasonal variations expected for the vacuum oscillation solution. The combination of the KamLAND reactor experiment and solar neutrino experiment give observational differences for the different solar neutrino oscillation solutions, using spectrum information, as well as monitoring day/night and seasonal differences.

The KamLAND project is a funded Japanese project that is well underway. The U.S. group proposes to add to the scope and/or enhance the performance in several areas: increase the photocathode coverage, employ multihit and deadtimeless electronics, construct side and roof veto, engineer and construct a mechanical chimney and calibration interface, plus contributions in calibrations and background studies. The total proposed U.S. project costs are estimated (by LBNL engineering, project management) to be \$7.8M. The Japanese commitment to KamLAND is \$20.8M.

The SAGENAP group finds the physics fundamental and very topical, aimed at resolving the solar neutrino oscillation physics. The reactor experiment is unique in that it uses terrestrial sources of neutrinos to access, in a complementary manner, some of the region probed in the solar neutrino experiments. The solar neutrino part of the project promises high statistics if the necessary background rejection can be achieved.

SAGENAP found the proposed contributions to be very ambitious and to significantly overlap areas that SAGENAP would have expected to be within the original scope funded by the Japanese. While wave shape digitization could be an important background rejection tool, the chosen solution appears to be very expensive. SAGENAP is also worried about the realism at this stage of integrating a complex calibration system, which seem to imply a total redesign of the top of the balloon. The team has not fully made the case for $870K management budget, which should have been within the initial scope of the Japanese project.

The addition of phototubes proposed by the US KamLAND participants, which increases the number of photoelectrons from 68 per MeV to 100 is an essential component of pushing the threshold down to the ^{7}Be region. However even though KamLAND has a factor 6 advantage in mass, the number of photoelectrons remains much lower than for Borexino (260/MeV). In addition, the shallower depth leads to dramatic increase of the ^{11}C background, which together with the ^{14}C dangerously brackets the ^{7}Be region. It should be noted that the techniques used by the two groups are very similar and therefore the intrinsic risks appear to be similar. However having two experiments checking each other in a spectacularly difficult measurement may be advantageous.

In general, SAGENAP finds the antineutrino part of the project to be unique and believes that the U.S. group has very relevant experience to join this effort and improve the ability to address the physics questions. Regarding the solar neutrinos, the presence of the Borexino experiment, which is underway at the Gran Sasso makes it less clear whether this proposal offers a significant or necessary improvement over that experiment, or even whether it offers sufficient complementarity.

Individual comments of note from SAGENAP members include the following:

The U.S. participation in the reactor experiment is strongly supported within SAGENAP. One reviewer states, *"I highly recommend support for US groups to participate on the reactor experiment, as described in the 1997 proposal. The US groups bring a wealth of knowledge about reactor neutrino oscillation studies to the experiment."* Another member states, *"This is an experiment where the US collaboration will play a crucial role. They will be a vital force in making this experiment work. They bring expertise in reactor neutrino experiments and low background experiments."*

SAGENAP recognizes the physics capabilities of the reactor experiment and is convinced the experiment can be done. One reviewer states, *"this is the only ground-based experiment which can fully cover the Large Mixing Angle solar neutrino solution. All other experiments require comparing data to the Standard Solar Model."* This reviewer continues, *"the collaboration provided convincing arguments that the backgrounds can be understood and the reactor flux can be well measured."* Another reviewer says, *"The reactor experiment, whose principal goal is to confirm/eliminate the LMA solution, seems uniquely suited to do so. It could be an excellent experiment on a very important topic and it does so in a completely different way than the direct solar neutrino experiments."* And yet another reviewer says, *"The strength of the anti-neutrino aspect of the experiment is that it addresses the "solar neutrino problem"* *without solar neutrinos. This makes it completely independent of the solar model. It completely covers the large angle solution region."*

On the other hand, SAGENAP members stated various concerns about the proposed KamLAND solar neutrino experiment on ^{7}Be. The existence of the Borexino experiment and the large overlap in techniques make the potential contribution unclear. One reviewer states, "*Borexino is in a position to make a decisive measurement of the Be-7 solar neutrino flux. Given the advanced state of the Borexino design and construction, I think it is very likely that the Borexino measurement will be available well before KAMLAND."* Another reviewer comments, *"Other than the potential for 6 times higher rate, KAMLAND does not provide sufficient differentiation in experimental technique to qualify as truly complementary to a successful Borexino."*

In general, SAGENAP felt that this measurement is important enough to warrant two efforts, however, the group did not see where KamLAND brought sufficient complementarily to Borexino to warrant a second experiment. One reviewer comments, *"The major case argued in this proposal is that Borexino has demonstrated that the desired purity can be obtained so therefore KamLAND can do it too. It seems clear to me that since this technique is nearly identical to Borexino, if Borexino can't do it how can KamLAND? But, if Borexino can do it, why should KamLAND do it too?"*

SAGENAP also had concern about physics capability of the KamLAND solar neutrino experiment, especially in the presence of Borexino, which has similar background rejection capability. One reviewer states, "*The statistics of the upgraded-Kamland are expected to be higher than Borexino, however, the proposers did not convince me that they will have better or even sufficient control of the systematics compared to Borexino."*

