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SUMMARY

SCIENTIFIC ASSESMENT GROUP FOR EXPERIMENTS IN NON-ACCELERATOR PHYSICS (SAGENAP)

March 5-6, 1997

P. RANKIN, 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). The group provides a standing review of the ongoing research program in non-accelerator physics. This review covers experiments funded by DOE, NSF, and NASA, and is attended by members of all agencies. It provides information to these three agencies.

This report covers meetings held in March and November 1997 in Washington, D.C. The experiments reviewed were AUGER, Km^3, AMANDA, CDMS II, AXIONS, ICARUS, GLAST, STACEE, CELESTE, STEREOSCOPIC AIR CERENKOV DETECTORS, and PION EYE at SUNSPOT. The second meeting concentrated on the AUGER proposal. This report contains summaries from the review letters written by members of the group and the recommendations based on their inputs.

The meetings were chaired by Patricia Rankin of NSF with P.K. Williams of DOE acting as co-chair. In addition, there was agency representation from NASA. The charge to the group can be found in Appendix A. The membership of SAGENAP is given in Appendix B. Details of the meeting schedules can be found in Appendix C. The group members reported individually by letter to the Chair, who was responsible for assembling the final report.

AUGER

Science

Cosmic rays were discovered almost 100 years ago and despite considerable work their origin is still not well understood. The only fully identified sources are inside the heliosphere although high energy cosmic rays are known to come from our galaxy, and at least a few other galaxies with magnetic fields produce cosmic rays. The "Standard Model" of cosmic rays is Fermi shock acceleration in supernova remnants but some recent high-energy gamma-ray observations (from Whipple and CYGNUS) call this model into question. The shape of the cosmic-ray energy spectrum changes at 10^{16} eV (the "knee") leading to the belief that at least some aspects of the physics change here. However, many (ground-based) attempts to measure the cosmic-ray composition in this region have yielded contradictory results. There is some evidence that the spectral shape and composition also changes at $\sim 10^{18}$ eV (the "ankle") but this is not certain.

The Auger proposal concerns a major initiative to build two air shower detectors to measure the arrival direction, energy, mass composition and flux of cosmic rays at the very highest energies - above 10^{19} eV. The flux of such high energy cosmic rays is known to be only about $1/km^2/year$ at the earth's surface. In particular, Auger wishes to study cosmic rays above the Greisen-Zatsepin-Kuz'min (GZK) cutoff of approximately 6×10^{19} eV. There is some evidence to suggest that cosmic rays can be seen above this limit with energies above those expected from conventional acceleration mechanisms – possibly

indicating "new" physics. The most interesting events above 10^{20}eV have a flux at least two orders of magnitude lower than that for events at 10^{19}eV. Events have been reported with energies as high as $3x10^{20}$ eV. If these events are real, their sources cannot be distant cosmological sources because of the energy loss resulting from interactions with the cosmic microwave background radiation. These interactions limit the origin of such energetic particles to sources within about 100 Mpc. Learning the nature of these cosmic rays may point to some new physics processes or a new class of cosmic accelerators.

The study of the spectrum and sources of highest energy cosmic rays is part of a larger effort to understand energetic astrophysical sources of high energy particles, including supernova remnants, active galaxies, gamma-ray burst sources and colliding galaxies, as well as possible sources such as topological defects or collisions of massive neutron stars. Other goals are to measure some aspects of high energy gamma-rays and neutrinos as well as measurements of cosmic rays at somewhat lower energies. The top end of the cosmic-ray spectrum is of particular interest because these particles are almost certainly of extra-galactic origin, but also simply because of their enormously high energy, orders of magnitude beyond energies accessible with machines on earth. The barrier to progress in this quest is the rapidly falling flux, which demands detectors which combine acceptance and duty factor to yield a usable event rate.

Because of the size and international nature of this project, SAGENAP felt that it was necessary to take more time to examine thoroughly the science goals, the technical feasibility, and the appropriateness of the Auger technique to address this science with great care; the construction of Auger would almost certainly limit the flexibility in the U.S. non-accelerator program for some years. For this reason, a second meeting of SAGENAP was held which focused on this experiment.

Method/Experimental Issues

The Auger collaboration proposes to build two arrays, each 3000 km^2 in area, one in the southern hemisphere (San Rafael, Argentina) and one in the northern hemisphere (Millard County, Utah). Detector arrays are located in each hemisphere to give full sky coverage. Since the arrays are identical in each hemisphere the systematics of both arrays will be similar. This feature allows a more sensitive search for galactic anisotropy, dipole and quadrupole moments of source distributions, and eliminates holes in the sky when cataloguing point source distributions.

The ground arrays consist of water tanks and photomultipliers to detect large air showers and measure the energy and will provide some information on the particle type. To calibrate these ground arrays, Auger proposes building fluorescence detectors to detect events (~10%) in hybrid mode with both kinds of detectors. The fluorescent and extended air shower array detector types are complementary and can be used to reduce systematic errors, particularly on energy determination. This will allow the first cross-calibration for any high energy detector on an event-by-event basis. Conversely, the same data set can

measure the effective sensitivity of the Fluorescence technique at the lower energies, a parameter which is determined by computer simulation at the moment. The cross calibration will be best at lower energies due to the reduced statistics of hybrid events. However, the hybrid design provides confidence about the more controversial and exciting possibilities. Do super-GKZ events exist? If they do, and do not point to known high energy sources, then this is so suggestive of new physics that it will be important to convince the physics community of the validity of the data. Doubts will continue to plague the single method techniques until cross-calibration exists.

The proposal results from a 5 year study and involves an international collaboration with physicists from about 10 countries. The technology here is mostly conventional. The newest part is the wireless communications. There are a large number of ways by which the highest energy cosmic rays might be detected. The Auger team has chosen to combine the two most established methods in a conservative experiment that is almost certainly guaranteed to achieve its predicted sensitivity. As noted above there is a very definite merit in having two distinct but overlapping detectors; this is one of the real strengths of the Auger experiment. These are fairly simple techniques and there is little mystery to their use; the Auger team is large and experienced and includes members who have extensive experience with both kinds of detector.

Cost/Schedule

The total cost is estimated to be $100M for the two detectors, to be constructed over 4 to 5 years. The proposed funding from the U.S. is for $30.6M, including site preparation and contingency. However, beyond the U.S. contribution, 25% of the funding for the full detector is not in hand, and there was no indication that it will be in hand in the near future.

Comments from SAGENAP

This proposal generated a lot of discussion by SAGENAP. The discussion raised several key issues on many of which there was a wide range of opinions. It was agreed that SAGENAP needed to reach a strong consensus if Auger was to be funded.

How Important is the Science? What Priority should it be given to it?

Probing the high energy frontier is a scientifically important goal and SAGENAP all agreed that this science was interesting and worth pursuing. The importance of this science is clear from the fact that it is already being pursued - HiRes, currently in construction and scheduled for completion in 1999, will provide information (for part of the sky) on the most important of the questions that Auger will study, although not with as high statistics. Auger will collect 10x more high-energy events than the High Resolution Fly's Eye (HiRES), most of them with the ground array. It was generally agreed that the most important thing to establish is if the cosmic ray energy spectrum

continues above the GKZ limit and if so, if there is a cut-off in this spectrum. The issue of sources and the issue of composition were also extensively debated. Finally, the scientific case for having two sites in order to study anisotropies in the distribution of sources was questioned by the group. Some members felt that it was premature to consider the anisotropy of these sources given what is currently known.

SAGENAP could not reach a consensus on the relative importance of the science of extremely high energy cosmic rays compared to that addressed by other non-accelerator physics experiments. There are many other scientific issues in the non-accelerator program, such as the study of solar and atmospheric neutrino oscillations, the pursuit of dark matter, and the study of high-energy gamma-ray astronomy. These were felt to all be possible areas of further investigation with well-posed scientific questions and with available and proposed detectors capable of addressing the questions. All of these experiments also raise the possibility for the discovery of new phenomena. While it was agreed that it was scientifically appropriate to pursue the science of extremely high energy cosmic rays, it was not agreed that it was imperative to pursue a very large program in this area to the possible exclusion of others.

The appropriate timeline for building an experiment with increased capabilities compared to HiRes was discussed in detail. Given that a substantial investment has already been made in this physics, and that significant information will be added to our knowledge about extremely high energy cosmic rays before Auger is on-line, some of SAGENAP felt that it was premature to build Auger. Other members argued that the increased capabilities of Auger and the importance of the science would require only that it be demonstrated that an optimal Auger design did not depend on the outcome of the HiRes experiment.

Collaboration

SAGENAP was impressed by the strengths of the collaboration and its progress to date. They also felt that the proposal contained many elements that should be encouraged for big-ticket programs in particle astrophysics , namely international cooperation and cost sharing. SAGENAP's opinion was that the Auger collaboration has many highly talented individuals who have advanced the interest in cosmic rays at extreme energies. They encouraged the leadership at the funding agencies to consider maintaining the capabilities and professionalism of the Auger collaboration by supplying some R&D funding.

Capabilities of Auger as designed

Given their scientific goals, the Auger detector must be able to accumulate a set of events above 4×10^{19} eV and 10^{20} eV with adequate energy, angular, and particle species resolution. There was some skepticism expressed about their claimed resolutions and whether the data from the ground array will be useful for identifying the particle species. Having two detector types helps, since the hybrid design provides a far more reliable assessment of energy and more handles for particle ID.

It was noted that if the energy calibrations of the AGASA, Fly's Eye, Hi Res, or Haverah Park results are not believable separately (though a good case has been made that these techniques are reliable), then the hybrid technique might not be that much more convincing. One reason for this is that the lower duty cycle of the fluorescent detectors reduces statistics on the number of events which can be cross calibrated (to about 10%) and limits this check to lower energies. This means that one must extrapolate to determine the higher energy calibration, and it is not certain that this extrapolation can be done well enough to convince critics of the technique.

