DEPARTMENT OF ENERGY

DIVISION OF HIGH ENERGY PHYSICS

REPORT

ON THE

SCIENTIFIC ASSESSMENT GROUP

FOR EXPERIMENTS IN

NON-ACCELERATOR PHYSICS

(SAGENAP)

FEBRUARY 20-21, 1996

P.K. WILLIAMS, CHAIRMAN

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INTRODUCTION

The Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) was assembled by the Division of High Energy Physics (DHEP) of the U.S. Department of Energy (DOE) in response to the advice given by the High Energy Physics Advisory Panel (HEPAP). This group will provide a standing review of the on-going research program in non-accelerator physics supported by DHEP. The charge to the group is shown in Appendix A. Meetings of SAGENAP will be chaired by a member of the high energy physics staff. Dr. P. K. Williams of the DHEP is the present chair. SAGENAP members were selected (Appendix B) by DOE in consultation with the National Science Foundation (NSF). The length of service on SAGENAP will be adjusted to assure the continuity of the review process and the right mix of expertise to review the proposed program. It is envisaged that typically the group will meet biannually.

Since the non-accelerator experiments involve scientists and projects supported not only by DOE and NSF, but also by the National Aeronautics and Space Administration (NASA) both NSF and NASA representatives were included in the meeting of SAGENAP. The first meeting of SAGENAP took place in Washington, D.C. on February 20-21, 1996. The purpose of the meeting (Agenda, Appendix C) was to review and make recommendations on four pending proposals to DOE-HEP: DUMAND — an underwater neutrino detector, GRANITE III — a ground based high energy γ-ray telescope, an experiment to search for dark matter via gravitational lensing of quasars, and GRSST — a prototype development for GLAST, a broadband high energy γ-ray space telescope using silicon strip detectors and cesium iodide crystals. Status and progress reports were given on the following experiments in progress: MILAGRO, MACRO, SOUDAN II, Super-Kamiokande, Auger project and Cold Dark Matter Search (CDMS). This report contains summaries from the review letters written by the members of the group and the recommendation from DHEP based on the inputs from the review committee.

Special thanks goes to the DHEP staff, particularly J. Mandula and T. Romanowski, who put together most of the body of this report.

DUMAND (Deep Underwater Muon And Neutrino Detector)

The DUMAND detector is an array of phototubes to be located at the bottom of the ocean 30 kilometers west of the Big Island of Hawaii. The phototubes are to detect the Čerenkov light from charged particles produced near the detector by very high energy neutrinos. From the pattern and timing of the detected Čerenkov radiation, the energy and the direction of origin of the incoming neutrinos would be determined.

Physically, the array would comprise nine vertical "strings", each holding 24 phototubes and ancillary calibration and monitoring instrumentation. A junction box and a single cable to shore links the array of strings with a laboratory on the Big Island. The DUMAND project and its construction has been separated into two phases; the first is the shore cable, junction box, and the first three strings, and the second is the remaining six strings. The original deployment concept called for one string to be connected to the junction box when it was lowered to the undersea site, and for the remaining strings to be attached undersea with the use of a deep submersible vehicle.

The DUMAND project has been a collaboration between groups from the U.S., Japan, Switzerland, and Germany. The University of Hawaii has been the lead institution. Nearly all the phototube modules provided to date have come from the Japanese participants.

At the present time, the shore cable and junction box are in place (although the condition of the latter is unclear), and the first three strings are mostly assembled and tested. The project has had a number of mishaps, especially associated with deployment operations.

The current proposal requests funds for the testing and/or repair of the junction box, and completion and deployment of the remainder of Phase I of the project.

The SAGENAP reviewers addressed all aspects of the DUMAND project in general and the current proposal in particular in great detail. A summary of their evaluations and judgements which follow can be grouped into several general categories.

Significance and Appropriateness (for DOE support) of the Proposed Research

All the reviewers who commented on it said that the physics case for pursuing neutrino astronomy is strong, perhaps more so than when DUMAND was originally conceived. The scientific goals of DUMAND are to detect high energy neutrinos from astrophysical sources. Potentially, this is a new probe of the Universe that could give us insight into the various astronomical objects like Active Galactic Nuclei (AGNs), acceleration mechanisms for high energy cosmic rays and even the origin of these cosmic rays. The detection of astrophysical sources of TeV-PeV neutrinos, it was asserted, would be a major contribution to high energy astrophysics with the potential for significant contributions to high energy physics as well. By this assessment DUMAND is within the scope of the DOE Division of High Energy Physics.

Neutrinos are a unique tracer of energetic hadron acceleration in sources. Whereas photons can be the product of hadronic or electromagnetic processes, such as inverse Compton scattering or synchrotron radiation of energetic electrons, neutrinos arise mainly from the decay of particles produced by hadronic interactions. As a consequence, neutrinos from astrophysical sources may act as probes of fundamental particle physics or astrophysical particle acceleration mechanisms.

On the other hand, it is now generally believed that a functioning 3-string (or 9-string) DUMAND detector may be too small to make significant high energy neutrino observations. Recent results from γ-ray astronomy, which have not confirmed early evidence for high energy γs from Cygnus X3 and other sources, indicate that although there are VHE γ-ray sources, but they may not necessarily be neutrino sources, and if they are, they are probably weaker than originally thought. Instead, a facility with an active region on the order of a cubic kilometer may well be required to do this physics. From this point of view, DUMAND would be considered a step along the way to such a facility — a large scale prototype and engineering trial — but not itself a powerful neutrino observatory.

Related Projects

The possibility of deep ocean detectors was discussed as early as the 1960s, and some form of the DUMAND detector was proposed during the 1970s. Three other related detector projects are under way at this time. The Baikal experiment began in 1980. Its site is not nearly as deep, but it has been operating for some time. A European detector project called NESTOR is planning to deploy towers of planes of detectors in the Mediterranean near the Greek island of Pylos.

The fourth project imbeds the detector modules in deep ice, at the South Pole. This detector, called AMANDA, has begun to report early, mainly engineering results. They have, after two seasons of deployments, four working strings of phototube modules at a depth of about 800m, and a like number at about 1900m.

Assessment of Progress to Date

The DUMAND project, which initiated the field of high energy neutrino astronomy in the U.S., has made considerable progress but has also been plagued by difficulties. One string is complete and two others nearly so. The collaboration successfully deployed one string and the junction box in the deep ocean and laid the cable to the shore station at the end of 1993. The string stopped operating after a few hours due to a leak, and it was recovered in January 1994. A leak in the junction box caused it to fail in February 1994. An attempt to repair the box with an undersea robot in 1995 was not successful. The overall impression was that of a collaboration encountering a never-ending series of technical problems. As each was understood and fixed another seemed to arise.

