REVIEW SUMMARY

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

APRIL 14-16, 1998

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INTRODUCTION

The Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) assembled for a meeting on April 14-16, 1998 under the auspices of the Department of Energy's Division High Energy Physics and the National Science Foundation's Division of Elementary Particle Physics. The group provides advice on proposals submitted to NSF and/or DOE through individual letters of the members submitted after each meeting. This report is a summary of the written reviews, and the conclusions that follow from them. The final decisions are of course in the hands of the agencies.

This report covers six experiments: The Pierre Auger Project, The GLAST (Gamma Ray Large Area Silicon Telescope) Project, The Super-Kamiokande Maintenance and Upgrade Project, an ICARUS High Voltage Feedthroughs request, a VERITAS Heads-up, and a SOUDAN II Continuing Operations request.

The meeting was co-ordinated by P.K. Williams (DOE) and Patricia Rankin (NSF). In addition, there was agency representation from NASA. The membership of the SAGENAP is given in Appendix A. The meeting agenda is in Appendix B.

A. <u>The Pierre Auger Project</u>

The goal of the Auger experiment is to understand the origin of the highest energy particles in nature. If these particles come from a uniform, cosmological distribution of sources, then the spectrum is expected to show a cutoff below 100 EeV (10**20 eV), known as the Greisen-Zatsepin-Kuz'min (GZK) cutoff. The observations are ambiguous at present because of limited exposure. On the one hand, there are a few events above the nominal cutoff energy, but on the other there are not as many as would be expected from a power-law extrapolation of the spectrum from lower energy. Because of the extremely low flux, there are not enough events to determine, for example, whether there is a cutoff with a few events from nearby sources above the cutoff or whether the spectrum simply continues to much higher energy. The situation is even more interesting, because acceleration mechanisms associated with several potential energetic astrophysical sources are expected to have maximum energies in the same energy region, namely, 100 EeV. The Auger experiment proposes to address the problem by building hybrid detectors in both southern and northern hemispheres with sufficient exposure (area X solid angle X time) to determine whether there is a cutoff and whether specific point sources of the highest energy particles can be identified. These are indeed extremely important scientific goals.

The Hi-Res Fly's Eye detector now beginning operation in Utah also addresses this science. In addition to its order of magnitude larger exposure above 10 EeV, the Auger Project will be complementary to Hi-Res in two important ways:

1) It is a hybrid experiment with a ground array and an optical detector. Approximately 10% of events will be observed by both components, giving a large subset (comparable to or somewhat larger than the Hi-Res experiment) of especially well-characterized events that can be used for calibration of the ground-array as a stand-alone detector. Especially important is the understanding of fluctuations that can be made with hybrid events. Not only will studies with the hybrid data allow better understanding of the errors in energy assignment with either technique alone, but it will also allow a better study of composition of the primary particles by providing additional observables that are sensitive to the mass of the primary particle.

2) A large detector in the southern hemisphere will cover a part of the sky not accessible to Hi-Res, including the galactic center as well as concentrations of cosmologically nearby luminous matter.

The science priorities are as follows (this order differs from that of the Auger collaboration):

(1) Are there or are there not sources of cosmic rays above the Greisen cutoff?

(2) If there are cosmic rays above the cutoff, is there gross anisotropy: are they from point sources (repeaters), concentrated in the galactic or supergalactic plane, or isotropic?

(3) If there is a cutoff, are the highest energy cosmic rays (say, energy greater than 10^18 eV) from point sources, concentrated in the galactic or supergalactic plane, or isotropic?

(4) Regardless of the question of the cutoff, what is the energy spectrum of the highest energy cosmic rays?

(5) What is the composition of the highest energy cosmic rays?

(6) Produce a sky map of the highest energy cosmic rays.

The magnetic rigidity might help uncover the source of high-energy cosmic rays if there are a handful of discrete sources and if the cosmic rays are all protons. However, if the intergalactic magnetic fields are more coherent than assumed (the Auger collaboration assumes a coherence length of 1 Mpc), or if the cosmic-ray composition is mixed, it will be very difficult to point the cosmic rays back to their source(s).