SAGENAP members had many questions about the scope, specifics and style of the specific proposed U.S. involvement. One reviewer says, *"I support the addition of 600 additional tubes as a high priority. I agree that multi-hit capability is necessary for background reduction. However, I feel that the electronics that have been proposed is unnecessarily expensive. For example, a long time record of the events can be established without looking at individual channels. Several PMT channels can be combined into each electronics channel to minimize cost. Other aspects of the requested budget are inflated."* Another SAGENAP members comments, "*I would strongly endorse US participation in the reactor experiment but not at anything like the price of $7.8M."*

In general, SAGENAP found the U.S. effort contained large items that would seem to be the responsibility of the Japanese group. For example, one SAGENAP member comments, "*The proposal requests a very large amount of money for management. It does not seem possible to effectively manage a Japanese construction project from the US."*

In conclusion, SAGENAP supports the general case for involvement in KamLAND by this strong U.S. group. The reactor experiment is unique and will provide very complementary information to the solar neutrino experiments. The solar neutrino part of the KamLAND proposal is less clear both because of the existing Borexino project at the Gran Sasso and uncertainties about how well backgrounds and systematics can be controlled. We note that the solar neutrino aspects of KamLAND and the need for complementarity can certainly be justified given the difficult nature of achieving sensitivity to these low energies, but the case as presented was unconvincing.

The U.S. KamLAND involvement, as proposed, is more ambitious than SAGENAP feels is warranted and should be limited to 'added value' for the physics goals being proposed. Also, SAGENAP does not endorse the project management and engineering contributions to an existing Japanese project. Instead, the U.S. group should limit their role to direct scientific and technical contributions. There was positive sentiment on SAGENAP to the addition of the PMTs. It was noted that this can only be done now and will make an important contribution.

Axion

The possible existence of the axion represents a central point of convergence between particle physics and astrophysics. The axion is a light pseudoscalar particle resulting from the Peccei-Quinn mechanism to explain strong CP conservation. The hypothetical axion has a mass window of 10^{-6} eV < m_a < 10^{-3} eV/c^2, bounded on the lower end by overclosure and at the upper end by the results from SN1987a. The intriguing implication of these limits is that if axions exist, they have to be cosmologically significant.

The favored mass region depends on whether the Peccei Quinn symmetry breaking occurred before or after inflation. In the first case, a wide mass region between 10^{-6} and 10^{-4} eV/c^2 would lead to a density close to the critical density, depending on the Peccei Quinn angle chosen in our horizon. In the second case (or equivalently if there has no been inflation), the formation of axion strings technically put in doubt the initial estimates favoring the low mass region. The contribution of axion string decay (which is difficult to compute) may push the favored region up.

This discussion of the mass region is relevant as the current search method based on the detection of a weak microwave signal in tunable cavities placed in a high magnetic field has difficulty to reach above 1.5 10^{-5} eV. After a first generation of experiments which missed the required sensitivity by a factor of 10^3 to 10^4, the current experiment run by the proponents is for the first time setting cosmologically significant limits ruling out hadronic axions, in a small mass region around 3 10^{-6} eV. The future running of the present detector will broaden the searched mass range, and the proposed upgrade will allow searching for electromagnetically coupled axion at the required sensitivity in a similar mass range.

Presently, the apparatus is being converted to 4-cavity running, which will broaden the mass window to higher masses (6 10^{-6} eV/c^2). The proposed upgrade consists of improvements to the Cryogenics and SQUID readout, which should reduce the noise level by about a factor of 15. Promising SQUID developments have come through the Clarke Group who have recently joined the Axion collaboration. The proposed program involves parallel efforts to run the axion experiment in the 4-cavity configuration, to develop the hardware for the proposed major upgrade and to perform continued R&D on SQUID detectors.

The proposal presented to SAGENAP is for a 5 year program totaling $7,977M ($3,265M at MIT, $2,315M at LLNL and $1,571M at UC Berkeley). The requested investment appears to breakdown roughly as $1.5M for equipment ($850K for the dilution fridge system and $650K for the SQUIDs). The rest is scientific: $1.1M/yr for experiment running and $180K/yr for SQUID development. The proponents estimate a steady state operating level of $1.2M/year following this upgrade.

There is uniform agreement within SAGENAP that the physics goals are compelling and that the axion represents a prime dark matter candidate. The proposing group has developed the key ideas and the technique being used for searches for the axion, and is the leading group in the world in this research. The experimental accessibility in the proposed program, however, remains somewhat limited. The higher mass regime remains inaccessible and there are some unproven questions whether the full factor of 15 improvement in sensitivity will be realized, as noise sources such as those from the movement of the rods might affect the sensitivity or duty cycle.

The written proposal gives little information about the recent progress (although the oral presentation was more informative) and does not discuss any long-term plan to increase the mass range. The provided financial justifications remain vague and the lack of engineering and technical support is surprising. No substantial justification is given for the running cost of the experiment. The management structure is unclear. The proponents do not propose a specific division of responsibilities between the groups nor of funding between DOE and NSF. Although some more information and clarifications were given in the oral presentations, SAGENAP does not have a precise enough document to recommend this major upgrade at this time.

SAGENAP members endorse continuing the present axion search program at approximately the current level of funding (about $350K/yr). Note that so far this activity has been supported by discretionary funds at Livermore and the NSF Career and DOE Outstanding Investigator grants of L. Rosenberg, which are all coming to an end. SAGENAP supports the continuing development of SQUIDS at a level of approximately $150K per year. SAGENAP encourages further study of the upgrade, potentially leading to a full-fledged proposal addressing the issues outlined above. Such a new proposal could be considered close to the completion of the current four-cavity search.