The DUMAND collaboration has not succeeded in its initial goal of deploying a working 3-

string detector. The committee identified several possible causes for this failure:

(a) A general lack of rigid quality control, and a freewheeling style toward new ideas and improvements. Essentially every member of the SAGENAP group expressed the view that the present DUMAND management had displayed a lack of detailed planning and careful attention to quality assurance deemed essential for the success of a project like DUMAND. The SAGENAP group was also disappointed in the DUMAND collaboration's response to the DOE review conducted in October, 1994, which had noted serious quality control and planning deficiencies. The general view was that the collaboration had not taken any significant steps to remedy these deficiencies (despite their assurances that they had).

(b) The small size of the collaboration and the lack of engineering support. The view was expressed that the problems that confronted DUMAND actually required vastly more manpower and other resources than the collaboration had envisioned.

(c) A serious underestimate of the resources required for the costs of apparatus and of deployment. DUMAND received all of the funding that was planned for the first stage, indeed about 40 percent more than was originally requested. However, each reviewer that addressed this issue said, at least in hindsight, that the required resources had been seriously underestimated. The view was that the resources necessary to mount a successful underwater operation would be very different than what had been conceived, perhaps requiring the infrastructure of a national laboratory and/or oceanographic institutes. It was felt that such an experiment would need the routine availability of a remotely operated underwater vehicle and ship time, and a great deal more input from (non-physicist) experts who specialize in working in the deep ocean. Some of these resources were to be requested of other funding agencies if needed. Many of these thoughts were brought up in a series of technical, cost, schedule and management reviews of the project by DOE. However, except for the most recent one, which took place after the first stage deployment failure, these reviews conluded that an incremental approach was appropriate, solving problems as they arose.

(d) The concept of a deep underwater junction box. At present it is not known if the junction box at the ocean floor can be repaired or is irreparably damaged. Reliance on the ability to make electro-optical connections at depth was certainly seen as a serious error, perhaps an insurmountable one.

(e) The hostility of the deep ocean environment and of the difficulty of working there. Several reviewers expressed the view that the collaboration led by the Hawaii group does not have a good track record, that their plan for repairing the junction box does not inspire confidence, and that the problem is still not well understood.

DUMAND is a medium sized project being executed in a hostile oceanic environment. The project is being implemented without the infrastructure of a DOE national laboratory or comparable institution. Significant engineering and management shortcomings have produced a situation in which major system components (especially the Junction Box) are deployed, are not working, and the cause of the failure and possibility of repair are not well understood. An appropriate engineering and management structure, while increasing the total cost of the project significantly, seems necessary if such a project is to have a good chance of reaching a successful completion.

In a sense, the difficulties experienced by DUMAND have already provided an answer to an important engineering question: it is going to be very difficult to complete an array under the sea, and it is going to be very expensive. Further work in the present mode was not advised by any reviewer.

The DUMAND Team

The reviewers noted many positive and negative attributes of the DUMAND team. They were described as pioneers who have made real contributions to every aspect of the field. Furthermore, the international collaboration, led by the group at Hawaii, was said to have made considerable progress.

However, the DUMAND collaboration has been plagued by delays and technical difficulties, and the collaboration, led by the Hawaii group, has not had a good track record in dealing with them. The universal sense was that the leadership of the project operates in a freewheeling style that is the antithesis of careful planning and quality assurance and control. This fatal failure was laid squarely on the project leadership, whose managerial skills and commitment to the project were strongly deprecated. Many reviewers said that the DUMAND "Project Director" should have been a strong Engineer/Manager of the NASA project variety.

Feasibility of Plans

The current DUMAND proposal requests funding to complete, test, and deploy the first three strings, and to carry out operations to repair the junction box.

In general, reviewers expressed their belief that the problems of doing neutrino physics in the deep ocean are not insurmountable, and that the DUMAND concept can be made to work. In fact, the fundamental questions for operating such a device in the deep ocean, such as water purity, bioluminescence, etc. appear to be manageable. However, reviewers stressed that to succeed such a project needs to be carried out with much better planning and much more attention to reliability than the DUMAND team has displayed. The feeling was that even if the funds to pay the vastly increased costs of the proper ocean support were forthcoming, their use by the present DUMAND working group would be very unlikely to lead to success.

Although reviewers felt that completing the three-string phase of DUMAND was desirable, especially to prepare for a much larger detector, no reviewer recommended going ahead with the proposed work. The clear sense of the reviewers was that, through the lack of detailed planning and painstaking attention to detail, the prototype Triad development and deployment were not being executed in a way which could lead to a successful outcome. Rather, the sense of the committee is that the problems encountered by DUMAND are completely a result of the technical implementation. One reviewer after another said that he lacked confidence that the present DUMAND group is capable of successfully deploying the strings. Those that addressed the junction box specifically said that problem of repairing it is not well understood, and as such the plan presented inspired no confidence.

DOE-HEP RECOMMENDATIONS

Further support of the DUMAND project is not recommended. At base, the reason is the track record of the collaboration in its work to date, and a judgement that the DUMAND team, including its leadership, lack the skills needed to carry out such a complex project in the unforgiving environment of the deep ocean. The project should be terminated, and its operations phased out in an orderly manner.

GRANITE III: A γ-ray Telescope for Next Millennium

The GRANITE III proposal by the Whipple γ-ray collaboration outlines possible upgrades of the present 10m Whipple atmospheric Čerenkov imaging telescope GRANITE II in order to increase its flux sensitivity, lower its energy threshold, and improve its energy resolution. GRANITE II is an optical reflector with a 10m diameter composed of 248 mirrors, each with a 7.3m focal length. The image of Čerenkov light is focused on a multielement camera placed above the reflector. Characteristics of the images of the cosmic ray showers depend on the type of particle originating the event. High energy γ-rays from discrete sources in space produce narrow electromagnetic cascades whose axes point towards the source. Cosmic ray showers generated by hadrons form extended images with much less pointing ability.