Furthermore, the 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. We are not optimistic that a startling discovery is around the corner in this field, having faith in nature to have a few tricks up her sleeve.

Ground-based detection of cosmic-rays has a long and, at best, a mixed history. 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 or observations coincident with a large ground array remove this problem.

Use of a ground array alone, such as Auger, leads to less robust measurements of energy and composition and is more prone to error due to shower fluctuations and Monte Carlo model dependence. The best data will have both fluorescence and ground-array measurements. In this ca the two types of information check each other and check the validity of the Monte Carlo model used to understand the data. Unfortunately, only 10% of the Auger data will be hybrid data; this may not be a large enough sample to understand the highest energy events.

<u>Comments regarding the Auger proposal and its relationship to the HiRes detector.</u>

1. HiRes is a funded experiment due to turn on in less than two years with much of the same capabilities. HiRes will establish the existence or non-existence of events beyond the GZK cut-off. Assuming that the HiRes detector has operated for three years, according to the Auger proposal P. 67, the HiRes experiment should see about 30 events beyond 10^20 eV and about 3000 events beyond 10^19 eV. In spite of the fact that there may be some uncertainty in establishing the energy of individual events, these statistics should establish this population with some certainty. HiRes will measure the spectral shape of the events above 10^19 eV very well and have sufficient data above this energy to look for anisotropy. Although the Auger detector as proposed (North plus South) will have ten times the aperture of the HiRes detector, since the number of events is falling rapidly with increasing energy, the Auger detector can only effectively search for anisotropy a factor of three to four above the HiRes (up to about 4*10^19 eV). Another larger array such as Auger certainly could be a next step, but the HiRes experiment can give us the basic information we need to understand the requirements of the next step.

2. The fluorescence technique used by HiRes is much better than that of the EAS technique in determining total shower energy and particle type. The Auger collaboration proposes to calibrate their array with a fluorescence detector, but the overlap in events would only be at the 10% level and it will be difficult to calibrate at the highest energies where there are very few total events. It's not clear that the total energy calibration is linear and that a calibration done at lower energies would be reliable at the higher energies.

3. HiRes is a scalable detector. If the need for a larger aperture is established, more and perhaps better optimized HiRes detectors could be built.

4. The HiRes technique seems to be a less expensive technique even as currently designed.

The hybrid detector proposed has advantages over previous experiments that use either fluorescence or surface arrays. The simulations of this hybrid detector suggest that the energy resolution of the surface array will be comparable to that of a fluorescence detector because in this energy range showers are very near maximum which reduces the size dependence of the point of first interaction. The cross calibration should allow them to test this. Providing that this works out, the hybrid array will have both high resolution and extremely large collecting area. The Hybrid array concept is also good in that it has the potential to answer the question of composition by simultaneously measuring the height of shower max, the energy of the shower and the muon component of the shower.

HiRes, because of its low duty cycle, will have a significantly smaller aperture than Auger. Still, HiRes will be operational sooner and has the potential to address and even answer some of the questions that Auger seeks to study. It is quite possible (maybe even likely) that HiRes will have some significant results on the question of the existence of large numbers of events above the GZK cut-off. Right now, the data that argues for these events less than compelling. Indeed it is also possible that HiRes will shed light on the questions of anisotropy and point sources. Further, there exists a possibility that what is learned from HiRes would dictate a different approach for a next generation detector. All of these issues argue for Auger to wait for HiRes to produce results.
On the other hand there are arguments that this is the time to act. The most compelling of these, not a scientific reason, is that the international collaboration exists and is ready to move now and that substantial delay may cause it to break up. Another argument that has merit is that redundancy and competition with the HiRes will benefit the community and give more confidence in this important result. Further the group argues that this HiRes only views the northern sky and that there may be interesting things to see in the south which are not in the north (large scale structure and the galactic center). This too has some merit.

The proposal itself DOEs not clearly specify the nature of the fluorescence detector to be built in the south. The Schmidt design looks interesting, but too little detail is presented to know if this approach is promising. A concern is that the fluorescence detector is an indispensable element of this design and the group may not take seriously enough the effort it takes to make one of these devices work. The expertise in this group to make this part happen is not so apparent. The proposal says that the prototype will solve all of these problems, but that remains to be seen.