Individual comments of note from SAGENAP members include the following:

Regarding the science motivation, the potential importance of the discovery of the axion is unquestioned. However, the large proposed costs for the proposed upgrade of the present experiment, coupled with the limited discovery reach in axion mass, prompts SAGENAP to endorse a more step-wise approach to this program. One member states that *"the discovery of the axion itself and confirmation of its role as dark matter would be a major event in physics and cosmology. Nonetheless, the $7.98M requested and the prospect that achieving technical success in one region (but not yet a discovery) will lead to further pushing of the technological limits puts it in a class of experiments which in themselves take a major portion of the non-accelerator program budget" … "Lacking the discovery, it would set a significant limit but would make no other measurements and it is expensive"* .

The programmatic aspects of the proposal are generally endorsed by SAGENAP. One member states that *"the present experiment has reached the sensitivity limit which was their goal and they are continuing to increase their coverage of the mass parameter space by the addition of cavities. This is important and worthwhile to do."*

The SAGENAP members are generally positive about the proposed major upgrade built around the developments of SQUID technology. One member states: "*The chief determining factor in setting the level of investment is the need to be at a lower temperature to take advantage of the SQUID technology. The additional investments explicitly included in this proposal are a new fridge to get into the ~100 mK range, new electronics and magnetic changes to shield the SQUIDS. These are all essential"*.

However, members have some concerns whether the full factor of improvement can be realized. For example, one member stated a specific concern that "*they have not* *yet shown that all the vibrational input to the system from the piezzo-electric motor is taken into account. If not, then they perhaps cannot maintain the kind of frequency scan rate they claim because they will have to wait for re-cooling."*

In summary, SAGENAP recognizes the importance of the search for axions and the contributions to the subject by this group. The continued running with four cavities is a natural extension of the successful present program and SAGENAP strongly endorses this continuation to fully exploit the present generation device. The developments in SQUID technology make the contemplation of next generation axion searches attractive and SAGENAP endorses the R&D program directed at these developments. However, SAGENAP suggests that consideration of a major upgrade be deferred until after the successful completion of the four-cavity experiment. At that time, a detailed evaluation of the physics potential, technical feasibility, time-scale, construction costs and operating level should be assessed.

SAGE

Solar Neutrino Experiments are a key part of the emerging field of particle astrophysics. The detection of neutrinos from the sun in the Homestake Chlorine experiment, followed by the direct detection of solar neutrinos in Kamiokande dramatically verified the fusion burning process in the sun. Beyond this important accomplishment, this work opened up new puzzles when the rates are quantitatively compared to the detailed predictions of the standard solar model. Both the Homestake and Kamiokande fluxes are significantly less than the predictions. These results have prompted much theoretical and experimental work, including the implementation of the critical Gallium experiments. The Gallium experiment is considered fundamental to our understanding of solar ν's because of the sensitivity to the fundamental pp reaction that is the beginning and least model dependent part of the solar fusion reactions.

First generation Gallium experiments have been carried out at the Gran Sasso Laboratory in Italy (GALLEX) and at the Baksan Laboratory in Russia (SAGE). Both experiments have yielded impressive and consistent results. The SAGE result for running from 1990-1997 is 67 ± 7, and they also independently measured the efficiency using a ^{51}Cr source and obtained a result of 0.95 ± 0.12. Again, this measurement verifies the deficit in the solar neutrino flux. The leading explanation for the deficit in these experiments on solar neutrino fluxes is that it is due to neutrino oscillations reducing the flux of ν_e's that reach the earth's surface.

The present experimental results for solar neutrinos, though strongly indicating the presence of neutrino oscillations, do not yield a consistent interpretation. We look forward to a new generation of experiments, SuperKamiokande, SNO, Borexino, etc, that will provide important new information. In addition, follow up measurements with Gallium can be very useful to determine quantitatively how much or whether the actual flux is larger than from the pp reaction alone. Such measurements are being undertaken at the Gran Sasso and this proposal is to also continue the program in SAGE.

The goals of the new SAGE proposal are:

1. Provide quantitative information on the neutrino mass within a natural hierarchy model for neutrino masses for the favored non-adiabatic solution for neutrino oscillations ($\Delta m^2 \sim 6 \times 10^{-6}$ eV^2).
2. Confirm quantitatively that the minimum neutrino flux from the Standard Solar Model of 79.5 ± 2 SNU is consistent with measured fluxes
3. Study any time variation in the pp solar neutrino flux.
4. Preserve the gallium for use in possible future solar neutrino experiments.

The request consists of $435K in equipment funding to improve the SAGE chemistry and operating systems, $120K/year in experimental funds to be spent in Russia, plus $60K/year in operations funding for the U.S. groups participation.