It is proposed to replace the 109-pixel camera on the 10m optical reflector by a 541-pixel camera with smaller pixels and an enlarged field of view. This modification would increase the angular resolution at all energies in the region from 100 GeV to 10 TeV and allow for energy measurement below 100 GeV. The present Čerenkov imaging telescope, with its achievable sensitivity, supplements observations made by the Compton GRO in the region where the EGRET γ-ray detector cuts off, at 30 GeV.
A second part of this proposal is to upgrade the mount on the nearby 11m reflector so that it can be used more effectively for stereo viewing with the 10m telescope.

Significance and Appropriateness (for DOE support) of the Proposed Research

All of the members of the SAGENAP panel thought very highly of this proposal. They pointed out that the Whipple group has largely developed the field of γ-ray astronomy in the energy range around 1 TeV, learning how to discriminate against hadronic background and have gradually improved the technique. The Whipple group is regarded as having an impressive track record, making the first universally-accepted observation of a source of 1-TeV photons (the Crab) and the first observation of an extragalactic source (Markarian 421) at this energy. In addition, they discovered the AGN Markarian 501, a γ-ray emitter which was not observed by EGRET.

While the conventional justification of this research in terms of particle physics has been that it may help us understand how cosmic rays are accelerated to very high energies and where that acceleration takes place, reviewers said that its impact might be broader. As an example, it was noted that γ-ray astronomy has been a vital tool in understanding the nature of AGNs. The members of the panel all felt that this project was within the scope of high energy physics.

Reviewers felt that the proposed improvements of the 10m would significantly advance studies of pulsars, supernova remnants, AGNs and γ-ray bursts. One reviewer said that it would produce the best instrument in the world for γ-ray observations in the 100 GeV to 10 TeV energy range. Another reviewer compared this project to GRSST/GLAST, and said that the GRANITE III proposal was easily as important as the new space instrument with regard to observing and studying AGNs, noting that with a new low energy 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.

On the other hand, reviewers were not nearly so enthusiastic about the plans to upgrade the mount on the 11m telescope. They found the case for the importance of the stereo imaging afforded by the addition of the upgraded 11m telescope unconvincing. Noting that the collaboration does not ask for funding for the 11m telescope upgrade until year 3, they said that the data to come from the new improved 10m telescope would be useful in establishing the utility of the stereo arrangement.

Feasibility of Plans

The reviewers felt that the proposed upgrade of the camera on the 10m telescope is well motivated and almost guaranteed to result in impressive new observations. The group's extensive experience with the existing telescope shows they are well qualified to carry out this upgrade and make it work. Also, one reviewer noted that they recently showed that further exploitation of the topology of the image can further discriminate against the single muon background.

It was said that the most promising improvement appears to come from implementing more and finer pixels which will help the separation of γ-rays from hadrons and muons as well as provide better resolution and extended sensitivity at lower energy.

The upgrade also calls for more sophisticated electronics, needed to lower the energy threshold. It was stated that this could be procured more cheaply if it were designed and built in house. However, the absence of a local electronics group may make such an effort impractical. The electronics upgrade proposed system is conservative.

The proposal did not include Monte Carlo studies to assure that the upgrade will have enough resolution to measure the energy cutoff in the sensitivity range of the telescope. A detailed discussion of the systematic errors in the energy measurements was desired. One reviewer thought that the stereo imaging provided by the 10m and 11m reflectors separated by 140m was important to discriminate between low energy, nearby showers and more distant high energy showers. If true, this would argue for upgrading the 11m telescope. In any event, it was said that the idea of stereo viewing of Čerenkov shower images was worth pursuing as a development project.

DOE-HEP RECOMMENDATIONS

The Whipple group has been making excellent use of the 10m γ-ray telescope. They recently reported the discovery of two AGNs, Markarian 421 and 501. The request for the upgrade of the 10m instrument as outlined in the GRANITE III proposal should be funded with high priority. The observations have a significant expectation of producing new and exciting physics.

It is recommended to delay consideration of the 11m telescope upgrade until data from the new 10m telescope is analyzed and it is determined that the stereo arrangement will significantly improve the sensitivity.

Sky Survey for Gravitationally Lensed Quasars

This is a proposal to instrument a ground-based optical Schmidt telescope with a large CCD camera. The telescope so instrumented would scan a large area of the sky each night to find and record the light output from gravitationally lensed quasars. Assuming that the number of quasars to be found and the fraction being lensed are near estimates, the survey would provide measurements of the Hubble "constant" early in the evolution of the Universe and of the cosmological constant. It would also be a search for large aggregates of Dark Matter.

This project is a collaboration between (high energy) physicists and astronomers at Yale University, the University of Indiana, and Centro de Investigaciones de Astronomia (CIDA) in Venezuela, where the telescope is located. The spokesmen are C. Baltay and G. Oemler, from the Yale Physics and Astronomy Departments, respectively. The current proposal requests funding for the equipment to be provided by the physicists: the CCD detector to be designed and built at Yale, and the readout and control electronics to be designed and built at Indiana.

Significance and Quality of the Research and Its Appropriateness for DHEP Support

All the reviewers found the science underlying the proposal very appealing, and most felt that it was of direct relevance to elementary particle physics. The astrophysics was characterized as fundamental, important, elegant, and fascinating. It was felt that the detection of the expected large number of quasars and gravitational lenses would be a major contribution to astrophysics, probably even if the estimates turned out to be somewhat optimistic. In short, the SAGENAP reviewers strongly endorsed the science.

However, as strongly as they applauded the science *qua* science, the reviewers felt every bit as strongly that the proposed work was fundamentally an astronomy project. None of them unambiguously recommended that the DOE Division of High Energy Physics should provide support for it. Their specific suggestions ranged from vigorous opposition to the DOE providing the equipment requested in this proposal to encouraging some sort of split funding arrangement with, for example, the National Science Foundation Division of Astronomy.

Carrying out this project requires a major sustained participation from observational astronomers, in both the data collection and understanding its reduction. For some reviewers, this capability was thrown somewhat into doubt by Oemler's departure from Yale, since he is the dominant figure among astronomer participants. Because of the essential involvement of astronomers in this project, reviewers stressed that it was crucial that the DOE understand that the participating astronomers have adequate resources to carry out their role. In particular, there was some concern expressed that, because of the project seems to have been at least partly initiated by high energy physicists, the requisite support might be less forthcoming than usual.

Furthermore, because much, if not most of the intellectual and technical input to carry it out this scientific effort must come from astronomers, the success or failure of the program as a whole, as opposed to simply the construction of the CCD camera, was not within the control or competence of the high energy physicists involved. Members of the panel raised a number of issues, such as dealing with observational bias or configuring the design of the instrument so as to assure that it performed with the requisite sensitivity and precision, not so much as to criticize the proposal as to express their sense that neither they nor likely any other high energy physicists were really competent to judge them, and so to illustrate why it was important that this proposal be scrutinized by the appropriate experts in the astronomy community explicitly in competition with other astronomy proposals.