In addition, the problem of mass/composition identification is very hard, as is shown from long experience with air shower arrays trying to measure the composition near the knee. Most proposals show mass separation in a highly idealized way, but complications are inevitable. The particles arrive with unknown energy spectra, and the composition may not be purely protons or iron (especially if there is an admixture of galactic and extragalactic particles in the data). While progress is being made on the question of composition near the knee by the surface arrays using hybrid approaches (air shower arrays combined with muon detectors and airCerenkov telescopes), they will need additional input from detectors which directly measure the composition to provide the critical normalization and calibration. Some detailed technical concerns are beyond the scope of the SAGENAP review (such as what is the effect of actual shower fluctuations, which have not been simulated, on the stated resolutions).

Hybrid designs are no panacea when it comes to the question of composition. It was felt that more work needed to be done with the simulations putting in realistic energy spectra, mixed compositions, and variants on the fragmentation models. It is important to see how all of the independent handles from Auger (depth of shower max, mu/e ratio, timing the wavefront, etc) are used to identify the nature of the particle. SAGENAP felt that this was a matter of degree. Auger was likely to be able to answer the question "are the particles that are detected at extreme energies all photons, all protons, or all iron?". If the particles at extreme energies are protons then Auger could provide a definitive answer. However, the observation that the high-energy cosmic rays are protons and cluster in the super-galactic plane with no outstanding bright spots (perhaps the most likely outcome) could accommodate a variety of sources from topological defects to radio galaxies to acceleration in galactic bow shocks.

The angular resolution is limited by bending in the intergalactic magnetic field: Even a particle with energy 10^{20}eV produced at a distance of 30 Mpc will be scattered by 1.5° in an intergalactic field of 1 nG. This fact, as well as possible confusion from "accidental" overlaps limits the point sources of cosmic rays that could be discovered to those that emit in the above energy band. Certainly, the larger magnetic rigidity will help if there are a

handful of discrete sources, but the crude information we have to date indicates that any anisotropy is at best small and might correlate only with large clusters of mass (the supergalactic plane), not point sources.

HiRes

The HiRes program is fully funded and scheduled for complete operation in 1999. Several important issues may be answered by 3 years of HiRes data. For example, HiRes will have ~40 events above $5x10^{19}$ eV with stereo mode reconstruction. This experiment should be able to answer the basic question - does the GKZ mechanism affect the energy spectrum of the cosmic rays at the highest energies? In addition, it can look for obvious correlations with likely sources for cosmic rays with energies above $5x10^{19}$ eV , check for systematic effects in shower profile, energy resolution, or event location relative to the horizon. If any of these show unexplainable behavior, then the case for a hybrid cross-calibration will be strengthened considerably.

Some of SAGENAP felt that the agencies should consider augmenting the pace of funding for HiRES so as to speed its completion and urge a strengthening and broadening of the HiRES team. HiRes will certainly be the fastest route to the first glimpse at this physics. HiRes is also a scalable detector. The questions remain, however, as to what type of hybrid array would be a better approach.

Required data set size

The acceptance of HiRes is energy dependent and that of Auger is not. Thus, Auger is 25x larger than HiRes at 10^{19} eV and 14x larger at 10^{20}eV. Clearly, more statistics are better. However, even if there is some uncertainty in establishing the energy of individual events, the flux should be established with some certainty by HiRes.(NB The question of the uncertainty in the distance to showers detected by one Fly's Eye leading to uncertainties in the reconstructed energies of cosmic rays is precisely the reason HiRes views showers in stereo). Here the sample of events in HiRes will be around 3000 events above 10^{19} in three years.

Timelines

The timing of this proposal was one of the main discussion issues. There was agreement that the science should be done, but not about when or how it should be done. Since Hi Res itself should provide an order of magnitude increase in statistics compared to current data, some of SAGENAP favored waiting. Others argued that the Auger collaboration was ready to begin construction now and that a delay could cause the collaboration problems in holding together. Many of SAGENAP favored encouraging the Auger collaboration to propose a next step (after HiRes) for research in this area. The exact form of this step was debated but not resolved.

What Approach Should be Taken to Optimize Progress in this Area?

SAGENAP agreed that a hybrid approach which provides more information on a subset of the events was more reliable than either a pure air fluorescent detector or an array which only consisted of shower detectors. They agreed that having two detectors, one in each hemisphere, would optimize the study of anisotropies. However, beyond these conclusions, a wide variety of opinions were expressed about possible future scenarios.

- Building Auger as proposed.
- Building one of the two Auger arrays.
- Only building the shower detectors.
- Taking a staged approach.
- Building a reduced scope Auger array in Utah
- Continuing with present plans for HiRes (and possibly accelerating the schedule).
- Building an extended fluorescent detector, possibly with Japanese involvement.

The Auger group is not the only group working on these ideas. There is a specific proposal to construct two large ground arrays (Auger), an ongoing project using fluorescence detectors (HiRes), a possible expansion of the fluorescence array in Utah in collaboration with a Japanese consortium (Telescope Array), and a possible satellite-based technique (Owl). SAGENAP also heard an idea from Dave Kieda that, while interesting, is not yet mature.

It was apparent that there is significant divergence in the detector design optimizations and technical approaches by the specialists (e.g., HiRes, Telescope Array) which made it hard to convince the entire panel that the Auger approach was the only approach to this science. SAGENAP felt that co-operation between the different groups was essential in order to maximize the scientific impact and minimize the cost. In particular, they wanted to encourage collaboration or at the least good communication with the HiRes project and with the Japanese Telescope Array Project. Many panelists felt that the statements presented SAGENAP concerning the distances over which the fluorescence technique can be made to work may make it possible to reduce the cost for the minimal Auger array, or, with funding from Japan, to construct a significant fluorescence detector, which would complement, and possibly extend the scientific reach of the international ultra high energy cosmic ray program. SAGENAP diverged when it came to recommending the specifics of the approach.

Because one is sitting beneath the earth's atmosphere, one is observing the products of the interaction of cosmic rays with the atmosphere generating extensive air showers: one must rely on detailed simulations of these showers. It is commonly believed that the fluorescence technique is a fairly robust method to measure both the primary particle energy and its composition; this is because it is a calorimetric measurement and one obtains a profile of the longitudinal profile of the shower development. However, one is relying on an understanding of the optical transmission of the atmosphere. When only one

fluorescence detector is used ("monocular") one has an uncertainty in the distance to the shower plane leading to an overall uncertainty in the energy; stereo observations remove this problem. Use of a ground array such as Auger proposed, if it were used alone for the large aperture device, is believed to lead to a less robust measurements of energy and composition and is thus believed to be more prone to error due to shower fluctuations; basically this is because one is measuring properties of the shower at only one depth in its development.

The hybrid design, such as Auger actually proposed, does provide a more robust strategy for the evaluation of the low energy response , but it was not obvious that this technique works well enough at the highest energies. The hybrid design generates better information for particle ID for the subset of events which can be observed by both detectors. This is noncontroversial; but, is it sufficient to do the job? If the goal is to distinguish gammas from hadrons, then yes, but so can HiRes. If the goal is to distinguish p from Fe, then probably - but not everyone was fully confident of this given the current state of composition studies near the knee. Again, HiRes has some capability through Xmax, and once this data exists it may be become imperative to measure the composition more carefully. However, it is clear that Auger could substantially advance the quality of data and provide an excellent measure of the critical things we want to know (Are these events all gammas, protons, Fe or mixed?). If sources exist, the same question would be asked for each source. If mixed, then Auger would be better than HiRes, and may be required.

SAGENAP debated, however, whether the balance of ground arrays and air fluorescent detectors proposed by Auger was optimal. As stated above, data from the ground array alone is of lower quality than fluorescence data. The idea of having some hybrid data is a good one, as one will then have more information for these events, which should lead to smaller uncertainties and a check of systematic errors. The Auger plan does not take advantage of all of the capabilities of fluorescence detectors. However, the Auger approach may still represent the best way to cover and calibrate the largest aperture at lowest cost. The ground EAS array, based heavily on the Haverah Park experience, appears to be a clever cost-effective way to get to a significant improvement in aperture than heretofore available.

One option might be a staged approach. Several ground array tanks could be constructed on-site in order to test the radio communcation links, assess the variability of water transparency in the arid environment of the proposed site, and gain experience with the level of vandalism at these unprotected sites. Partial funding could be used to construct one fluorescent detector with some water tanks around it. This could be used to verify the utility of the hybrid approach. It was suggested that HiRes could be used to act as the fluorescent detector. It was argued that once the inter-calibrations between the fluorescence and water techniques have been performed, the case for the lower duty cycle fluorescence telescopes could either be made stronger, or the approach modified if it became apparent that the fluorescence telescopes were not worth the additional investment.

For these reasons, the majority of SAGENAP felt that the co-location of HiRes and Auger would be mutually beneficial. The ground array needs a fluorescence detector for calibration. The HiRes technique could benefit from (and indeed may need) a ground array to confirm the way its acceptance grows with energy. Thus a natural next step beyond the present HiRes detector would be to locate it inside an array like that proposed by Auger, or to build ground arrays on the HiRes site. Recognizing the strengths and expertise of the two groups, several panelists wanted to encourage a study to see whether a joint proposal for a somewhat reduced version of the Auger ground array with a HiRes type fluorescence detector would be feasible for the Northern hemisphere detector.

Funding Issues

While SAGENAP argued that the proposal to the U.S. was reasonable from a certain point of view, namely that $30M over five years and two funding agencies is only $3M per agency per year, they also agreed that this was an extraordinarily large amount of money relative to the funding presently available for non-accelerator physics. Concerns were raised that since much of particle astrophysics competes with other well established sciences, principally elementary particle physics and astronomy, it is hard to get significant new funding in the field. It was felt that other projects, including the GLAST gamma ray observatory, and a future very large neutrino observatory, will have similar problems being funded. It was suggested that the funding agencies (DOE, NSF, and NASA) should study this problem.