Individual comments of note from SAGENAP members include the following:

SAGENAP agrees on both the importance of this physics and the contributions of SAGE. One member states, *"I am sympathetic to the physics goals. Exploring neutrino masses is one of the more important new fields that has emerged in importance over the last several years. The solar neutrino deficit, first seen in chlorine detectors, has become more convincing. SAGE has been an important contributor to this science."*

Another member states: *"The proposal is to upgrade the existing and very successful (former)* ***S****oviet-****A****merican* ***G****allium* ***E****xperiment which has made significant contributions to our understanding of solar neutrinos." … "I believe they are capable of achieving these goals, that to achieve them they need to make at least the upgrades mentioned above and that they should run at least for the time period outline."*

And a third SAGENAP member states, *"the SAGE Collaboration has made significant contributions to the field in the past and will continue to do so if this proposal is supported."*

Regarding the specific proposal, one member recommended *"that support be provided for requested items 1-4, including shipping and overhead. I recommend that the INR contract request of $120K/yr and operations request of $50k/yr be granted."* The SAGENAP members broadly stated this view. Overall, SAGENAP endorses the continuation of SAGE.

As one SAGENAP member says, *"this experiment has done good science and could continue doing so with modest support from the U.S."*

The SAGENAP members all acknowledge the success of the SAGE experiment and agree regarding the value of continuing the experiment to obtain increased statistics. SAGENAP supports the upgrade of the counting system (attachments 1 & 2), as well as increasing the chemical extraction efficiency (attachment 3) and equipment to minimize the expenditures on consumables (attachment 4). These seem to be necessary and prudent investments, if the program is to be continued. However, there was not support by SAGENAP for the large proposed investment to make the chemical extraction system automatic (attachment 5). The operations expenses ($50K) for the U.S. groups and the equipment support funds in Russia ($120K) to continue this program are supported, but there is some sentiment on SAGENAP that the commitment to this program should not be made for six years at this time.

Fluorescence Detector R & D

The origin of the highest energy cosmic rays is not understood. It has proven extremely difficult to find a mechanism capable of accelerating particles to such high energy in known astrophysical systems or to associate the few highest energy events with known objects that might have accelerated them. In addition, the Fly's Eye and AGASA results indicate a continuing flux beyond the Greisen-Zatsepin-Kuz'min (GZK) cutoff of ~50 EeV. The GZK cutoff is due to energy losses from pion production or photodisintegration in the 2.7 degree background radiation. Therefore, such events are presumed to arise from relatively nearby sources.

The possible existence of such events, plus the fact that the detection of particles with energies of such high energy (even below the cut-off) should give a probe of particles of extragalactic origin and make studies of the highest energy cosmic rays of fundamental interest. These motivations have given rise to a new generation of experiments; HiRes in Utah and the recently approved Auger experiment. The techniques for detecting these high energy particles on the tail of a rapidly falling spectrum are still evolving. The HiRes detector is based on a stereo fluorescence detector, the AGASA detector on detecting extensive air showers at the earth's surface, and AUGER is a hybrid of both techniques.

Determining the viability of extrapolating the fluorescence technique to larger scale detectors will require R&D to determine how far the separation can be increased, how to reduce the unit cost, and to determine the necessary monitoring of the atmospheric conditions, etc. The proposed R&D program is directed at these questions as they apply to developing an array of ~ 10x the HiRes array in Utah. A collaboration with a Japanese group, which is proposing major equipment funding in Japan for such an array in Utah, is being organized.

The SAGENAP members expressed several opinions and some concerns. The general need and importance of enabling R&D toward a next generation fluorescence detector is strongly supported by SAGENAP. However, it is felt that a significant amount of the information that is needed can come out of the HiRes data analysis and that the new work should be targeted to areas that cannot be addressed in HiRes. In particular, there was concern in SAGENAP about developing a new site until that is necessary. It is suggested that it would be more systematic to begin this R&D at the existing sites. The detailed goals for this R&D program are not defined in terms of what must be measured and how well, for example in order to either understand atmospheric effects or to determine the maximum practical separation for a large array.

Several SAGENAP members recommend that the priority at this time should be on the data taking and analysis of HiRes. Though the new groups bring added strength to the collaboration, the proposed R&D program should be carried out following integration of the new members into HiRes. The special questions regarding atmospheric monitoring and separations that cannot be measured and answered with HiRes should be targeted in a well-focussed and minimally interfering R&D program.

Individual comments of note from SAGENAP members include the following:

SAGENAP members recognize the advantages of the fluorescence technique for studies of the highest energy cosmic rays. One member states: *"I believe that the air fluorescence technique used by HiRes is intrinsically better than that of the EAS technique in determining total shower energy and particle type. This is a very important feature in this application where the small numbers of events at the extreme energies could be due to fluctuations in the tails of distributions."* Another reviewer states, *"Fluorescence detectors have the advantage over surface detectors in their ability to image the shower profile. This gives higher confidence in the energy estimates as well as the ability to look at composition. Plus it allows for the discovery of exotic phenomena."*

The goals of this proposal are regarded by SAGENAP as crucial to accomplish before a proposal for a large array can be developed. One reviewer states: *"I believe that the optimization of the air fluorescence technique is an important project. The most challenging aspect of the R & D is to determine the maximum useful separation distance between telescopes. The larger the possible separation, the more cost effective the array becomes."*

However, there is not uniform support for all parts of this proposal on SAGENAP. There is general sentiment that as much as possible should be done at the present sites and with the HiRes data before investing in a new site. For example, one members comments, *"Certainly some, maybe most of this work should be supported. It seems, however, a bit premature to go for development of the Black Rock site at this time."* Another member comments on the desirability of a phased approach to the R&D, *"Rather than working all at once on all these topics, it seems sensible to suggest a more phased approach. First, test the optics, electronics and mechanical infrastructure to whatever extent possible at the existing site, and then develop the new site."*