Reviewers had some appreciation for the willingness of the DOE to support high energy physicists even when their interests take them somewhat afield. However, they said that such support should be primarily for work in areas where there is no traditional source of support, or where the technical expertise brought by the high energy physicists is unique.

Related and Competing Activities

Many members of the SAGENAP panel were aware that there were other projects, some technically similar or related and others not, whose scientific goals were essentially the same as the present proposal. Indeed, there are large,active projects in progress which have given us our present understanding of the cosmological parameters. However, none of the members of the panel felt either knowledgeable or familiar enough with them to be able to say how well the present project compared to them. Several reviewers noted that various groups of astronomers had built CCD cameras for optical telescopes comparable in size to the proposed effort, so that this proposal did not represent the transfer of technology developed in high energy physics to a community innocent but desirous of it.

Feasibility of the Proposed Work

This is a proposal to instrument a large-aperture Schmidt optical telescope with a large modern CCD array in order to systematically study gravitational lensing from quasars. A very large sample of quasars would be observed over a several year period. The responsibility for the data collection and analysis was mostly with the collaborators from Astronomy Departments. The project promises measurements of the Hubble constant, the deceleration parameter, and the cosmological constant, and will search for clusters of dark matter.

The issue of the feasibility of the work can be taken on two levels: the feasibility of the work proposed by the high energy physicists, *i.e.*, building the CCD array and its associated electronics; and the feasibility of the scientific project as a whole. There was general, though not complete comfort with the ability of the proposers to build and instrument the array. However, the members of the panel saw that for this project to succeed requires a very high level of expertise, manpower, and money, from the astronomers participating, and they did not see that those contributions were committed or funded.

Members of the SAGENAP panel had concerns regarding the magnitude of the job of data collection and analysis. They questioned whether the group was capable of handling the volumes of data that will be accumulated in this experiment, and raised the possibility that the need for additional people or for a good deal of computer hardware would be felt once the data taking had started.

There were also questions raised about one technical aspect of the CCD array project. The Yale group has had considerable experience with CCDs on a large scale, but apparently to make the array sensitive in the ultraviolet, the CCDs must be made unusually thin. This is an aspect of the project where CCD expertise would have to be transferred from the astronomy to the high energy physics community. It was not asserted that the Yale group could not successfully construct such an array, but rather that for them to do it right the first time with the world's largest CCD camera is not an easy undertaking.

DOE-HEP RECOMMENDATIONS

This project, including all of its equipment costs, should be submitted to the National Science Foundation Astronomy Division and should be reviewed in competition with the other related projects arising within the astronomy community. The question of DOE support for equipment should be deferred until the proposal receives proper consideration in the NSF/Astronomy context.

The High Energy γ-ray Silicon Strip Telescope (GRSST)
A Prototype Development for γ-ray Large Area Space Telescope (GLAST)
A Broadband High Energy γ-ray Space Based Telescope Using Silicon Strip Detectors and CSI

The GRSST is a prototype development effort for the GLAST experiment, which will be a broad band high energy γ-ray detector telescope made of 49 tower modules of silicone strip/cesium iodide detectors covering an area of $2.8m^2$, and a range of γ-ray energies from 10 MeV to 300 GeV. The primary scientific motivation for GLAST is to study the physics of γ-ray emission from supermassive black holes and other possible astrophysical sources, relativistic jets in active galactic nuclei (AGN), γ-ray bursts, rotation powered pulsars and diffused galactic and extragalactic radiations. The GLAST would replace the EGRET instrument now taking data on the Compton (GRO) observatory satellite launched by NASA in 1991. The remaining operating time for EGRET is about a year. In comparison GLAST exceeds EGRET in all of the important detection capabilities: energy resolution and range, effective area solid angle acceptance, and point source sensitivity.

Each tower, with a frontal area of 24cm x 24cm, has a charged particle veto scintillator layer followed by 12 tracker/converter layers and a 10 radiation length CsI calorimeter. Each of the first 10 tracker/converter layers has 0.05 radiation lengths of high density converter followed by 2 planes of (x,y) silicon strip detectors (240 μm pitch). These are followed by two tracking layers. The base of the tower is an array of 3cm x 3cm x 10 radiation length of CsI(Tl) crystals.

The objective of the GRSST proposal is to construct only one GLAST detector module, and test it in an accelerator beam. In order to demonstrate that the design is suitable for space operation, a more traditional NASA approach will be to test the prototype tower as well in a high altitude balloon flight. GRSST/GLAST is an international collaboration, and research and development pertaining to many phases of the project is in progress.

Summary of the SAGENAP Review and Other Reviews

The scientific assessment group was in complete agreement that the GLAST people did an outstanding job in presenting the underlying science and techniques contained in the proposal. Their uniform view was that this project has high scientific merit and should be supported. However, there was great concern expressed about whether HEP funds should be used to support the GLAST project.

The proposal has passed three NASA reviews and was the only one in this energy range selected for a mission concept study. It is highly regarded in the community of astronomers. The collaboration has already accomplished an impressive amount of work simulating the detector and has extensive experience with the silicon strip detector technology that is crucial to the design of GRSST/GLAST and is strong enough to carry out the proposed program.

The need to demonstrate that the detector is worthy for a space mission was clearly recognized, as was the fact that doing so would be costly and without value for science *per se*. The proposed balloon flight to test one calorimeter module, a technology that is well understood and has been used in many accelerator experiments, will not produce any new scientific results and would be costly, yielding a small return. Finding a cheaper method of the detector certification for the use in space was deemed clearly desirable.