The surface array measures the lateral development of the cascade shower well, while the fluorescence detector measures the longitudinal development of the shower. The fluorescence detector can only be operated on clear, moonless nights giving it a duty cycle of ten per cent, while the surface array has a duty cycle of 100 per cent. The two detector types are complementary and can be used to reduce systematic errors, particularly on energy determination. It is proposed that two detector arrays be located in the northern and southern hemispheres to give almost full sky coverage in order to search for anisotropy in the arrival directions of the cosmic rays.

The technology here is mostly conventional. The newest part is the wireless communications, which should work. If conventional communications are required, the cost would certainly be much higher.

Is Now Time to Build Auger?

This question exposes significant weaknesses in the Auger proposal for an immediate construction start. Auger addresses the Science issues, but so DOEs the HiRes instrument. While the event rate is lower for HiRes, it nevertheless will have something significant to contribute to science questions such as existence of and gross anisotropy of beyond-cutoff cosmic rays, and it will make some contribution to the energy spectrum. The issues of composition and producing an all-sky map will not be addressed very well by HiRes, but these are not the high priority questions right now. The crucial question is that of the existence of cosmic rays beyond the cutoff, and the HiRes is poised to make a significant contribution to this. At the SAGENAP review, the Auger group's argument for downplaying HiRes capability in this was that the non-uniform HiRes energy acceptance and careful atmospheric corrections raise significant questions about the veracity of the HiRes energy reconstruction and flux measurement. This following is certainly true: faulty acceptance corrections will impeach HiRes flux estimation, and faulty atmospheric attenuation, Cherenkov contribution, and fluorescence yield corrections will impeach HiRes energy estimation. However, over the last two decades, the Utah group has made great strides in understanding the corrections in their reconstruction. There is no evidence that the Utah group underestimated energy uncertainty in the highest energy Flys Eye showers, and this understood energy resolution, extrapolated to the HiRes is adequate to address the question of the existence of a cutoff; it was hard to follow the Auger argument that the statistics of HiRes is so low that HiRes would notsignificantly address the cutoff question. If HiRes energy resolution function is understood at the level of the Flys Eye (and they have many showers at "lower" energy with which to study their resolution function), then they should have a sufficient number of HiRes events to answer this question. The argument was further sharpened: since HiRes will have just 10's of events well above the cutoff, the tails of the HiRes energy resolution function become crucial in that a misunderstood tail could mimic events above the cutoff. This is of course a valid argument. But, consider: Should the HiRes result be "fuzzy" (this the scenario where HiRes has a few events above cutoff and with relatively poorly understood resolution function) then a more powerful Auger instrument could be deployed. However, should HiRes not find cosmic rays above the cutoff, then there would be very little scientific incentive for building Auger.

This calls into question the scientific sense in building Auger immediately; it might be better to wait for the results of HiRes. It could well be that HiRes sees nothing above the Greisen cutoff or it could be that exciting results from HiRes make it imperative to build Auger, but we won't know this for at least two years. The Auger group argues for an immediate construction start on three grounds

(1) The international Auger collaboration is fragile and anything less than an unambiguous commitment from U.S. agencies might destroy the collaboration;

(2) The HiRes group is slow in building HiRes and we can anticipate equal pokiness in analyzing their data;

(3) The likely outcome of HiRes science will be "fuzzy" and Auger will very likely be needed anyway.

The last issue, a "fuzzy" outcome, was discussed in the previous paragraph; while a "fuzzy" outcome was possible, there certainly were scenarios where this was not the case. Issues (1) and (2) are valid issues of sociology and need careful consideration. Indeed, the Utah group has been slow in instrumenting HiRes and analyzing data. This could be due to funding constraints or to pokiness of the investigators. NSF is increasing HiRes support in consideration of the first case, but NSF should as well monitor HiRes to ensure results are published in a timely way. If the Auger collaboration falls apart, it is unlikely Cronin et al. would be inclined to re-form the collaboration two years hence. However, if should there should be hints of exciting new physics in the HiRes data, the key question would be: Is there another competent group that would be anxious to build a new instrument? (If not, then how compelling is the science?)