There was general recognition of the potential importance of the atmospheric studies. For example, one reviewer comments, *"The only thing I found compelling about this proposal was the request for R&D on atmospheric monitoring. I am persuaded that not enough is currently known about the fluorescence absorption and Cerenkov effects in the atmosphere without further research to make good use of flourescence detectors on the larger baselines envisioned for future efforts in this field. I would expect that the knowledge gained from this work could also be of significant use to the AUGER group."*

The understanding of the atmosphere is based on a one-dimensional model. The parameters of that model must be determined and whether that model can be used in extending the technique to larger spacing for a future array. One SAGENAP member comments, *"To work effectively, fundamental atmospheric characteristics must be measured. We were told that four parameters are necessary to describe a one-dimensional atmospheric model: the extinction and scale-height for both aerosol and molecular components. The molecular parameters are apparently known to 5% from Rayleigh scattering, but the aerosol parameters are unknown and must be measured. These parameters must be known to better than 10-20% to have better than 10% knowledge of the overall energy scale."*

Overall, the SAGENAP group feels the goals of this R&D effort are important for a possible next generation fluorescence detector as is being considered in collaboration with the Japanese. SAGENAP notes that the collaboration has been recently expanded, which should create the strength to carry out this R&D program. However, SAGENAP recommends this be done in the context of the new groups integrating into the day to day running and data analysis on the new HiRes detector. This will educate them to the real issues in such an array, enable their participation in the science of HiRes, and allow an incremental approach to the R&D toward a future array. We encourage the promising developments of collaboration with the Japanese, and note that this R&D program should also be useful to Auger and their application of fluorescence detectors, as well as plans for a future Northern Hemisphere detector.

Homestake Iodine Detector

The Homestake and Kamiokande experiments verified the basic fusion process in the sun, while at the same time, they measured fluxes that are significantly less than the predictions of the Standard Solar Model. These results prompted both theoretical and experimental work, including the critical Gallium experiments, which are sensitive to the fundamental pp reaction, that is the beginning and least model-dependent part of the solar fusion reactions.

The present experimental situation strongly supports a neutrino oscillation hypothesis, as well as an absence of ^{7}Be neutrinos. The new generation of experiments, SuperKamiokande, SNO, Borexino, etc should provide important new information that will help us to determine the underlying physics.

The proposed Iodine experiment is focussed on the ^{7}Be question in a radiochemical experiment that promises to have both large enough rate and fast enough extraction to measure day/night effect, as well as to become a supernova detector. The proposal is to develop a detector of 1000 tons in stages, expanding first from 100 tons to 300 tons. The first three year proposal using a 100 ton Iodine detector requests $280K equipment funding, plus $200K/year operations expenses. Each additional 100 tons would cost approximately $100K.

The SAGENAP found the idea interesting and the resolution of the ^{7}Be question to be very important. However, several problems were identified.

The 100-ton device is not large enough to do a definitive experiment to determine the flux of ^{7}Be neutrinos. It can measure the rate to 5% in 6 years, which SAGENAP members feel is too long. The sensitivity of 300 tons (5% in 2 years) would be more adapted to the current state of the field.

Although the concept of a low running cost of a radiochemical detector for doing a supernovae watch is attractive, the low expected rate of galactic supernovae (at most a few per century) decreases the likely return of this investment. Again, the present detector is too small to be a supernova detector and this will require the full 1000 tons. About $1M would be necessary to bring the detector to the mass of 1000 tons, which is necessary to measure unambiguously the potential effect of oscillations of the electron neutrinos on the temperature.

Although the cross section measurements are now precise enough to confirm the high rate expected on iodine, the cross sections cannot be indirectly measured precisely enough for the full interpretation of the solar neutrino data. In particular, the transition to the excited level of Xe cannot be computed reliably and the full calibration of the experiment will have to be made with a radioactive source. There was no detailed plan presented to accomplish this and it may require significant resources and time to accomplish. Since the experiment measures the flux, rather than also measuring the neutrino spectrum, uncertainties in the cross sections would make the interpretation unreliable.

Individual comments of note from SAGENAP members include the following:

SAGENAP finds the scientific case well motivated. One member states that *"The Iodine experiment in this proposal represents an opportunity to test the Chlorine result."* Another member said, "*the scientific motivation is very good and is directed toward detection of neutrinos from galactic supernovae and solar neutrinos in the 1 MeV portion of the spectrum*".

However, in terms of actual implementation one of the SAGENAP members continues that "*for application as a galactic supernovae detector, the prototype would not be adequate; it would have to be increased to the full size 1000 ton detector".*

SAGENAP was also somewhat concerned about whether the group is adequate to carry out this experiment. One member says, *"I am concerned that the experiment is understaffed. I recognize that the experiment does not require a lot of personnel. Nevertheless, I would recommend seeking at least one more active collaborator to help with the project*

In conclusion, the SAGENAP members do not feel the case has been made to make the large proposed investment in an Iodine experiment and the SAGENAP members expressed concern about the small size of the group or lack of collaborators to participate in such a large project. More generally, SAGENAP feels it is debatable whether the investment of 1-2 M dollars in a radiochemical experiment, including at least 300 tons of target and the calibrating source, is the right step for solar neutrinos at this time. In spite of their higher costs, investment in real time experiments seems more appropriate.