Concerns were raised that not all of the needed funding was in place for the international project (about $27M was not accounted for, of the $100 M total) and that the U.S. might be asked to increase its contribution to cover either this shortfall or problems with the other projected contributions. There was also a comment that the contingency allocated might be too small. SAGENAP also felt in general that a more modest proposal to address this science would be more competitive and that descoping options should be seriously considered.

Some of SAGENAP worried that if the project was not funded in the U.S. that the experiment might not proceed and that this would be a set-back for the prospect of studying extremely high energy cosmic rays. It was argued that only Professor Cronin could have assembled this collaboration and that it would be hard to form such a collaboration again. Various suggestions were made as to how to fund Auger if approved, these ranged from a redirection of Fermilab funding, to joint funding by DOE and NSF, to searching for outside funding from private sources.

Summary

Areas of Agreement

Not to proceed with the design as proposed
The collaboration is strong
The hybrid technique is favored
Collaboration is recommended between different groups involved in this science

Areas of Contention

When to take next step
How to optimize the design

Recommendations

SAGENAP does not recommend approval of Auger as currently proposed.

SAGENAP strongly endorses the science goals of the Auger project. The collaboration is very strong and has done impressive work in bringing the project to its current stage of development. However, SAGENAP has significant reservations about the scope of the proposal and the conceptual approach to resonance fluorescent detectors as proposed. The recommendation is for the collaboration to look closely at descoping options, making every effort to reduce costs, and optimizing the use of the fluorescent detectors making use of their unique features for addressing this science.

SAGENAP encouraged the leadership at the funding agencies to consider maintaining the capabilities and professionalism of the Auger collaboration by supplying some R&D funding.

Neutrino Physics

Science

In the past decade, we have observed neutrinos from supernovae, the sun, and atmospheric neutrinos produced from high energy primary cosmic rays interacting with the Earth's atmosphere. These neutrinos are extremely important probes both of the physics of the sources and of the possibility of neutrino mass and mixing. High energy gamma-ray astronomy, while it has pointed to some gamma-ray sources such as Active Galactic Nuclei (AGN's), has not made the case for the existence of neutrino emission from these sources (all the gamma-ray sources could, in principle, have electron progenitors). This means that neutrino astronomy is highly explorative since there is no experimental basis for any ultra high energy source fluxes. Nevertheless, neutrino astronomy represents an exciting new frontier for astronomy and physics research.

Neutrinos are a unique tracer of energetic hadron acceleration in sources. Photons can be produced by hadronic or electromagnetic processes such as Inverse Compton scattering or Synchrotron radiation of energetic electrons, however, neutrinos can only be produced by hadronic interactions. Neutrinos (or the lack of them) can in this case be used as a diagnostic tool to understand the acceleration mechanism. Since gamma-rays can be absorbed in the source or on the way from the source to the earth, neutrinos which only interact weakly open a totally new observational window with the potential to reveal previously unknown point sources. The principal scientific goal of the experiments presented to SAGENAP is to study neutrinos with energies above 10 TeV, where the reasonably well understood atmospheric neutrinos become less plentiful than the posited extragalactic neutrinos.

There is no consensus on the best technique to use for neutrino detection nor on whether there are astrophysical sources powerful enough to be observable, even with a km^3 detector. Very large neutrino detectors, like SuperKamiokande, MACRO and Baksan appear to be too small to effectively study these objects. The best possibility appears to be to instrument an existing transparent medium to detect neutrinos (e.g. ice or water), where a very large volume can be covered economically and to take advantage of the fact that the neutrinos detected have passed through the bulk of the Earth. The pursuit of neutrino telescopes is a worldwide activity (AMANDA, NESTOR, ANTARES, and Baikal). AMANDA and the Lake Baikal project appear to be making good headway towards initial measurements. Astrophysical neutrino detectors are difficult and expensive to build and, of necessity, investment in this area is a gamble. However the techniques are interesting in themselves, along the way some interesting measurements can be made and there may be a spectacular pay-off.

Km^3

Method/Experimental Issues

A group from the Lawrence Berkeley National Laboratory presented a systematic approach to the eventual ocean deployment of a 1 km^2 in area neutrino detector. The proposals presented were for R&D for simulations, for electronics systems, and for ocean deployment.

The deep ocean detectors have the advantage over surface detectors of a vastly reduced cosmic-ray muon flux to contend with. This gives them the ability to look higher up toward the horizon and implies a larger sensitivity for a given surface area. Their major disadvantages include the difficult ocean installation and the high singles rates produced by the K-40 in the sea water. The DUMAND collaboration struggled with these difficulties for many years before its demise and it is now clear that the funding and support facilities necessary to mount a successful underwater operation is probably considerably more than was first proposed.

The proposed site of the new detector near Los Angeles and the proposed location of junctions at a shallow sea-mount are attractive ways of minimizing the risk of problems that DUMAND encountered as a consequence of the great depth of its junction box and its relatively remote location.

Cost/Schedule

This proposal requested $1.7M over three years. The total cost associated with the three years of Stage I is $4.5 million. Stage II would be a two year demonstration project of 6-12 strings of deployed photo-multiplier tubes having an effective area of 30,000 m^2 , Stage III would be a three year exploratory science project with an effective area of 200,000 m^2, and Stage IV would be the full implementation with an effective area of 1 km^2. There was no estimate given regarding the cost of the full cubic kilometer detector which would begin to be constructed after an eight year development and exploratory science program. The total cost of this set of proposals is driven by the cost of the ocean deployment R&D.

Comments from SAGENAP

The presenters argued that they were addressing the difficulties that proved fatal for the DUMAND project, but SAGENAP was not convinced. SAGENAP felt that the DUMAND experience showed that a project which requires placing phototubes in such an inaccessible place requires a higher level of engineering and quality control. Potential problems still exist in obtaining the needed ocean engineering, deployment facilities and expertise. The assertion that it will be easy to get ships of opportunity remains to be justified.

The expenses involved in the Monte Carlo work were felt to be high. It was thought that the simulations would get done in a number of places, whether this proposal was funded or not.

The research on a Digital Optical Module, including testing at AMANDA was commended. The electronics approach proposed was considered reasonable for this scale of detector, but perhaps not be sufficiently conservative. However, it was felt that this work was worth continuing if there was a chance it could be used somewhere.

SAGENAP felt that the issues raised by these proposals (Km^3 and AMANDA) were not primarily related to scientific merit but to their timeliness and cost. A decision to start on R&D for an ocean deployment of a neutrino detector has to take into account the probability of a successful deployment in polar ice which appears to have a number of advantages over the ocean. The AMANDA collaboration has been testing this idea and has been very successful in installing the required equipment in the ice. However, it remains to be seen whether the disadvantage, due to the fact that light scattering in the ice

appears to be larger than that in the deep ocean water, can be overcome. The proponents of ice detectors still need to show that this technique can be successful and that it will lead to an economically feasible detector of the required sensitive size.

The time lost in recovering from the DUMAND program puts an ocean detector at a disadvantage relative to AMANDA. There are also efforts in Europe to develop an underwater detector. The Baikal experiment began in 1980 and has had some recent success in large measure due to the significant influx of money, technology and manpower from the former East German group led by Christian Spering. The NESTOR and ANTARES experiments are in the development stage and hope to deploy small test arrays in the Mediterranean Sea.

Eventually results from AMANDA and the accumulated experience with deep underwater detectors will help decide the best way to move toward a very large neutrino detector. This proposed detector and the following stage II proposals would give five years from now an ocean deployed detector one half the effective area the AMANDA project will have in two or three years, but the ocean detector would be much deeper.

AMANDA

Method/Experimental Issues

The AMANDA project seeks to measure the flux of ultra high energy neutrinos incident upon the earth from both diffuse and point sources. The collaboration has initiated and partially demonstrated the feasibility of the technique of using the optically transparent South Pole ice as a detector. Upward going neutrinos produce highly collimated upward going secondary charged particles in the target rock and ice. These particles are detected through Cerenkov radiation in the ice. The major concerns with the AMANDA project are that the ice may not be suitable as a particle physics detector either for physical reasons such as light scattering, or for logistics reasons given the difficulties of doing anything at the South Pole. There is also the possibility that there might be some unforeseen physical or technical difficulty associated with the operation of photomultiplier tubes (PMTs) in ice, in particular, that there might be some unforeseen breakdown with time. Also, AMANDA cannot look at sources in the Southern sky.

Progress at the Pole has been impressive and there seem to be no major problems with infrastructure. The novel technique of drilling to great depths with hot water, then deploying the strings of detectors before the holes refreeze, has worked well; so far there is no impediment for expanding this technique. While the deployment was successful; the performance of the first 4 working strings of 20 optical modules each placed 800m under the ice in 1993-94 was not satisfactory. The performance of these detectors was found to be dominated by the effects of micro-bubbles in the ice which scattered and absorbed the light. This was not anticipated and took considerable effort to understand. A second, considerably deeper deployment in 1995-96 at 1500m has shown a markedly improved

performance. Initial studies at this deeper site have been to characterize the ice. The properties of ice at these depths are still not fully determined, and although the initial worries about bubbles seem to have been answered, some questions remain to be answered (see below under discussion).

Cost/Schedule

The overall plan presented at the SAGENAP review was a staged approach to develop AMANDA to a km^3 scale over the next 5 years. The AMANDA project is based at the South Pole. It takes advantage of and relies on the infrastructure from the NSF Polar program and the majority of the funding comes through that program. The future program is linked to that of the polar program at the NSF. It is assumed that AMANDA can continue, if scientifically , technically and financially viable, within the context of polar programs.