Views on Funding

The funds for GRSST R&D requested from DOE/HEP by the SLAC group in collaboration with UC/Santa Cruz and for a period of 3 years is $2.46M. The projected DOE/HEP contribution to fully instrumented GLAST detector would apparently be in the range of $25M. Until now the GRSST/GLAST R&D effort was supported by SLAC at a level of about 450K/year. The group on balance was of the opinion that, with perhaps some additional funding from the DOE HEP funds, this mode should continue. Request for additional R&D support from DOE/HEP funds should be examined within context of priority needs at SLAC and pressures on the national HEP program. Questions have been raised about the present and future levels of support given to experiments in astrophysics by two single purpose high energy accelerator laboratories in view of expected future funding profiles. This should be addressed in the context of the national HEP program, particularly with regard to the relative merits of the physics. Groups such as SAGENAP can address whether projects make sense in the context of the set of non-accelerator activities, and SAGENAP is asked to comment on whether, to them, projects make sense as high energy physics. However, SAGENAP should not be the sole judge as to whether non-accelerator projects make sense for the national high energy physics program. This group was rather strongly of the opinion that project funds for the U.S. GLAST effort should come from NASA, which has traditionally supported such scientific missions. If there is a formal proposal to DOE/HEP, this would significantly impact the on-going national HEP program and should be examined in that context.

DOE-HEP RECOMMENDATIONS

DOE support for the GRSST prototype detector effort has come from existing programs at SLAC, and to a limited extent, at UC/Santa Cruz. R&D support should continue at a level compatible with the existing SLAC and UC/Santa Cruz programs. It appears that a revision of the scope of the DOE GRSST effort may be in order to lower the costs of the planned R&D. A proposal for support of the GLAST project should be reviewed in the context of the national HEP program.

STATUS AND PROGRESS REPORTS

MACRO

MACRO is a massive detector comprised of liquid scintillator modules, shower tubes and track-etch detectors. It is organized unto 12 supermodules, each 12m by 12m by 4.5m high and arranged in a double decker configuration 12m wide by 72m long and 9m high. The MACRO detector is located in Hall B of the Italian Gran Sasso National Laboratory (LNGS) at an average depth of 3800 meters of water-equivalent and 963m above sea level. The detector's acceptance for isotropic particle fluxes is 10,000m^2sr and is designed to determine upward or downward particle trajectories and provide identification for various types of penetrating radiation. The MACRO detector was designed primarily to search for magnetic monopole predicted by Grand Unified Theories (GUT). Its versatility allows for other searches and measurements in particle physics, astrophysics and cosmic ray physics. These are: high energy neutrino studies (E_ν~ 1 GeV and above), observations of neutrinos from supernova gravitational collapse, searches for neutrino oscillations, strange quark matter, fractionally charged particles, and other rare phenomena which may occur in cosmic rays. MACRO's maximum threshold for downward going muons is 1.4 TeV, which allows for studies of high energy muon astronomy. The detector has been fully operational since September of 1995, performing as planned, and a physics program for the next 5 years is in progress. The project is heavily supported by the Italian groups, and the INFN provides the significant infrastructure at the laboratory.

Summary of SAGENAP Reviews

The MACRO collaboration has made impressive progress; the detector is completed and runs well. It has been operating in a fully instrumented mode for about a year and it is capable of reaching its designed scientific potential. The search for monopoles will set a sensitive limit and already high quality cosmic ray data has been accumulated. Physics results obtained so far from MACRO have been solid but somewhat mundane. The operation of MACRO is planned for a period of 5 years and the support of the U.S. collaboration should continue.

DOE-HEP RECOMMENDATIONS

MACRO is a fully instrumented detector and is now in a routine data taking mode. Data analysis is in progress and results are appearing in refereed journals. The collaboration is committed to data taking for a period of the next five years.

Support of the U.S. participating groups for operating the MACRO detector and data analysis should continue.

Cryogenic Dark Matter Search (CDMS) (Cabrera)

Cryogenic Dark Matter Search Experiment is searching for weekly interacting massive particles (WIMP's) which could be constituents of the cold dark matter around our galaxy. This experiment is also capable of observing supersymmetric particles over a significant range of parameters. The detector is installed in the Stanford Underground Facility (SUF), a tunnel 34.5 feet below the street level. The experiment uses a new class of particle detectors based on the propagation of phonons in silicon or germanium crystals at temperatures below 0.1K. In these detectors signals from phonons and ionization are readout simultaneously which provides event by event discrimination between nuclear recoils from dark matter candidates and electron recoils from nearly all backgrounds. The infrastructure for the experiment, anticoincidence shield and the cryostat for the dark matter detectors together with the refrigerator are in place. In the coming year, the University of California at Berkeley group, a member of the CDMS collaboration, is scheduled to take data with two germanium detectors weighing 60 g and 160 g each. The Stanford group will use two detectors in the WIMP search, one 100 g silicon and the other 100 g silicon together with 250 g germanium. The aim of that group is eventually to instrument two detectors with masses of 500 g silicon and 500 g germanium. It is expected that the experiment in the SUF will run for two years. In parallel the collaboration is planning to build another cryogenic apparatus to continue the WIMP search in a deep site laboratory which could be the Soudan Mine in Minnesota.

Summary of SAGENAP Reviews

The cold dark matter search using cryogenic techniques can lead to a fundamental discovery and is a research program that fits the DHEP mission. The experiment presents a difficult technical challenge. The Stanford and Berkeley groups pioneered detector development in this field, made several original contributions and produced working detectors that will be used in an experiment which is now beginning. The CDMS collaboration is the only existing strong and capable group in the U.S. in this field and faces many competitors in Europe and Asia. The table top appearance of this search may give a deceptive impression that the project is inexpensive. In fact the low temperature cryogenic technology together with the desired very low background from natural radioactive impurities of parts used in the detector demand costly and state of the art R&D in material sciences. The work of this group is first rate and the plan to field the experiment at first in an existing cavity at Stanford with a shallow overburden is sound. A follow-up experiment would use a deep underground facility possibly in the Soudan mine.

The group seems to operate in an R&D intensive way and the progress towards obtaining data is slow. The CDMS project is supported by both DOE and NSF and the recommendation is to continue funding this prototype experiment.

DOE-HEP RECOMMENDATIONS

Research and development of the detectors for searching for non-baryonic cold dark matter is advancing. An experiment is beginning at the Stanford Underground Facility. A silicon germanium wafer detector is in the R&D stage at Stanford, will be used during the course of the on-going search. Support of this project should continue through the prototype experiment. Support for the deep mine phase should be considered at a later date.