CDM – TPC

The nature of the dark matter is one of the central issues in astrophysics, and supersymmetry is the favored theory for physics beyond the standard model in particle physics. These two fields come together in the possibility that the dark matter particles are Weakly Interacting Massive Particles (WIMPs) with the particle physics candidate being supersymmetric particles (e.g. neutralinos). After a decade of development, direct searches at the required sensitivities for these dark matter candidates can be anticipated in detectors sensitive to the very small recoil energies (~ 10 KeV) in elastic scattering of these dark matter particles in matter.

The CDM-TPC proposal is aimed at the development of a WIMP dark matter detector that uses a high resolution TPC. The idea is to develop a technique that will be background free, by obtaining maximum information per event, including in this case directional information. In principle, this will allow a more sensitive search if the background levels are indeed smaller. To accomplish this goal, the experiment must be performed deep underground. The proposal to the NSF is for support of groups from Temple University and Occidental College to work toward these goals.

The low pressure TPC technique is promising since it will yield measurements of ionization and give some directional information, in particular the effective range and direction of the recoil. The detection of the direction of the recoil would provide the ultimate confirmation that it is due to dark matter linked to the galaxy, as the mean direction will rotate during the day. In order to obtain better transverse resolution and to obtain longitudinal information, the group proposes a variant of the TPC technique by using a negative ion drift chamber. This promises better control of transverse and longitudinal diffusion. In addition, another nice feature is that no magnet is required.

This approach has possible advantages over previous schemes, as the radioactive background likely to be generated by the magnet was a clear disadvantage. Electronic recoils are recognized by their range, and the remaining contamination is either due to abnormal curling of the track or insufficient sampling by the detecting wires. The group has obtained a first experimental indication of a rejection of 99.9% at 6 KeV (equivalent energy corresponding to 15 KeV). Low energy alphas originating from the wires may mimic nuclear recoils and particular attention has to be given to the radiopurity of the cathode wires. The approach is interesting although the density is low (100 g for 1 m^3) requiring eventually a much larger set up with a very large number of channels.

The proposed work will be done in collaboration with the larger UK DRIFT program being developed for the Boulby Underground Site in northern England and at 3000-mwe depth. The U.S. group proposes to build a cubic meter chamber in collaboration with UK. If the background is as low as expected, this detector will have about 10x the sensitivity of DAMA and have a clear path to scale it up to a larger detector. The U.S. group proposes to contribute in the areas of electronics and DAQ, mechanical construction and vacuum and the requested budget (operations and equipment) is about $200K/year for three years at which time the 1 m^3 detector will be available. They propose to use STAR electronics for the readout, which could be an economical solution, but SAGENAP members wondered whether the low per channel cost is realistic. The UK contribution over this period amounts to ~$250K, plus 2 FTEs.

The SAGENAP group agrees on the fundamental importance of the search for WIMP dark matter. These searches require significant R&D in order to develop detectors of sufficient sensitivity. Moreover, the detection of the direction of the recoil would be the ultimate proof that the detected events are linked to the galaxy. In case a convincing signal is discovered by previous generation experiments, directionality will be an essential element of an experiment designed to fully confirm such a claim, and low pressure TPCs may be the simplest way to do it.

SAGENAP acknowledges the progress of the R&D program and the potential of this technique. Some individual comments from SAGENAP members include the following:

SAGENAP agrees on the importance of this physics problem. As one member states, "*The prospect that there is a sizable local density of a totally new type matter passing through us unmeasured is enormously compelling."* This reviewer goes on by noting *"the exciting possibility of detecting these particles directly via elastic scattering in the laboratory."*

The SAGENAP members were positive about the importance of the innovative R&D contributions of this group. One member comments, *"The idea to use negative ion drift in a TPC is a very clever one and as the field of direct detection of dark matter experiments gets into high gear – as it is now beginning to do – the ability to have a technique sensitive to directionality and good event signature will be invaluable.*

The potential of this technique for future searches for the dark matter is intriguing. As one SAGENAP member comments, *"If the directional capability were to be demonstrated, construction of this detector would be an important new step forward in this field."*

On practical grounds, however, SAGENAP is concerned at the strength of the group to undertake the actual construction, yet feels they are correctly directed toward developing a real detector that can perform a sensitive search. One member comments, *"My main concern is that this effort appears to be sub-critical. There will be significant help from UK collaborators, but the U.S. group probably needs more dedicated personnel. Anything the agencies can do to help in this regard would be worthwhile."*

In conclusion, SAGENAP finds that this small group has done a very nice job of demonstrating the promise of using negative ion drift chamber technique. The proposed program for the next three years is ambitious and SAGENAP members are concerned whether the group is undercritical in size, especially with the proponents' large teaching obligations. On the other hand SAGENAP applauds the involvement of researchers from teaching schools in research in this field and urges the agencies to be creative in their support to enable them to participate effectively.

GLAST – NSF Participation

GLAST is a high energy gamma ray detector proposed as a follow up to the very successful EGRET mission. EGRET has identified a rich sky of objects yielding gamma rays up to ~ 1 GeV. EGRET discovered gamma-ray quasars, prolonged gamma-ray emission from gamma ray bursters, 170 previously unidentified sources, delayed GeV emission from solar flares, a gamma ray glow in the direction of the milky way, etc. A follow up mission seems one of the best motivated projects for the coming decade. NASA is expected to release an “Announcement of Opportunity” (AO) in mid June, with proposals due in September.