The deployment of 10 deep strings, each with modules between about 1500 and 2500 m depth, starting next season will begin the construction of AMANDA-II. At present, ten strings of PMT's have been deployed in the AMANDA-B configuration at depths of 1500-2000 meters. (The shallower deployment limited by short scattering lengths with four strings of PMT's tubes was called AMANDA-A). Capital funding (from polar programs) is in place which will extend the AMANDA-B configuration by ten additional strings. The combined twenty strings, called AMANDA-II, will have an effective area of approximately 0.06 km^2.

While the staged approach seemed reasonable SAGENAP considered it premature to give approval to it at this time. SAGENAP felt that good progress was being made in understanding the detector to continue with the exploitation of the current detector and the prototyping and studies leading to AMANDA-II. However, the long term plan was not spelled out in any detail and will depend on the results of the analysis of the present array. Details of the number of strings, spacing, etc. to reach that area are still not determined and could affect the cost and performance. If no important problems are uncovered in reconstruction, the plan for making a km^3 array over a period of years seems viable. Even if this research does not lead to building a km^3 neutrino ice detector it will give valuable information both as a complement and competition to the deep ocean ideas.

Comments from SAGENAP

The effective area of AMANDA-II should be large enough to enable the collaboration to make a thorough study of atmospheric neutrinos. This important milestone will permit a measurement of the primary background process to the study of ultra high energy neutrinos. The flux of atmospheric neutrinos at energies above several TeV should be measured, the neutrino flavor composition can be studied (although it is not clear from the data provided how well this can be done), and the angular distribution of the events can be determined. It is possible that a statement could be made about the presence or absence of

flavor oscillation of atmospheric neutrinos. This could become a very important confirmation of the effects seen in the Kamiokande and IMB detectors. This accomplishment will be important in showing the successful instrumentation of a large volume of detector, especially since the initial deployment produced discouraging results on the scattering of light in ice.

To do this physics the collaboration must be able to reconstruct muon tracks in the ice. The array is now working and the deeper, 10-string AMANDA-B is able to see downward going muons clearly. SAGENAP wanted to know what limitations that light scattering in the deep ice imposes on the ultimate performance of AMANDA - specifically what ultimate angular resolution can be achieved compared with the design angular resolution in the proposal and how well they can identify upward-going events. While initial results look promising, no definitive reconstruction of upward muons (from neutrino interactions) with clear background rejection has yet been demonstrated. The reconstructed muons they showed were a bit "too hot off the press" to really see how well the detector will perform. The group must demonstrate that they can determine the origin of muons as a test of the method. A disadvantage of the ice is that the depth is limited, which implies a higher background from downward atmospheric muons than would be present at a deeper ocean site. Thus the ability to distinguish upward muons is a key challenge for this experiment.

SAGENAP thought the AMANDA collaboration had been focusing on their initial deployment and extending the capability of the detector, which they needed to do, and that they had not been able to maintain a suitable pace for data analysis. The experimenters seemed to be focused on two major objectives: first, to complete the array strings B, and II; second, to develop the technology of strings. It was strongly felt that more effort in data analysis was needed, perhaps by augmenting the collaboration. Concern was expressed that the current collaboration does not have the right mix of participants. The inclusion of new collaborators (perhaps at the postdoc level) who are most interested in data reduction and the possible detection of astrophysical sources would be beneficial. In particular, in order to make the step to the km^2 effective area, it was felt that this project would need substantially increased resources and higher level management tools. The AMANDA collaboration as presently constituted was not considered likely to be able to build and operate a project of this scale.

SAGENAP thought that the rush for the technical development might have been driven by the belief that a decision on which Km^3 detector proposal to pursue was imminent. SAGENAP stated very strongly that they wanted experience to be gained with a detector in ice before encouraging in any way the building of a Km^3 detector. The problems and insights necessary to optimize the future development of such detectors will likely to be revealed in the actual reconstruction of events, study of tails, scattering, etc. These in-depth studies are now thought to be at least as important as the technical developments that have dominated the effort thus far. Funding beyond the present round should be made contingent on passing these milestones.

SAGENAP recommended that the collaboration maintain the focus on the original goals and ensure the spacing and number of strings are optimized for high energy detection. The discussions of the physics promise of AMANDA often includes goals relying on good low energy neutrino detection. This requires much finer spacing thought to be incompatible with the practical realization of a Km^3 detector. Although a central core might well be of finer grain, it is unlikely to be larger or better than detectors such as SuperKamiokande for this physics. The three scientific justifications presented did not convince SAGENAP of the need for finer spacing.

Recommendations

Km^3

It would be premature to fund this project now to build and deploy 3 strings. However, given the interest in moving toward a kilometer-scale neutrino detector the spending of modest resources from existing funds, or from institutional development funds in this R&D area, is entirely appropriate.

The LBNL group has made good progress in developing a design for an optical module that very well could be the answer to the basic system component necessary for the string design. Both the AMANDA and ANTARES collaborations are interested in this design. The LBNL group should be encouraged to continue this work.

A detector that can look at southern hemisphere sources may be needed. The deep ocean detectors appear to be the best prospect for this. Given the relatively undetermined cost of mounting such an experiment, the resources required to demonstrate this technique may have to come in the context of an international collaboration. Participation of this group in such an effort is preferred to the mounting of another completely independent effort in the U.S..

While R&D is appropriate, it is premature to consider committing to a deepwater project in the U.S.. Any decision on such a detector should take experience with AMANDA and other detectors into consideration.

AMANDA

This is a project that has made impressive progress. Continuing support is warranted and an increase in funding, if possible, would be beneficial.

Approval for the Km^3 array should not given at this stage. The current detector (0.01 km^3) must be completed and operated for a reasonable length of time.

The collaboration needs to increase the efforts going on in data analysis and demonstrate that they can reconstruct upward going muons.

The collaboration needs to consider strengthening itself, including the possibility of expanding.

CDMS-II

Science

One of the most important problems in astrophysics is the nature of the dark matter. There is much evidence for the existence of dark matter; from the rotation curves in spiral galaxies to velocity dispersion in clusters. At least as importantly, it appears necessary for the development of a consistent picture of cosmology e.g. primordial nucleosynthesis) that there is a substantial non-baryonic component to dark matter. Dark matter candidates include Weakly Interacting Massive Particles (WIMPs), axions, light neutrinos and magnetic monopoles. The lightest supersymmetric partner could be a WIMP.

Since these particles only interact weakly, searching for WIMPs presents severe experimental challenges due to the low event rates The dark matter experiments search more generically for WIMPS than is possible for experiments at accelerators. Indirect searches have yielded useful limits. They have looked, for example, for high energy neutrinos emerging from the center of the earth or from the sun that signal the gravitational capture and annihilation of WIMPs and anti-WIMPs at the center of the sun or earth. Direct searches for WIMPs are less model dependent and therefore more definitive.

Method/Experimental Issues

The CDMS collaboration has been working for a number of years to design and develop a detector which could discover WIMPS directly by measuring the ionization or excitation of ordinary matter when it undergoes elastic collisions with a WIMP. This requires sensitivity to very low energy recoils, a formidable technical challenge requiring cryogenic techniques not normally used by particle physicists. The very high background rejection needed means that a good discrimination signature is needed and that the work must be done in an extremely low radioactive background environment. The experiment must also be done on a very large scale (kilogram mass targets) due to the low WIMP fluxes predicted using the expected cross sections and the existing astronomical constraints.

The detector development program of the CDMS group has finally produced large (~100 gram) cryogenic detectors of sufficient quality that an attempt to search for WIMPS with a kilogram of detector mass is now feasible. To achieve this the group has developed two related but different cryogenic techniques. Both make use of the simultaneous detection of phonons and ionization resulting from recoils in crystals (Ge in one case and Si in the

other), but in the former a thermistor readout with warm electronics is used, while in the latter a super-conducting thin film and SQUID are employed. The phonon + ionization method has enabled them to make an impressive breakthrough in signature discrimination since recoiling nuclei can be separated from the electrons which are a major background using these two variables. Currently they have installed and are reading out about 0.1 kg of each type of crystal. Some of this data was presented at SAGENAP.

Both experiments are achieving a 99% rejection of electrons (from Comptons and beta-decay) and there seems to be a good understanding of the residual sources of electrons. The neutron background is well characterized and consistent with their expectations for this shallow site from previous surveys, Monte Carlos and anti-coincidence shield efficiency tests. While the performance of the individual detectors (e.g. FWHM resolution and thresholds) is not quite at their ultimate goal they appear to be in striking distance and the performance they have achieved is very impressive and in line with their expectations. They have demonstrated an energy threshold of around 15 keV (and expect to reach 2 keV) The present background rate from neutron recoils in the Ge running is an impressive 0.08 cts/keV/kg/day (the goal is 0.01 cts/kg/keV/day). The photon background at energies above 15keV is about a factor of three above the goal. Already, from their ~2-kg-day running they have a dark matter detection limit that is quite competitive with the best that has been done by other techniques which are already approaching their systematic limits. The present operating plan for further experimentation in this shallow site is to push it to the expected background limit by adding more detector units into the present cryogenic setup. The experiment will use both Ge and Si crystals (this helps in neutron background diagnostics as well as mass identification in case of a positive result) and will have 400 gm of Ge-73 --providing some sensitivity to axial vector interactions. With this arrangement the collaboration expects to have ~250-kg-days of data, to have improved the limit by a factor 70 and to have reached into the region of the theoretically estimated rates for candidate particle physics models.

In addition to performing a useful physics measurement the collaboration will gain experience. They are proposing a move to the much deeper site at the Soudan Mine in Minnesota and to increase the mass over the final CDMS-I amount by a factor ten. The deeper mine should give an improvement of perhaps a factor 50 in the background rate for neutrons. The plan is to install 35 240gm Ge crystals and 7 100gm Si crystals. These would be arranged into 7 towers of six crystals each. Improvement in electron background would come from improvements in cleanliness of handling and self-shielding. They would expect to run for about five times the Stanford time. They estimate that the overall improvements should give them a gain of another factor 30 beyond the Stanford experiment and push them well into the theoretical candidate range for supersymmetric (SUSY) dark matter candidates..