Super-Kamiokande

The Super-Kamiokande Detector is a 50,000 ton ring-imaging water Čerenkov detector constructed at a depth of 2700 meters water equivalent (MWE) in the Kamioka Mozumi Mine in Japan. It consists of a stainless steel tank in the shape of a right circular cylinder 39m in diameter and 41m height, filled with purified water. The detector is optically segmented into an inner volume and an outer anticoincidence region 2.5m thick completely surrounding the cylinder. The inner detector is viewed by 11,200 photomultiplier tubes (PMT's) each 50cm in diameter, uniformly distributed on the inner boundary giving 40 percent photocathode coverage. The outer volume is viewed by 2,200 PMT's of 20cm diameter with wavelength shifter plates. The American collaboration is responsible for equipping the outer detector with PMT's and associated electronics, providing the opaque light barrier and reflectors, on-line DAQ monitoring, calibration system for the entire detector, universal time coordinated (UTC) clock system and radon reduction system. The physics objectives of Super-Kamiokanda are to improve the present limits on nucleon decay by a factor of 10, study the atmospheric neutrino anomaly, solar neutrinos, high energy neutrino astrophysics and finally search for gravitational stellar collapse neutrinos. This detector will also be used in a long base line neutrino oscillation experiment planned at KEK.

Construction of the Super-Kamiokande has been completed, the detector is now filled with water and the commissioning is now in progress. Data taking is expected to start on April 1, 1996. [Super-Kamiokande is now taking data.]

Summary of SAGENAP Reviews

This is an experiment of unprecedented magnitudes and scope. The Super-Kamiokande detector design was based on the experience gained from the highly successful Kamiokande and IMB experiments and the collaboration should achieve all of its anticipated scientific goals. Super-Kamiokande is principally a Japanese experiment with Japan providing most of the funding with significant financial and intellectual contributions from the U.S.

Some concern was expressed that the U.S. groups lack strength and resources in the data analysis effort to maintain a highly visible U.S. participation. A continued strong support by DHEP for the U.S. groups is recommended including the request for $342,000/year to cover operating expenses at the KAMIOKA mine.

DOE-HEP RECOMMENDATIONS

Construction of the detector is completed. The experiment commenced data taking on April 1, 1996. Funding of the experiment is planned to proceed according to the established profile. Additional funding to cover operating expenses should be considered with high priority in FY 1997.

MILAGRO

MILAGRO is a water-Čerenkov detector designed to study extensive air showers and is deployed in an existing pond with the size of 60 x 80 x $8m^3$ located at Fenton Hill in the Jemez Mountains at the Los Alamos National Laboratory geothermal site. The pond will be surrounded with an array of scintillator counters encompassing an area of $35{,}000m^2$. The volume of water will be divided into 3 layers by arrays of photomultipliers (PM's): air shower layer with 450 PM's, muon layer with 150 PM's and hadron layer with 150 PM's. The air shower layer detects ~50 percent of all incident particles. The overall angular resolution is 20.6^o and 0.3^o above 1 TeV. MILAGRO's detection energy range extends from the thresholds of 250 GeV to above 100 TeV.

Physics objectives of MILAGRO are: observations of the energy spectrum of the crab, Markarian 421, γ-ray bursts, search for evaporating primordial black holes VHE emissions from SNR's and solar physics.

MILAGRO's status at present is: most of the infrastructure necessary to operate the experiment is in place, sufficient fraction of electronics and DAQ has been installed to take data with 40 PM's and the run is in progress. This project is primarily supported by the NSF.

Summary of SAGENAP Reviews

The MILAGRO project is well motivated and it is certain that it will observe very high energy γ-rays. Also it will significantly extend the reach of γ-ray astronomy because of the detector's large duty factor, low energy threshold and a large angular acceptance which permits the all sky search. However, MILAGRO has a limited capability of detecting point sources. Assembly of the detector is progressing well. Support of this project should continue.

DOE-HEP RECOMMENDATIONS

The capabilities of this ground based high energy γ-ray detector complement the existing reach of Whipple and EGRET observatories and extend the potential of discovery in the field.

The primary source of funds for this detector comes from the National Science Foundation. However, some DOE funds were used to prepare the pond for operation and for procuring a fraction of necessary electronics. Funding of the experiment is planned to proceed according to established profiles at DOE and NSF.

The Soudan II Detector

The Soudan II experiment uses a currently operating detector in an underground laboratory 713m (2090 water-equivalent) beneath Soudan, Minnesota. The detector consists of 963 metric ton fine-grained tracking iron calorimeter surrounded on all sides by a two-layer active shield of proportional tubes. Its primary goal is to search for nucleon decay, a process which may be dominated by neutrino-interaction background in water Čerenkov detectors. The Soudan II experiment has been operational since July 1988, when the first 275 tons of detector was turned on. Data was taken while the detector was being constructed; operation of the complete 963 ton detector began in November 1993. Exposure to date is 2.7 Kt-year, and 5.0 Kt-year is expected by 1999.

Besides the search for nucleon decay, Soudan is measuring the atmospheric neutrino flavor ratio, also searching for muon point sources and for neutrinos from Active Galactic Nuclei.

Summary of SAGENAP Reviews

At this time the only possibly competitive edge of Soudan II is the capability of observation or setting a limit on the K^+ mode of proton decay $p \rightarrow K\upsilon$. The existing limit on that decay set by IMB exceeds the Soudan result by two orders of magnitude and will be considerably improved by the Super-Kamiokande detector which is about to take data. [Super-Kamiokande is now taking data.] Super-Kamiokande will outperform Soudan II in all of the overlapping physics topics. The measurement of the neutrino atmospheric anomaly by Soudan II seems to favor the results obtained by Frejus which is another segmented detector. The statistical precision of data from these experiments is not significant enough to resolve the existing disagreement between the results obtained by both water Čerenkov detectors, (IMB, Kamiokande) and the segmented ones (Soudan II and Frejus). The cosmic ray data taken by Soudan II which indicated existence of muon point sources in space no longer supports that discovery and is now consistent with the null observations of that phenomenon by other detectors. The MACRO detector, because of its larger area, is a better detector for cosmic ray studies. Soudan II seems to detect muons generated by horizontal neutrinos. The physics objectives in using these events are not clear.

It was felt by the group that the analysis of the Soudan II data has been slow and the results have little scientific impact. It was suggested by some of the reviewers that the operation of Soudan II should be downsized, perhaps even moth-balled, but keeping the infrastructure of the experimental cavity functioning for other possible underground experiments. On the other hand some sentiment was expressed to fund the completion of the Soudan II five kiloton year run.

DOE-HEP RECOMMENDATIONS

Physics results from SOUDAN II do not appear to be competitive with the existing data addressing similar questions by other experiments. The capabilities of SOUDAN II will soon be surpassed by Super-Kamiokande. Therefore, scaling down of the operation of SOUDAN II should be planned. The SOUDAN mine is a desirable site for other underground experiments. Costs of maintaining minimal infrastructure enabling the site to be preserved for other experiments should be assessed and the possibility explored of keeping the site on standby status.