There is an intermediate and sensible path. Most of the physics could gleaned from a southern site alone, all but the all-sky map and subtle anisotropy effects. They have recently reduced their emphasis on composition studies because interpretations of composition appear to be strongly model dependent. One could envision building the southern instrument, analyzing the data, then deciding whether a northern instrument is advised.

CONCLUSIONS/RECOMMENDATIONS

One of the impressive aspects of the Auger proposal is the excellent international organization and the particularly strong Latin American interest in the project. In view of this support, coupled with item (2) above, it is natural to recommend that the Auger group begin by constructing the southern hemisphere array first. Together with the Hi-Res detector in the north, this will provide a first look at the whole sky in >10 EeV particles. There should then be a detailed, revised proposal for the northern hemisphere array after significant results toward the science goals are obtained with the southern array. This second stage would be several years hence and would therefore be able to take account of results and experience from Hi-Res. Moreover the cooperation suggested by the memorandum of understanding among Auger, Hi-Res and Telescope Array that is in the present proposal could be developed into a coherent project for the northern hemisphere.

1) The argument for two Auger sites is scientifically very weak; there is no reason to believe that two sites will yield significantly more information than one site, other than statistics. If the data from one site indicates that a second site is warranted, it can be added.

2) We cannot neglect the fact that a strong, international collaboration has been assembled, and one should not turn down this opportunity lightly.

3) While the fluorescence technique might be preferable to the ground-array technique, the hybrid approach is better than either technique alone.This should be the focus of any new plans in this area.

4) A compromise plan wherein the Auger collaboration builds one detector at the southern-hemisphere site is supportable. This detector should be~3000 km^2 in size and should include both surface and fluorescence detectors. The main goal should be to obtain significant numbers of hybrid events above 10^{19} eV.

5) An important condition for U. S. funding of the Southern detector should be that significant foreign contributions to both the construction and operation of this detector should be guaranteed.

6) Only after the first Auger array and its fluorescence detectors are working and significant scientific results from this instrument (as well as from HiRes) have been obtained (so that we have a better knowledge of the true scientific issues above 10^{19} eV), should a proposal for the second Auger array be considered. However, limited efforts aimed at characterizing the Northern-Hemisphere site might be warranted before that time.

There are several advantages of this plan:

1) This gives us the first look at the southern sky in this energy regime. The southern sky contains the Galactic center as well as most of the supergalactic plane. The northern sky will be covered by HiRes.

2) This will result in a second (and, hopefully somewhat independent) group developing the fluorescence technique. The friendly rivalry should lead to faster progress on both sides. The idea of Schmidt optics is an excellent example of this.

3) This will keep the Auger collaboration active and will give them good results as least as quickly as in their present proposal.

4) This will involve Latin American countries in this research and display their commitment to this research (as well as the commitment of other countries).

The experiment should only be approved with the following guidelines:

1. The R&D program should be completed with an engineering design that allows a reliable technical evaluation, as well as validated of cost and schedule.

2. Construction should be undertaken in `series,' with the South American site being constructed first. The reason for this includes the fact that NO Southern array exists; that the group can carry out the construction more expeditiously if they concentrate on a single site, and because questions still exist for the North American site especially with regard to the fluorescence array.

3. The North American site should proceed after significant results on the science goals are obtained from the South American site and the details of the fluorescence array are settled. It should be possible to do this with no loss of time between the construction of detectors for the Southern array and continuation on the Northern array. The rate of completing the Southern array can be more rapid that the original plan, allowing the completion of both arrays in a timely manner.

While the timing of this proposal is perhaps still premature, it is possible to support starting the proposed development project aimed toward the construction of a hybrid array starting in the Southern Hemisphere only. While this may be considered support for the project as a whole, it is only with the qualification that the southern array be built, operated in the hybrid mode and physics results evaluated before northern hemisphere construction is authorized. The southern array can be used to answer many of the physics questions that will not be answered by the HiRes. Unless the results dictate it, the northern array should not proceed. Specifically, results from this southern array should be considered as a gateway to funding of the northern hemisphere array. Once significant results are obtained, a panel should review the instrument performance, the scientific state of the field and the design of the northern hemisphere detector before proceeding.