Cost/Schedule

The plan outlined by the group is to pursue the present prototype experiment through 1998, when they should have 100 kg-days/tower of data and will have an improved sensitivity of nearly 100 over present limits. During the same period, they propose to begin construction of CDMS II. The details are not yet determined and need results from the present effort, but the goal is to go to a deep mine site (probably the Soudan mine) and increase both the detector mass (x10) and exposure time (x5). At this sensitivity the deep mine site will be required for background rejection.

The incremental cost of this program through the year 2000 was presented to be at a level of ~$5.5M. About $1M is already in hand from an NSF-ARI award for cryogenics development which will be used as part of the staging of the experiment at Soudan. In addition, there would be ongoing costs for the base support of the collaborating groups and the Center for Particle Astrophysics. These operating costs were not presented in detail but were assumed to continue at present levels.

The time lines presented for the various tasks were used to argue for an early decision for optimum deployment. The production of the many crystals from start to finish is a very labor intensive and high tech process involving as it does the fabrication of the crystal, attachment of electrodes and phonon sensors, operational testing and characterization and at the same time maintaining essentially clean room conditions for preventing accumulation of activity. They estimate about one week per crystal in initial production into the subsequent handling pipeline. Prior to this they have a crucial decision to make, namely a choice of a single, phonon readout technique (thermistor vs superconducting films). They want to make this decision by Fall '97. The decision will be based on subsequent performance in CDMS-I and further tests in which the tungsten film technique is tried on Ge instead of Si. The electronics will also depend somewhat on this choice and they need to plan for that. The electronics fabrication itself was presented as taking a long time but with the potential for spin-off applications.

Comments from SAGENAP

The group has brought the technology to a level where a large scale experiment appears feasible. Within about 5 years a full scale sensitive detector for direct detection of cold dark matter could be developed. The experiment is arguably the best effort in the world with the best chance of successfully detecting WIMPs or of setting definitive limits.

The group has been a pioneering one in the initiation and development of cryogenic detectors for just this purpose. Their stated aim is to have sensitivity which will cover a substantial amount of the available parameter space for dark matter candidates which would make the discovery potential of the experiment very significant. Future developments - such as high spin nuclei enriched targets - of their technique might well open up still further discovery reach.

Expansion of the detector should be straightforward. There should be no new wrinkles associated with the cryogenics --- it is all similar in scale and degree of difficulty to what they already have in hand. All in all what they propose is quite a reasonable and careful approach and promises to be an excellent experiment The combination of CDMS-I and CDMS-II would improve upon the present sensitivity by a factor 2000. This would represent a sensitivity to a cross-section of a few 10^{-45} cm^2 at ~80 GeV/c^2 of particle mass. This sensitivity overlaps that of theoretical predictions for SUSY WIMPs.

SAGENAP would have liked the collaboration to comment on the advantages and disadvantages of moving the infrastructure of the current detector to the Soudan mine at the end of useful running at the Stanford site. Could cost savings be made by re-using the cryogenics , for example?

There are several efforts in the world to detect WIMPs directly, but this seemed the most promising to SAGENAP. The competition can be divided up into two categories. First, there are two principal cryogenic competitors neither of which have active particle discrimination, are in somewhat different mass ranges, have different form factor effects, etc --- in terms of preparation and sensitivity it was felt that they were more comparable to CDMS-I though both have ambitions to do much better. In the scintillation techniques, there are hints of systematic limitations and thresholds but here too there are possible new developments with low temperature liquids. Other possibilities seem much further off. The competition means that delays should be avoided but the CDMS program has many advantages. Second, there are experiments which look for different types of dark matter such as neutrinos or axions or MACHO's. CDMS is complementary to these experiments.

The investment was considered worth the incremental costs presented and SAGENAP recommended that the agencies should incorporate funding for this project in their plans after a more comprehensive understanding of its funding was established within the agencies.

Recommendations

A detailed technical and cost review should be undertaken when the techniques are established and enough experience is gained with the present detector. This should cover all aspects of the costs of the experiment.

AXIONS

Science

This experiment is searching for axions. One of the outstanding questions in particle physics is why the strong interaction appears to conserve CP (charge-conjugation-parity). One way to explain this is to invoke a protective symmetry - the Peccei-Quinn symmetry

which is spontaneously broken. The Goldstone boson associated with this symmetry breaking is called the axion. This process is analogous to the mechanism leading to the Higgs particle. Currently, axions are also of interest as one of the prime candidates for non-baryonic dark matter. Axions with a mass in the region of 10^{-6} - 10^{-3} eV could have effected structure formation by seeding the formation of galaxies and could represent as much as 90% of the mass of the Universe. While the couplings of the axion to hadronic matter are well-defined (due to the connection to the strong CP problem), the coupling to the leptonic sector is not. Axions with no tree level coupling to leptons are termed KSVZ axions and axions with the same tree level couplings to quarks and leptons are termed DFSZ axions (which arise in simple GUT scenarios). The search for such particles is therefore motivated both by their importance to particle physics and by their potential importance to astrophysics.

The techniques and early experiments have been developed over the past decade. This experiment is the first with a sensitivity capable of reaching the limits of cosmological predictions over an axion mass range of 10^{-6} to 10^{-5} eV. A progress report was made to SAGENAP.

Method/Experimental Issues

A scan is made through a continuum of resonant frequencies of a low noise RF cavity sitting in a high magnetic field in order to detect the production of an axion of microvolt mass via the Primakoff effect (here the axion couples to two photons, one real and one virtual). The signal power which needs to be detected is of the level of 10^{-22} W.

Cost/Schedule

The present run will complete the search for the KSVZ axions by the end of 1998 using an upgraded detector which will increase the frequency scan rate in order to cover the signal range more rapidly. The experimenters then propose to improve the sensitivity of their apparatus by an order of magnitude to have sensitivity to the DFSZ axion.

The level of support was described as reasonable.

Comments from SAGENAP

This prototype experiment was generally felt to be excellent and to involve a strong team of experimenters. They have demonstrated that the apparatus is capable of operating with good signal to noise and have established credible criteria for the identification of a positive signal, and can vary the magnetic field to confirm the presence of any positive effect. SAGENAP strongly endorsed continuing this experiment which they felt was progressing well.

Recommendations

This effort should be supported to complete its goal. If an axion signal were detected, the axion could be the answer to the dark matter question.

ICARUS

Science

The goal is to build a large (5000 ton) detector deep underground to search for new phenomena. In particular, the detector would study proton decay and work on clarifying the atmospheric neutrino anomaly. The detector would also be available as a remote neutrino detector for a possible future CERN neutrino beam. The detector would be the analog of an electronic bubble chamber. An imaging detector could, in principle, give the ability to do detailed studies of well identified rare processes. The proposal presented was to participate in building a 600 ton module; the primary motivation presented for this was the study of ^{8}B solar neutrino interactions in the argon.

Method/Experimental Issues

ICARUS is a project to construct large liquid argon time projection chambers (TPC).The technique employed is to use very pure liquid argon as a medium and to drift the ionized deposits over large distances and to image them for reconstruction. This requires very good spatial resolution, high purity liquid argon, low background levels, a high voltage system capable of drifting long distances, and a readout system to record and reconstruct tracks. A small (~5 ton) prototype has been built at CERN and has demonstrated, at that scale, the ability to image for a variety of tracks and interactions. The present proposal is for a U.S. contribution to the construction of a 600 ton prototype with wires spaced close enough so that solar neutrinos (>5 MeV) can be detected. This device is to be installed in the Gran Sasso in 1998. Additional modules adding to 5000 tons are projected for the future.

Cost/Schedule

The schedule seems highly optimistic. ICARUS is funded by Italy. The U.S. role in this project is small and consists of the UCLA group with proposed responsibility for the high voltage system. This is a challenging engineering-intensive effort requiring handling 80kV operating voltage with low noise, operating at low temperatures, with non-trivial high voltage feed-through issues and long term reliability issues. The proposed capital equipment costs of this system is $277K through the year 2001.

Comments from SAGENAP

After many years of R&D leading to the prototype, the technology for a liquid argon TPC appears ripe. The prospect of an "electronic bubble chamber" with excellent resolution and tracking is appealing. It is a technological tour de force. However, the schedule seems highly optimistic. The detailed technical requirements and engineering ability of the UCLA group are difficult to assess, but this particular engineering-intensive job does not appear to be a natural match and contribution for such a group. The project is very long-term, and the UCLA contribution does not seem to lead to a major involvement in the future results, being limited to purchase of a high-voltage system for the experiment. The Italian collaboration is strong and is likely to bring the detector construction to a successful conclusion with or without these funds.

Moreover, compared to two other present generation experiments to study the ^{8}B solar neutrinos, the counting rate in ICARUS is modest. In the elastic $\nu + e$ channel, which is sensitive to both electron neutrinos and to the μ and τ neutrinos into which the electron neutrinos in the solar source may transform, the counting rate is 230 events/year. In the "absorption" channel, which is sensitive only to electron neutrinos, the counting rate is 1440 events/year. For comparison, the SuperKamiokande project, which has been running for almost a year, counts almost 8000 elastic scattering events/year.

The SNO project, which should be operating in one year, will count about 350 elastic $\nu_e + e$ events/ year, 3500 disappearance events/year, and 2800 neutral current events/year. Given these two experiments, both of which have strong U.S. participation, studying the 8B solar neutrinos, the scientific motivation for a third experiment with a smaller counting rate is not compelling. It was recognized that the ICARUS detector can, in principle, produce data of a very different nature from the water Cerenkov detectors. Nonetheless, the argument for participation in the construction of the detector was not felt to be strong. It is certain that a successful 5000 ton detector will have tracking capabilities far superior to any detector previously constructed for studying proton decay. This will permit excellent kinematic reconstruction and background rejection. However, the limited reach of the 5000 ton fiducial volume and the late start relative to the 50,000 tons of SuperKamiokande, makes it problematical that there will actually be much new territory to explore.