The Pierre Auger Project

The Pierre Auger project proposes to study the properties of the highest energy cosmic rays, namely those with energies above 10^{19} eV. It is not understood what the nature of these particles is, nor how they are accelerated to such high energies.

There are no models that can explain the acceleration of cosmic rays to these energies so the composition and origin of this flux is completely unknown. In addition, it is expected that particles of energy greater than a few times 10^{19} will interact with the cosmic microwave background and be absorbed (degraded). The recent observation of several events well beyond this cutoff implies a relatively local origin (less than 50 Megaparsecs) and is a great puzzle. Extrapolating the present small sample of events at these energies, the Auger detector would detect about 7000 showers per year with energies above 10^{19}eV and about 70 above 10^{20} eV. With these statistics, it might be possible to localize the sources of the highest energy events and interpret the shape of the spectrum in terms of various acceleration mechanisms. The experimental techniques for such studies exist and assure that a construction of a cosmic ray observatory with adequate sensitivities for reaching the desired energy range is possible.

The collaboration proposing to build two very high energy cosmic ray observatories one in each hemisphere, is truly international, involving physicists from 17 countries around the globe. Organization of the project is emerging and related R&D is pursued by many participating groups. The aim of the collaboration is to submit a full fledged proposal for the Pierre Auger observatory in the latter part of 1996.

Summary of SAGENAP Reviews

This is a well motivated long range project with a discovery potential. It is evident that the already existing international collaboration is capable of building the proposed large size cosmic ray detector arrays. R&D on some aspects of this project is in progress in the U.S. and abroad. Fermilab assisted in the conceptual stages of the proposal with engineering and administrative help. The cost estimate for the Auger project of $100M is a result of the self limit imposed by the collaboration. It is not clear to what extent that decision may compromise scientific objectives. The expected request for the U.S. contribution to the Auger project (NSF and DHEP together) will amount to about $25M. This by far exceeds the level of the present DHEP funding for non-accelerator experiments, so a major increase of this budget category would be necessary. The decision to fund the Auger project would have an impact on the national HEP program and raise fundamental questions of the relevance of the project to the primary mission of DOE/HEP and the operation of the National Accelerator Laboratories. It was remarked that a reported favorable recommendation from the review panel of the Auger project which met in Paris France on November 22-24, 1995, did not consider funding issues and their impact on other programs. A similar review by the Fermilab committee delineated a limited involvement of the laboratory in the Auger project.

A technical question was raised as to what extent the capabilities of the HIRES Fly's Eye detector are competitive with the Auger project and whether HIRES could produce compatible data.

The Auger project will have a high international visibility but limited scientific aims; however, it is conceiveable that new physics will be required to explain the highest energy events. Further consideration of the Auger project is needed, and this can take place when a full-fledged proposal is received by the funding agencies.

DOE-HEP RECOMMENDATIONS

A formal proposal is expected in the summer of '96. Decisions on funding of this experiment will depend on the outcome of the review process following receipt of the proposal by DOE and NSF.

SUMMARY
OF
DOE-HEP RECOMMENDATIONS FROM THE SAGENAP REVIEW

DUMAND

Further support of the DUMAND project is not recommended. At base, the reason is the track record of the collaboration in its work to date, and a judgement that the DUMAND team, including its leadership, lack the skills needed to carry out such a complex project in the unforgiving environment of the deep ocean. The project should be terminated, and its operations phased out in an orderly manner.

GRANITE III

The Whipple group has been making excellent use of the 10m γ-ray telescope. They recently reported the discovery of two AGNs, Markarian 421 and 501. The request for the upgrade of the 10m instrument as outlined in the GRANITE III proposal should be funded with high priority. The observations have a significant expectation of producing new and exciting physics.

It is recommended to delay consideration of the 11m telescope upgrade until data from the new 10m telescope is analyzed and it is determined that the stereo arrangement will significantly improve the sensitivity.

Gravitationally Lensed Quasars

This project, including all of its equipment costs, should be submitted to the National Science Foundation Astronomy Division and should be reviewed in competition with the other related projects arising within the astronomy community. The question of DOE support for equipment should be deferred until the proposal receives proper consideration in the NSF/Astronomy context.

GRSST/GLAST

DOE support for the GRSST prototype detector effort has come from existing programs at SLAC, and to a limited extent, at UC/Santa Cruz. R&D support should continue at a level compatible with the existing SLAC and UC/Santa Cruz programs. It appears that a revision of the scope of the DOE GRSST effort may be in order to lower the costs of the planned R&D. A proposal for support of the GLAST project should be reviewed in the context of the national HEP program.

PROJECTS PRESENTING STATUS REPORTS

MACRO

MACRO is a fully instrumented detector and is now in a routine data taking mode. Data analysis is in progress and results are appearing in refereed journals. The collaboration is committed to data taking for a period of the next five years.

Support of the U.S. participating groups for operating the MACRO detector and data analysis should continue.

SUPER-KAMIOKANDE

Construction of the detector is completed. The experiment commenced data taking on April 1, 1996. Funding of the experiment is planned to proceed according to the established profile. Additional funding to cover operating expenses should be considered with high priority in FY 1997.

SOUDAN II

Physics results from SOUDAN II do not appear to be competitive with the existing data addressing similar questions by other experiments. The capabilities of SOUDAN II will soon be surpassed by Super-Kamiokande. Therefore, scaling down of the operation of SOUDAN II should be planned. The SOUDAN mine is a desirable site for other underground experiments. Costs of maintaining minimal infrastructure enabling the site to be preserved for other experiments should be assessed and the possibility explored of keeping the site on standby status.

MILAGRO

The capabilities of this ground based high energy γ-ray detector complement the existing reach of Whipple and EGRET observatories and extend the potential of discovery in the field.

The primary source of funds for this detector comes from the National Science Foundation. However, some DOE funds were used to prepare the pond for operation and for procuring a fraction of necessary electronics. Funding of the experiment is planned to proceed according to established profiles at DOE and NSF.

CRYOGENIC DARK MATTER SEARCH (CDMS)

Research and development of the detectors for searching for non-baryonic cold dark matter is advancing. An experiment is beginning at the Stanford Underground Facility. A silicon germanium wafer detector is in the R&D stage at Stanford, will be used during the course of the on-going search. Support of this project should continue through the prototype experiment. Support for the deep mine phase should be considered at a later date.