As far as we know, two proposals will be submitted: "Silicon-GLAST" (P. Michelson, PI) and "Fiber GLAST” (Geoffrey Pendleton, PI). They differ by their charged particle detector, silicon in the first case and fiber in the second), as well as in the calorimetry (CsI crystal in the first case and sampling fiber readout in the second). The DoE participation in Silicon GLAST has been reviewed previously by SAGENAP and the DoE has committed to the project. The present proposal is for a group of traditionally NSF funded Universities to participate both in the science and the instrument construction. The proposing groups have been an integral part of the design process for GLAST.

GLAST will have sensitivity capable of detecting many Active Galactic Nuclei (AGN), extend the measurements of the gamma ray spectra and search for sources into the presently blind region (beyond EGRET) up to 300 GeV nicely overlapping the proposed ground based observations in VERITAS. In addition to quantitative follow-up of the many important physics measurements of the sources, new physics will be opened up by the increased sensitivity. For example, the study of supernova remnants promises to yield information on the origin of cosmic rays. In general, the factor of 100 improvement over EGRET will likely lead to new physics not yet anticipated in this young field.

GLAST (and VERITAS) will provide complementary indirect search for supersymmetric dark matter to that looking for neutrinos resulting from WIMP annihilation in the center of the earth or sun. The predicted signal is a monoenergetic gamma ray at $E_\gamma = m_\chi$ and/or $E\gamma = M_\chi(1 - M_Z^2/4M_\chi^2)$ that result from neutralino annihilation. If neutralinos comprise the dark matter and are sufficiently concentrated near the center of the galaxy, presently a controversial point, GLAST could have sensitivity for detection. The required good energy resolution to enable searches for high energy gamma ray energy structure has been designed into the detector. No other known processes are expected to produce narrow lines, so the observation would be strongly suggestive of WIMP annihilation.

The submitted proposal is for participation in GLAST by groups from University of Chicago, American University, UC Santa Cruz, University of Utah and the University of Washington in GLAST with an emphasis on the physics of Gamma Ray Bursters. The original proposal (submitted 1/99) proposed an equipment contribution of a context instrument for gamma ray bursters. However, in response to the NASA AO, the collaboration has dropped plans for the context instrument. Instead, the group has changed their proposed hardware involvement to the main detector, primarily in the

Silicon tracker, plus contributions to the calorimeter. No details were given of this involvement because the decision to change their involvement was made very recently. A total NSF supported hardware contribution of the scale of $5-7M is being proposed.

The SAGENAP group finds the science of GLAST compelling and the potentials of the new GLAST mission a significant advance. The SLAC/University proposal for GLAST brings strong HEP groups and technologies to bear on an important mission in space. The University groups have been centrally involved in the design of GLAST and are expected to play an important role in the implementation and science.

SAGENAP strongly endorses the participation of the NSF groups to Silicon - GLAST. It would have been of great value to have an NSF commitment to these groups participation before the submission of the NASA proposal, so that it would be clear that GLAST is a three-agency program: DOE, NSF and NASA. The difficulty is one of timing, as the NSF does not have yet a revised equipment proposal. Moreover, the preliminary idea floated at the meeting (NSF participation in the purchase of silicon) does not have the appeal of an well-identified and unique NSF contribution. SAGENAP encourages the group to rapidly submit a revised proposal with a well thought out and motivated NSF hardware participation. If such a proposal is submitted SAGENAP advises NSF to find ways to rapidly review this proposal, so that the NSF participation can be inserted into the NASA process at the earliest possible time.

Individual comments of note from SAGENAP group members include the following:

The scientific motivation and goals of this group's involvement in GLAST is supported by SAGENAP. One member states, "*the group has developed a proposal based on a united interest in Gamma Ray Bursts. The source of these bursts represents an exciting, open scientific question. They are wise to combine their various complementary skills in attacking this interesting question.*"

There is recognition on SAGENAP that the NSF University groups have been strong contributors to GLAST during the design phase and that this group can bring considerable strength during both the construction and scientific phases. SAGENAP endorses this University group's participation in GLAST, as one reviewer comments: "*these are good people; b) the physics of GLAST is very good and it would be great for them to participate in it as real partners; c) they can make valuable contributions (as many of them have been doing already) through their modeling, calorimetry and beam testing. I would expect they would tool up to participate in the fruits of the project.*"

Although the submitted equipment proposal is being withdrawn, one SAGENAP member comments, "*their proposal represented an innovative addition to the GLAST detector which helped advance the scientific goals of the proposers. The quality of the proposal for this detector should be taken as an indication of the high capability of this group.*

Overall, SAGENAP is enthusiastic towards GLAST and the participation of the NSF-based Universities. The focus of the group around the physics of Gamma Ray Bursts provides an important scientific unifying theme. SAGENAP encourages the group to resubmit a new integrated proposal, including both the scientific role and any equipment proposal. As one SAGENAP member states, "*I think the GLAST science is so* *interesting, and the Oreglia et al. group so strong and energetic, that the universities, GLAST management, and the agencies should make it a priority to find a way to incorporate the NSF-supported university groups in a sensible construction and operations role. I think it would be a shame to have this opportunity lost."*

<u>**Neutrino Burster Experiment**</u>

NuBE

A preliminary proposal was presented of a concept for a specialized experiment to detect high energy neutrino production associated with gamma ray bursts. The idea is to detect very high energy muons produced by neutrinos deep underwater. Specifically, a site in the Caribbean has been suggested at 4 km depth. A recent paper by Waxman and Bahcall using a fireball model for the gamma ray bursts predict such a high energy neutrino flux.