Recommendations

Participation of a small U.S group in this project seems reasonable if the UCLA group wishes to devote a portion of its operating funds to participation.

Funding of the HV feed-through construction is not recommended principally because there is too small a scientific payoff for U.S. participation.

Note added in proof
This proposal was withdrawn by the proponents shortly after the SAGENAP meting in March.

GLAST

Science

The highly successful space based EGRET instrument surveyed the sky for high energy gamma ray sources and found many examples. It also measured the energy dependence of these sources up to energies of several GeV. The ground based Whipple Observatory has looked for sources at much higher energies - above a TeV- but has not seen the majority of the sources observed by EGRET. The sources of these gamma rays include active galactic nuclei, supernova remnants, certain pulsars, and diffuse emission from galactic and extragalactic sources.

The satellite-based GLAST project will study gamma rays from 100 MeV to 300 GeV in energy and close the gap in the range of observed energies between space and ground based observatories. Establishing the highest emission energies of these sources and their energy spectrum will play an important role in determining emission mechanisms. The experiment should also lead to an improved understanding of cosmic-ray propagation and the interstellar medium.

A detailed presentation for GLAST R&D was made at the previous SAGENAP meeting. The actual proposal for GLAST itself is expected to be presented at the April 1998, meeting of SAGENAP.

Method/Experimental Issues

The GLAST collaboration is continuing to develop the next-generation satellite-based high-energy gamma-ray telescope. Compared to EGRET, GLAST will have almost two orders of magnitude better flux sensitivity, sensitivity to higher energy gamma rays, and improved energy and angular resolution. The design goals are for a solid angle acceptance of 0.82 sr (vs. 0.15 sr for EGRET), energy resolution at 1 GeV of 4.4% (vs. 9% for EGRET), and pointing resolution of 0.420 degrees (vs. 1.50 for EGRET). The GLAST instrument uses no consumables and could have a very long lifetime of scientific productivity. SAGENAP heard reports on a very active program of R&D and design effort, involving work on front end electronics, SSD's, CsI Calorimeter, Scintillator veto system, triggering simulation studies, and analysis and tracking codes.

At the previous meeting, the focus was on activities of DOE-supported groups and SLAC. At this meeting SAGENAP focussed on the efforts of traditionally NSF supported university groups in GLAST. Significant technical progress has been made in several areas. One area is in the development of the support structure for the Silicon microstrip

tracker. Considerable progress has also been made towards converging on a design for the front end electronics for the silicon strips. Finally, imaging calorimetry is being investigated as an alternative to the baseline "tower" configuration of CsI. If successful, it is expected to provide a significant increase in their overall acceptance and diagnostic power at no great cost in energy resolution. This calorimetry work appears to be based largely at Chicago with some input from Columbia.

Cost/Schedule

The plan presented was to build tracker prototype for a test beam run next Spring; to build one full prototype of a "tower" for a trial flight of both by 1999, with the goal to submit a GLAST flight proposal by the end of 1999. However, assuming the R&D is successful, it is still unclear how the construction will be supported.

Comments from SAGENAP

The R&D effort seems to be progressing well. It was felt that the university groups involved are strong and capable of making a sizable contribution to the simulation effort and to developing the imaging calorimetry system. These are important parts of the GLAST project and the participation of university groups in the project was supported.

No mention was made of the balloon flight proposed a year ago. It was not clear if the balloon flight has been eliminated, or was simply in the background in the relatively abbreviated presentation.

The GLAST project has been endorsed by a NASA national advisory panel, has strong community support, and has strong scientific justifications. While the SAGENAP meeting was taking place there was a technical review of GLAST by NASA and the project received a strong endorsement. SAGENAP supports the NASA assessment and agrees that GLAST should be given high priority at NASA, and that it should be flown within the next decade.

However, SAGENAP felt that the issue of agencies other than NASA supporting construction was complex. SAGENAP agreed that the experiment represented good science and was proposed by capable scientists. The scientific collaboration was described as very strong, involving accelerator groups with experience in calorimetry as well as groups with experience in space physics. The current R&D effort is funded by NASA, DOE (through SLAC) and NSF. The techniques GLAST will use are those developed for use in high energy physics, modified only to the extent that they become space-qualified. The interest in the high-energy physics community in this physics is clear but SAGENAP was concerned about the implications and possible consequences if major construction funding were to come from HEP programs.

SAGENAP noted that the construction costs would be significant when compared to the current budgets for non-accelerator experiments. One suggestion was that "new money" be found to support this activity. Another option raised was to require GLAST funding to represent a redirection of DOE/SLAC funding (SLAC has supported a significant amount of the R&D on GLAST).

SAGENAP raised several questions for discussion. What advantages would there be in NSF and DOE supporting a space venture that in the past would have been wholly supported by NASA? How desirable is interagency co-operation? What are the prospects for some reciprocal funding from NASA for ground-based or other experiments? What is the need to accelerate the construction schedule for GLAST? Would NSF and DOE support have a significant effect on the schedule? How does the priority for GLAST fit within that of the HEP program in general?

Recommendations

The R&D work must continue and should be supported at a modest level by the high-energy physics programs. A joint discussion on the issue of funding of GLAST itself is ultimately needed among the DOE, NASA and NSF.

Ground Based Gamma Ray Experiments

Science

SAGENAP heard three proposals which had been submitted to NSF concerning ground based gamma ray astronomy in the 10-100 GeV range. The STACEE and CELESTE projects utilize existing solar furnaces with their large light collecting areas as gamma ray telescopes with thresholds of a few tens of GeV and with sensitivity up to a few hundred GeV. The Stereoscopic Air-Cerenkov Detector project covers the same energy interval, but proposes to establish a new laboratory at a "green fields" site. These projects fill in the gap in energy observations between the satellite experiments (EGRET and the future GLAST) and the ground based experiments (Whipple Cerenkov telescope and the MILAGRO air shower detector). This is one of the few gaps in the known astronomical electromagnetic spectrum. There are tantalizing differences in what is observed below and above the gap. There are over 150 gamma ray sources with energies above 100 MeV observed with the EGRET detector, but only a handful of sources observed by the Whipple observatory with energies above 300 GeV.

Gamma ray astronomy has made great strides in the last several years with the new results from the Compton Gamma Ray Observatory (GRO) and the perfection of the imaging technique for background reduction at the Whipple telescope. The major scientific issues to be investigated are: turnover in the AGN spectra, turnover in pulsar spectra, serendipitous sources, detection of supernova remnants, unidentified EGRET sources and gamma-ray bursts.

The GRO has discovered a large number of gamma ray sources in the energy range from 30 MeV to 30 GeV. Gamma ray bursters have been cataloged by the BATSE experiment and about 130 objects have been observed by the EGRET experiment. About one half of the objects seen by EGRET have been identified as pulsars and AGN's. The AGN's have red shifts ranging from 0.03 to 2.29 while their gamma ray energy spectra seem to follow a power law over the entire observable range. Unfortunately, it is not currently possible to follow the emission of these sources between 30 GeV and 300 GeV - the energy at which the sensitivity of ground based gamma ray detectors currently begins. In this higher energy range the Whipple instrument has in fact been successful in observing only one of the EGRET pulsars or supernova remnants (the Crab Nebula) and only one of the AGN's (Mrk 421). Mrk 421 is the closest and one of the weakest sources seen by EGRET. The Whipple telescope has also seen one AGN that was not observed by EGRET. This object, Mrk 501, is at about the same distance as Mrk 421. This observation suggests that the gamma rays are attenuated by interactions on the intervening background infra-red radiation left over from early epochs of galaxy formation and that only the very closest sources can be seen above 300 GeV. Recently there have been suggestions that the infra-red background may be less intense than this interpretation suggests, in which case the absence of some specific sources in the Whipple data may reflect a cutoff of the spectrum at the source. In either case, it will be of great interest to measure the spectrum between EGRET and Whipple energies. The details of the gamma ray absorption curves can be interpreted to give a measure of the intergalactic infrared photon density .The gamma-ray absorption should decrease with decreasing energy; a study of the AGN energy spectra as a function of distance could yield important information on the intergalactic IR field, which depends, in turn, on various aspects of cosmological evolution. Thus this is an important, fundamental measurement.

There is a similar interest in filling in the gap in gamma-ray spectra from the EGRET supernova remnants, IC443 and Gamma-Cygni. Since shock acceleration at SNR is thought to be the main source of galactic cosmic rays, and since the spectrum of accelerated particles should extend up to at least 100 TeV, it is somewhat surprising that Whipple is not seeing gamma-rays of, say, 300 GeV at the expected level. Perhaps the cosmic-ray upper limit is lower for these particular SNR or the source of the gamma-radiation could be different. Hence there will definitely be some astronomy to be done. However, since these gamma ray telescopes must point at the source they are investigating, the number of targets with sufficient flux to be interesting is limited.

So, these experiments will address questions about gamma ray generation mechanisms, the nature of the sources and interactions with the intervening medium. Aside from the testing of models there is a discovery potential for new effects and perhaps new sources. It seems worthwhile therefore to build an instrument that would be sensitive in the energy range immediately above that of the GRO and reaching out to the 300 GeV threshold of the Whipple-like instruments.

STACEE

Method/Experimental Issues

The STACEE group intends to make use of the large existing mirror area at the solar heliostat facility at the National Solar Thermal Test Facility run by Sandia in Albuquerque. This proposal appears to have a good chance to fill in the energy gap in observations of high energy gamma ray sources in a short time scale.The large light collection area (increased from 80 m^2 at Whipple to 1,800 m^2) afforded by the 48 heliostats allows the detector to take data at a low energy threshold (maybe as low as 25GeV?). The proponents stated that the DoE facility at Sandia will be made available at no added cost and can be used at night.