THE PIERRE AUGER PROJECT

A formal proposal is expected in the summer of '96. Decisions on funding of this experiment will depend on the outcome of the review process following receipt of the proposal by DOE and NSF.

APPENDICES

APPENDIX A - CHARGE TO SAGENAP

In the context of the national High Energy Physics program, taking into account necessary stringencies in funding and limitations in scope, the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) is charged to review, assess and evaluate activities related to "non-accelerator" experiments in the Division of High Energy Physics (DHEP), and advise on the implementation and maintenance of a high quality program of this kind. The assessment should include experiments proposed separately to the DHEP and jointly to the DHEP and the National Science Foundation Division of Elementary Particle Physics and/or other agencies as appropriate. As these reviews may be of use to the other agencies, coordinate fully with cognizant persons in those agencies, as necessary. The study will include not only new activities which have been proposed to the Division of High Energy Physics, but also relevant on-going activities, and should look ahead to activities that are in the preliminary stages of R&D or prototype experiments.

For each activity reviewed please provide recommendations on the following:

a. Does the proposed work fall within the scope of DHEP?

b. If the proposed work does fall within the scope of DHEP, should the work be supported?

c. If recommended for support:

    i) What level of equipment support is appropriate? This issue is both general and specific and should include the overall level as well as levels for specific experiments.

    ii) What extraordinary levels of operating funds are required for recommended activities? This is to alert the agencies as to special operating needs such as R&D, power, facility operations, maintenance, additional manpower, etc., which may go beyond the normal operating budgets of the participating institutions. These numbers should be integrated into the overall levels of support recommended.

d. What terms and conditions are attached to your recommendations for the support of experiments, such as progress milestones, assumed support from other agencies, unsettled make-up of collaborations pending international agreements, etc.?

e. Are the recommended activities and support for them integrated into scope and funding priorities of other agencies (if relevant)?

Please provide your recommendations by individual letter-report to the Chairman within two weeks after each meeting.

APPENDIX B - MEMBERSHIP OF THE SAGENAP PANEL

Scientific Assessment Group for Experiments in Non-Accelerator Physics Members:

Barry Barish
Caltech
Physics, Math & Astronomy Division
Pasadena, CA 91125

Steve Barwick
University of California at Irvine
Physics Department
Irvine, CA 92717

Gene Beier
University of Penn.
Department of Physics
209 S. 33rd Street
Philadelphia, PA 19104-6396

Tom Gaisser
Physics Department and
Bartol Research Foundation
University of Delaware
Newark, DE 19711

Cy Hoffman
Los Alamos National Laboratory
Mail Stop H844
Los Alamos, NM 87545

Robert Lanou
Brown University
Department of Physics
Providence, RI 02912

Adrian Melissinos
University of Rochester
Department of Physics and Astronomy
Rochester, NY 14627-0171

Hank Sobel
University of California at Irvine
Physics Department
Irvine, CA 92717

Trevor Weekes
Smithsonian Institute
Whipple Observatory
670 Mt. Hopkins Rd.
P.O. Box 97
Amado, Arizona 85645

DOE

P.K. Williams, Chairman
U.S. Department of Energy
19901 Germantown Rd.
Germantown, MD 20874-1290

Jeff Mandula
U.S. Department of Energy
19901 Germantown Rd.
Germantown, MD 20874-1290

Tom Romanowski
U.S. Department of Energy
19901 Germantown Rd.
Germantown, MD 20874-1290

NSF

William Chinowsky
National Science Foundation
4201 Wilson Blvd.
Suite 1015
Arlington, VA 22230

Marvin Goldberg
National Science Foundation
4201 Wilson Blvd.
Suite 1015
Arlington, VA 22230

NASA

Vernon Jones
NASA Headquarters
Code SS
Space Physics Division
Washington, DC 20546

Alan Bunner
NASA Headquarters
Code SZ
Washington, DC 20546

APPENDIX C - AGENDA FOR THE FEBRUARY 20-21, 1996 MEETING

U.S. Department of Energy
Division of High Energy Physics
Scientific Assessment Group for Experiments in Non-Accelerator Physics

Sheraton-Washington Hotel
Washington, D.C.
February 20-21, 1996

Tuesday, February 20, 1996

8:30 a.m.	Executive Session	
9:30 a.m.	DUMAND Status	
9:30 a.m.	Science Update	J. Wilkes
9:50 a.m.	Experiment Status	P. Gorham
10:20 a.m.	Summary	J. Learned
10:40 a.m.	A Robot for Installation & Repair	H. Crawford
10:55 a.m.	Discussion	
11:00 a.m.	Hubble-Cosmological Constant Measurement Proposal	C. Baltay
12:00 noon	Granite III Proposal	
12:00 noon	Background and Scientific Objectives for Granite III	R. Lamb
12:30 p.m.	History, Current Performance and Future Expectations for the Whipple Observatory's Gamma Ray Imaging Telescopes	D. Carter-Lewis
1:00 p.m.	Lunch	
2:00 p.m.	AUGER Project R&D Status and Vision	J. Cronin
3:00 p.m.	Cold Dark Matter Search Status	B. Cabrera
4:00 p.m.	MILAGRO Status	C. Hoffman
5:00 p.m.	MACRO Status	B. Barish
6:00 p.m.	Executive Session	
7:00 p.m.	Adjourn	

Wednesday, February 21, 1996

8:30 a.m.	Executive Session	
9:00 a.m.	GRSST Proposal	
9:00 a.m.	Introduction and Program Overview	E. Bloom
9:30 a.m.	High Energy Gamma Ray Astrophysics	N. Gehrels P. Michelson
10:00 a.m.	Prototype Tower and GLAST Instrument	W. Atwood
10:30 a.m.	Summary, Project Budget and Schedule	E. Bloom
10:45 a.m.	Review of Collaboration Commitments	
10:45 a.m.	NASA Goddard	J. Ormes
11:00 a.m.	Rome	A. Morselli
11:15 a.m.	UCSC	R. Johnson
11:30 a.m.	NRL	K. Wood
11:45 a.m.	Chicago	M. Oreglia
12:00 noon	AGN: A Laboratory for High Energy Physics and Cosmology	J. Primack
12:30 p.m.	Lunch	
1:30 p.m.	Soudan II Status	M. Marshak M. Goodman
2:30 p.m.	Super-Kamiokande Status	H. Sobel
3:30 p.m.	Executive Session	
5:30 p.m.	Adjourn	