Experimentally, the idea is to build a large area detector that can detect these high energy events in coincidence with the observed gamma ray bursts. The array consists of 4 strings with 2 nodes each and spaced by 300 meters. An innovative concept is to transmit the signal acoustically.

The proposed cost of this experiment estimated to be $3.6M.

SAGENAP members agreed that it was premature to bring this proposal to the group at this time. The problem of understanding gamma ray bursts is important and a modest R&D program toward underwater detector development seems warranted. This group might well try to collaborate with the larger groups developing under water or under ice detectors to pursue the experiment within that context.

Overall, SAGENAP was intrigued with some of the ideas presented and one member succinctly said, *"He may be on to something interesting; let's wait and see."*

SAGENAP Meeting

April 12-14, 1999

April 12, 1999, Room 375, National Science Foundation, 4201 Arlington Boulevard, VA 22230

9:00 AM	Executive Session
9:30 AM	Introduction
9:40 - 10:40 AM	VERITAS 1st hour
10:40 - 10:50 AM	Q&A
10:50 - 11:05 AM	Break
11:05 - 12:05 AM	VERITAS 2nd hour
12:05 - 12:15 AM	Q&A
12:15 - 1:15 PM	Executive Session Lunch
1:15 - 2:30 PM	New Axion
2:30 - 2:40 PM	Q&A
2:40 - 3:10 PM	Axion
3:10 - 3:20 PM	Q&A
3:20 - 3:35 PM	Break
3:35 - 4:35 PM	KamLAND
4:35 - 4:45 PM	Q&A
4:45 - 5:45 PM	KamLAND
5:45 - 5:55 PM	Q&A
5:55 -	Executive Session

1 representative from each group to stay until the end of the executive session to receive a list of questions to be answered by tomorrow morning.

Second day, April 13, 1999, Room 375, National Science Foundation, 4201 Arlington Boulevard, VA 22230

8:30 AM	Executive Session
8:45 - 10:00 AM	VERITAS, Axion, KamLAND Q&A
10:00 - 11:00 AM	SAGE
11:00 - 11:15 AM	Break
11:15 - 12:15 AM	Fluorescent detector development
12:15 - 1:15 PM	Lunch
1:15 - 2:15 PM	Iodine Neutrino Oscillation Detector
2:15 - 2:35 PM	Executive Session
2:35 - 3:35 PM	CDM-TPC
3:35 - 3:50 PM	Break
3:50 - 4:50 PM	GLAST-Addition
4:50 - 5:20 PM	NASA Interstellar Initiative/Other important items
5:20 - 6:05 PM	NUBe
6:05 - open ended	Executive Session

1 representative from each group to stay until the end of the executive session to receive a list of questions to be answered by tomorrow morning.

Third day, April 14, 1999, Room 375, National Science Foundation, 4201 Arlington Boulevard, VA 22230

8:30 - 8:45 AM	Executive session
8:45 - 10:00 AM	Remaining questions for VERITAS, Axion, and KamLAND
10:00 - 10:30 AM	Break

10:30 - 11:45 AM	SAGE, Fluorescent, Iodine, CDM-TPC, GB on GLAST Q&A
11:45 AM	DRAFT Report production
6:00 PM	Adjournment

SAGENAP Members

Barry Barish - Coordinator
Janet Conrad /Columbia
Jordan Goodman /U of Md
Thomas Gaisser /Bartol
Robert Lanou /Brown
Steven Ritz /NASA/GSFC
Leslie Rosenberg /MIT
Bernard Sadoulet /UC Berkeley
Hank Sobel /UC Irvine

Marvin Goldberg	NSF	
Gene Loh	NSF	730-306-1895 FAX 703-306-0566
Jim Stone	DOE	301-903-0535 FAX 310-903-2597
P.K. Williams	DOE	

Proposal Representatives

Karl van Bibber/LLNL	Axion
William Goldstein/LLNL	Axion
Michael Kreisler/LLNL	Axion
Charles Alcock/LLNL (not confirmed)	Axion
Pierre Sikivie/U.of Florida	Axion
Neil Sullivan/U.of Florida	Axion
David Tanner/U.of Florida (not confirmed)	Axion
Leslie Rosenberg/MIT	Axion
Steven Asztalos/MIT	Axion
Robert Redwine/MIT	Axion

Chicago/Simon Swordy	VERITAS
FLWO/SAO Trevor Weekes	VERITAS
FLWO/SAO Vladimir Vassiliev	VERITAS
FLWO/SAO Steve Criswell	VERITAS
Iowa State University/ Frank Krennrich	VERITAS
University of Leeds (UK)/ Joachim Rose	VERITAS
Purdue University/ John Finley	VERITAS
Purdue University/ James Gaidos	VERITAS

University of Utah/ David Kieda	VERITAS
Washington University/ James Buckley	VERITAS
Temple/ Jeff Martoff	TPC-Dark Matter
UCLA/ K. Arisaka	Fluorescence Det
LBL/ Hank Crawford	NUBe
Penn/ K. Lande	Neutrino Oscillation
LBL/ S. Freedman	KamLAND