Cost/Schedule

The project appears to be technically ready for funding. Management should not be an issue for a project of this scope. The proposal is for a two year program of construction of secondary mirrors, PMT tube arrays and electronics followed by observations in the third year.

The STACEE costs listed were for the Chicago group only, were for a total of $272K and to cover mirrors, mechanical structures, analog & digital electronics and data acquisition which was claimed to be the major cost of the experiment. Other material and equipment was expected to come from the McGill, UCSC and UCR collaborators and the amount was not stated (but is epected to be less than the above total) The hardware responsibilities of the universities are; UC Santa Cruz - Trigger; UC Riverside - Analog Trigger; Cal State LA and CALTECH - Atmospheric monitoring system; McGill University (Canadian funding) - Cameras, PMT cans, Winston cones, PMT's and bases, High Voltage and cables. There was some discussion in the Q&A period as to whether their choice of a 8-bit digitizer will be adequate since it was by far the least expensive one investigated. It could add significantly to the cost if it proved an inadequate choice. However, the experiment was felt to be a "bargain". In addition support would be needed for participating groups. The Chicago group is asking for about 1M$ over 3 years. While it is not possible to say exactly what the impact of a negative funding decision in Canada would be on the funding request existing information on the Canadian situation makes it appear that they are planning to make a major contribution (and have done so already)

Comments from SAGENAP

The site itself appears excellent for purposes of this experiment. The STACEE team seems to have done a good job in characterizing their detector. They have developed the idea to use solar collectors as gamma ray observatories over the past few years to the point where the feasibility is well established. A complete prototype has been installed and run at the site using eight of the heliostats. The measurements and simulations they have done

establish that the technique will not be limited at the lower threshold due to night sky noise. Other atmospheric effects are not a problem, and the proton-gamma ray separation appears to be adequate.

CELESTE

Method/Experimental Issues

CELESTE is a French project which utilizes a solar furnace - the Themis site in the eastern Pyrenees. The Utah group proposed to join the ongoing effort to build a gamma ray detector using an existing solar heliostat array.

Cost/Schedule

The first phase of this construction has been completed with the operation of six heliostats with a temporary outfitting of secondary mirrors and electronics. The second phase of the construction will employ forty heliostats by the spring of 1998 with the final design of the secondary mirrors and VME electronics. The Utah group wanted to supply the FADCs, memory, HV and an innovative design for analog delay lines jointly being developed by LeCroy and Utah.

Comments from SAGENAP

CELESTE is being built in the immediate vicinity of existing gamma ray detectors. The CAT imaging telescope (0.2 to 2 TeV), the Themistocle array (3 TeV) and the ASGAT array (600 GeV). This is an advantage for CELESTE. It will allow cross calibration of individual showers with the CAT telescope and in principal compare shower identification and direction with a device that uses a well- developed technique. In addition, the infrastructure that has been set up by the other experiments can be used to good advantage. As an example, a LIDAR system is already on the site. The Themis site is no longer being used for solar energy research and the IN2P3 has given full control to the CELESTE collaboration. Some panel members thought that it was an advantage since tuning and development could proceed at the site during the daytime possibly leading to a much quicker implementation of the detector. It was noted though, that the project would proceed independently of the proposed small U.S. involvement

STEREOSCOPIC AIR CERENKOV DETECTORS

Method/Experimental Issues

This presentation concerned the building of a new multiple dish array at high altitudes (3900m) to be placed at White Mountain in California. The present proposal consists of two 10m dishes (each equipped with a 128 PMT camera) on a 150 meter baseline giving a `stereo' vision with imaging cameras. This separation gives an angular resolution of 0.2

degrees. In contrast to the Solar Heliostat arrays which use a very large mirror area to achieve a lower threshold, this group proposes to get to a lower threshold by a combination of somewhat larger collection area (eventually seven to nine mirrors instead of the current one or two at Whipple), a higher altitude and a narrow timing window. Since the Cerenkov signal is inherently fast, a fast data acquisition and digitization system should produce a smaller background. They also discussed the experiment's potential for expansion to seven dishes. The site selected for the array is the White Mountain Research Station in the southern California high desert which is under operational control by the University of California. Documentation was presented from UC Administration confirming their willingness for this use.

Cost/Schedule

A quite modest amount of R&D has been done on some aspects of the project. However it would seem to come on later than either STACEE or CELESTE and the attractive features come at quite a high price. The cost per fully equipped dish was presented as $990K for a total of about $2.8M for the initial two dish array. They have an application to the NSF MRI solicitation for $1.8M with an additional ~$0.8M to come from the academic and industry participants. The budget was felt to be unrealistic and did not include the actual cost of running a facility at high altitude.

Comments from SAGENAP

The present proposal is for the construction of a stereoscopic pair of telescopes. There are many attractive features to this proposal such as the potential to cover a broader energy window, a larger angular acceptance, and a somewhat larger gamma area than either STACEE or CELESTE. The stereo is indeed a very useful handle on backgrounds at the low energy as is the high altitude (3.9 km) and the use of an isochronous dish-shape design and high speed electronics.

This project seeks to build on the success of ACT imaging systems and carry them one step further by going to higher altitude and using faster electronics. These innovations would certainly improve on the sensitivity of existing telescopes such as Whipple but the factor of improvement is debatable. While faster electronics are needed in future ACT experiments no special efforts are needed to achieve this. The advantages in going to higher altitudes are offset by the practical disadvantages of working at this altitude. It was felt that the difficulty of constructing a 10m dish which would survive operating at high altitude was severely underestimated. The group was generally judged to be too inexperienced in this field. For example, the use of stereo telescopes at Whipple achieved only limited success and demonstrated that a separation of 140 m is too large; nevertheless, this stereo proposal is for two telescopes, similar in size to the Whipple telescopes, separated by 150m.

It was unclear why the effective area available for photon detection was so large (22,000 m^2). The claim that very fast timing will reduce backgrounds to the needed level, and the claim of a 10 GeV threshold needed to be substantiated. In particular, the threshold needs to be discussed in light of large expected backgrounds from cosmic electrons. The narrow timing feature is untested and the group proposes to demonstrate the efficiency of this technique with a 2.4 meter telescope mounted at Mount Hopkins.

The ultimate goal of the proposal was to build as many as seven telescopes. In this greater context, this proposal bears a striking similarity to the VERITAS proposal submitted to the Simthsonian Institution from an extended Whipple Observatory collaboration. The principal advantage of the present proposal over the VERITAS proposal is a slightly lower threshold achieved by siting the laboratory at a higher altitude.

It was generally felt that the proposal was not fully developed and was not ready for serious consideration for funding. It was considered to be expensive and not competitive with either STACEE or CELESTE.

Comparisons

SAGENAP looked at these experiments in the context of the current and future program in this field. First, given that GLAST is expected to eventually (Year 2000+?) cover this region by moving up in the energy scale beyond EGRET and other, larger ground based techniques will be moving downward from 200 GeV, they considered if any experiment should be done in the interim. How do these proposals compare to VERITAS and GLAST?. The answer is that they are not really in competition since the emphasis on GLAST will be on the lower range of energies and VERITAS on the upper range. The unanimous decision was that these experiments represented a real target of opportunity which could yield useful information in less than three years --- information which might very well influence the direction of the developing and upcoming larger, more comprehensive experiments --- and in some cases at quite modest cost. However, since both these experiments will ultimately overlap and have greater sensitivity in their chosen ranges it is important for STACEE and CELESTE to be completed as soon as possible. In that way they will be complementary to the other larger projects which might not be operational until 2004.

The STACEE and CELESTE proposals have many points of direct comparisons and to high order would appear to have quite similar capabilities in the near term. In attempting to go to the lower energy end of the "open window" it is essential to increase significantly the light collecting area; both STACEE and CELESTE do this by making use of heliostats originally constructed for solar power research. STACEE would use 1800 sq-meters of the device at the Sandia Lab in New Mexico while CELESTE would use 2200 sq-m (with a long term goal of 3 times that at some unspecified future date) of the Themis installation in France. Both are at altitudes of 1500 meters. Both have similar fields of view of 1-degree and similar angular and energy resolution. CELESTE aims for a threshold of 20

GeV while STACEE is at 40 GeV and above. The lower energy threshold for CELESTE depends on lowering the much higher night-sky-background at that energy by requiring a fast coincidence using a variable analog delay technique with the electronics to be provided by the U.S. group from Utah. Both projects have completed some R&D and done to demonstrate feasibility and both could achieve the main part of their goals. It is difficult to say that there might be any important difference in the time line to data for the two experiments. In each case the research teams are strong and very capable. There are no major technical difficulties anticipated but there may be unsuspected sources of background which may limit the flux sensitivity.

However, there are some contrasts between the two experiments: Sandia's device is still used for solar research in the daytime while the Themis site is given over entirely to astrophysics research. On the other hand there are guarantees from the DoE lab management that excellent access will be made available while the Themis site has a variety of other experiments there all under French control with no official U.S. leverage for participants. The number of cloud-clear days is greater at Sandia but on the otherhand Themis is in the Pyrennes away from any city. The CELESTE detector seems to have the same sensitivity as STACEE and is on the same site as other gamma ray detectors that can be used for cross calibration. This may be very useful, especially in this case where the technique to separate signal from background has never been tried before. STACEE argues that Whipple and Milagro are sufficiently proximate to provide corroboration.

The table below summarises the similarities and differences.

	STACEE	CELESTE
Composition	U.S.-Canadian	French-Czech-U.S.
Location	U.S.	France
Status of facility	Active: nighttime	Closed: full use
Nature of site	Energy research	Gamma-ray observatory
Site	Not ideal: city	Not ideal: weather
Threshold	40 GeV	20 GeV
Flux sensitivity	Same	Same

The question is, in view of the presently funded Milagro experiment (which may extend down to 100 GeV) and in view of GLAST which should produce results before the end of the next decade, how much support should go into these experiments? The majority of SAGENAP felt that only one experiment should be supported. Having two detectors do this science however was not ruled out given the importance and difficulty of this science. The consensus was that the U.S. should support the simplest and quickest experiment.

The STACEE project has an impressive record of technical achievement and have already presented many technical results to the community (at conferences and in papers). Their proposed program is well thought out and feasible. As with CELESTE the ultimate test of

this new technique will come with the detection of the Crab. It is a small group but this is their major activity. Recent reviews of the proposal from the Canadian side of the collaboration make it apparent that they are planning to make a major contribution.

The CELESTE project is the more ambitious of the two and has the advantage that it is a permanent facility, is located close to other gamma-ray telescopes and will be largely funded by the French, assuming that the initial tests and demonstration of the detection of the Crab is successful. Their progress to date is not as well documented as STACEE. U.S. involvement to date seemed small and the influence of U.S. investigators on the design was unclear. It was evident that their main contribution would be the electronics expertise of the Utah group for the fast ADC technology. SAGENAP did not agree with the reasons given in the presentation for getting involved with CELESTE rather than STACEE.

Why does the majority of SAGENAP prefer the STACEE proposal over the CELESTE one ? They felt that STACEE can do the job as well as CELESTE and expected the STACEE experiment to get off the ground more quickly and more coherently. They commented that the personnel who have been intimately connected with the R&D tests will also do the experiment and that control of the experiment rests with them from start to finish. They described STACEE as a well-focused effort to attack this problem with an enthusiastic and very competent group of researchers. There was some concern about access at all times to the solar facility; this would need to be firmed up with Sandia. The STACEE group would accept collaboration by the U.S. participants in the French CEL ESTE experiment, which is likely to go forward in any case. In contrast, the CELESTE proposers are a handful of U.S. physicists in a very large existing group of French scientists who have already pushed the experimental tests to the present level. The U.S. contingent will be providing some electronics for the experiment; but did not appear to be a major intellectual weight to the project as a whole. The STACEE project seems to have a clear beginning and end; CELESTE has the possibility to go eventually to 160 heliostats beyond the projected 48 which implies more expenses down the road.

While SAGENAP said they had been influenced by all these issues a key factor for many of them was the fact that the viewing time at Sandia will be greater. It was felt that access to the STACEE equipment on a "need" basis will be easily achieved. Also the tests shown by both groups did not appear to indicate that the night-sky-background at Sandia will put STACEE at a significant disadvantage. To most of SAGENAP, the U. S.-led STACEE project made a better case for U.S. funding than CELESTE; CELESTE may be ready sooner but the Sandia site has better weather. The supporters assumed that STACEE would focus on a short-term program of observations; should a longer-term program emerge, it was felt the decision should be revisited.

One member of SAGENAP disagreed with statements recommending that the U.S. limit its investment in experiments in other countries in those cases where we are not a majority interest. He argued that the intellectual contribution to an experiment is not correlated to a group's monetary contribution. In these times of limited resources, he felt that the physics

output should be maximized and that the U.S. community should not insist that every experiment be implemented on U.S. soil. He believed the presence on the site of other gamma ray experiments to be a very important advantage for this collaboration over that of STACEE. He recommended funding the participation of the Utah group in CELESTE with high priority with less priority given to the establishment of a separate and redundant STACEE effort in the United States.

In summary, while the science that all of these projects would seek to do is the same and is good, STACEE seems to have the best team, has made the most progress to date, and has the best development and operating plan.

Recommendations

Only one experiment should be supported.

The majority of SAGENAP supports the NSF funding of STACEE.

Funding of the CELESTE project is not recommended by the majority of SAGENAP.

Funding of the Stereoscopic Air Cerenkov experiment is not recommended.

The STACEE experiment is supported with the proviso that this is focused on a short-term program of observations; should a longer-term program emerge, the decision on further support would need to be re-evaluated by SAGENAP.

The BU and Utah groups should consider joining forces with the Chicago group since it would be to everyone's advantage to complete STACEE as soon as possible.

It is premature to make a very large investment in a new facility. This new facility should only be considered if the task cannot be done with Whipple and/or STACEE and the science demands a new optimized facility at higher altitude. Given the infrastructure at the existing Mount Hopkins site and the large expense associated with operating a new observatory location it is hard to argue for a new site. While this would be giving up the possibility of getting to very low thresholds these observations will eventually be made with GLAST.

The narrow timing window technique should be tried. If it works the technique could be incorporated into VERITAS.

PION EYE at SUNSPOT

Science

The question of the composition of primary cosmic rays is an interesting one, especially in the region of the knee where changes in the composition may lead to an explanation for the change in the slope of the spectrum. However, definitive composition studies at these energies have proven difficult.

Method/Experimental Issues

A proposal was presented to convert an existing facility at Apache Point, Sunset Mountain Laboratory in New Mexico. A large array would be built on the surrounding terrain to investigate air showers and do cosmic ray physics at energies above the knee and to identify primary particles on an event by event basis. The proponents argue that by correlating muon measurements, accurate timing and extensive air shower information they can determine primary composition on an event by event basis.

Cost/Schedule

There was no estimate provided of the sensitivity or cost of the experiment. The presentation to SAGENAP was too preliminary in nature to allow any consideration for funding. It was felt however, that the potential results did not appear to be sufficiently promising to warrant support.

Comments from SAGENAP

Although this is intriguing, little evidence or simulations were presented to convince SAGENAP. The sampling seemed very sparse and the various potential resolutions were not made clear. This proposal was felt likely to be inferior to the BLANCA adaptation of CASA currently in progress.

Some members of SAGENAP felt that the existence of the site at Sunspot may represent a real asset. However, others argued that while the facility is interesting it is not unique and has a limited area for development as a cosmic ray laboratory. It was generally felt that it should be the responsibility of the university (New Mexico State) to underwrite this task and that of the funding agency to fund the experiments. It was not clear that the university had accepted this responsibility.

Recommendations

If the proponents are to make a case for funding this experiment a detailed proposal is needed. Funding of the facility is not recommended.

APPENDICES

APPENDIX A – CHARGE TO THE SAGENAP PANEL

In the context of the National High Energy Physics Program, the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) is charged to review, assess and evaluate activities related to non-accelerator experiments. The study will include not only new activities but also relevant on-going activities and should look ahead to activities that are in the preliminary stages of R&D or prototyping. SAGENAP is requested to provide information on what activities should be considered an essential part of a high quality program in this field and to comment on what level of funding it considers necessary for such a program

In addition, for each activity please supply recommendations on the following
What level of equipment support is appropriate for this experiment? This issue is both general and specific and should include the overall level as well as levels for specific experiments. What extraordinary levels of operating funds are required for recommended activities? This is to alert the agencies as to the special operating needs such as R&D, power, facility operations, maintenance, additional manpower, etc.., which may go beyond the normal operating budgets of participating institutions. These numbers should be integrated into the overall levels of support recommended. What conditions are attached to your recommendations for the support of experiments, such as progress milestones, assumed support from other agencies, unsettled makeup of collaborations pending international agreements, etc.? How does this activity compare in relative importance to other activities reviewed? How does this activity compare in cost/significance to other activities reviewed?

In addition, for the review of the AUGER proposal the charge was:
In the context of the National High Energy Physics Program, the Scientific Assessment Group for Experiments in Non-Accelerator Physics is charged to review, assess and evaluate activities related to Non-Accelerator experiments.

For the Auger experiment in particular, please supply recommendations on the following
The importance to the field of the scientific questions to be addressed

> What are they?
> Where will they lead?

A recommendation on the best approach to take to answering these questions

> Should we support building Auger as proposed/modified?
> On what timescale should action be taken?

An evaluation of the relative merits of this activity compared to others in the program

> Are there activities we should be prepared to cancel if necessary to fund Auger?

APPENDIX B - MEMBERSHIP OF SAGENAP

Scientific Assesment Group for Experiment in Non-Accelerator Physics Members:

Barry Barish	Caltech
Steve Barwick	UC-Irvine
Gene Beier	Pennsylvania
Tom Gaisser	Bartol
Cy Hoffman	LANL
Bob Lanou	Brown
Adrian Melissinos	Rochester
Hank Sobel	UC-Irvine
Trevor Weekes	HSAO
Patricia Rankin	NSF (Chair)
P.K. Williams	DOE (Co-chair)
Vernon Jones	NASA

APPENDIX C - AGENDA FOR THE MARCH 5-6, 1997 MEETING

8:30-9:00am	Executive Session
9:00-10.45am	Auger
10:45-11.00am	Break
11:00-11:15am	Discussion on Auger
11:15-12:00pm	Km^3
12:00-1:00pm	LUNCH
1:00-2:45pm	Amanda
2:45-3:00pm	Break
3:00-3:15pm	Discussions on Amanda
3:15-4:30pm	CDMS II
4:30-5:00pm	Axions
5:00-6:00pm	Executive Session

8:30-9:40am	Executive Session
9:40-10.15am	Icarus
10:15-10:50am	GLAST
10:50-11.00am	Break
11:00-11:35am	STACEE
11:35-12:10pm	CELESTE
12:10-1:15pm	LUNCH
1:15-1:50pm	Stereoscopic air Cerenkov detectors
1:50-2:25pm	Pion eye at Sunspot
2:25-5:00pm	Executive Session

AGENDA FOR THE NOVEMBER 3rd - 4th, 1997 MEETING

1:00-2:00pm	Executive Session
2:00-3.30pm	Auger
3:30-4:30pm	Hires
4:30-5:00pm	Break
5:00-6:00pm	JTA
6:00-6:40pm	OWL
6:40-7:00pm	Other possibilities
7:00pm...	Executive session
Approx 8:30pm	Reconvene for presentation of new question list.....
9:30am	Reconvene - question responses and discussion
12:00-5:00pm	Executive Session (after 1pm experimental reps should be available in case of questions and for closeout discussions as needed)