(a) The HiRes detector will cover the Northern hemisphere with adequate statistics through the next five years.

(b) By that time the Southern Auger detector will have equal statistics as HiRes and it will become possible to assess -- on scientific evidence -- the need for, and importance of constructing a hybrid detector in the Northern hemisphere.

(c) Developments on possible funding of the Telescope Array will have advanced to the point where plans for a collaboration with Auger could be realistically considered.

(d) Operating experience with the Southern detector will provide valuable information on the performance of hybrid detectors built over such an extended physical scale.

(e) The Auger collaboration will be able to concentrate its financial and manpower resources on the construction and operation of a single detector and thus accelerate the schedule for first data acquisition. An additional benefit of constructing the Southern detector is that whole sky coverage will become available early. Recall that all high energy cosmic ray detector arrays are in the Northern hemisphere. Furthermore the

Southern array can view the galactic center and other regions of interest where large scale structure is observed. Finally, the enthusiasm and extensive planning carried out by the Auger collaboration both in the concept and hardware of the detector will be fully utilized.

Costs and Schedules

Construction of a single detector at this time reduces the immediate pressure on the Auger budget and allows the initiation of the project at an early time. Presumably all participants will contribute to the Southern detector and in particular the U.S. should make an important commitment in equipment funds (roughly half of the support requested in this proposal from DOE/NSF for the entire project; a major portion of this committment can be in kind, i.e. in terms of equipment manufactured in the U.S.). Furthermore, the U.S. groups should fully commit their efforts to the construction and initial operation of the Southern detector. When scientific results from the Southern detector become available (a possible timeframe is the year 2003) the construction of the Northern detector should be re-evaluated. This could involve design modifications based on the experience from the Southern detector and/or the physics data accumulated. Furthermore, collaboration with other high energy cosmic ray detectors should be reconsidered and hopefully established. It is expected that the collaboration as a whole would contribute in equal measure to the Northern detector, if approved, as is currently proposed for the Southern detector.

B. GLAST

The GLAST proposal is aimed at implementing a new mission in space to detect high energy gamma rays. This is a follow up to the highly successful EGRET mission by extending the sensitivity by a factor of 10 and improving the angular resolution (0.1 degrees at 10 GeV). EGRET studied sources of high energy gamma rays (30 MeV < E < 20 GeV). That instrument used gas spark chambers and due to gas degradation has now come to the end of its life cycle, except for using the remaining gas for special targets over the coming few years.

Basically, GLAST calls for expanding gamma ray spectroscopy up to several hundred GeV gamma ray energy. GLAST should catalog thousands of energetic gamma ray sources. Compared to earlier instruments (e.g., EGRET on the Compton Gamma Ray Observatory), GLAST will be sensitive to much weaker sources over a broader energy range. The instrument has a large angular acceptance, so the exposure to new sources is large. The hope is that these energetic gamma rays will open up a new astronomy, much as radio and x-ray observations did in other frequency bands.

EGRET has a long list of accomplishments, including the discoveries of gamma-ray quasars, a possible planet orbiting the Gemina pulsar, and perhaps most importantly, about 200 unidentified sources of energetic photons. In addition, observations have been made of prolonged gamma ray emission from gamma-ray bursters, delayed emission of GeV gamma rays from solar flares, and the detection of the Large Magellanic Cloud which helps confirm the idea that there is galactic confinement of cosmic rays. Among the biggest surprises are the observation of ~50 extragalactic sources (active galaxies), the detection of high-energy emission from a number of gamma-ray bursts, the detection of a large number of unidentified galactic sources, and the discovery of a significant amount of diffuse extragalactic gamma rays. It is clear that we would like to know more about the gamma-ray sky, but EGRET is limited by its relatively poor angular resolution, small effective area (especially at the higher energies where shower "backsplash" vetos potentially good events), and limited remaining lifetime. GLAST aims at improving the EGRET design with significantly larger effective area and field-of-view, and better angular resolution. It is a foregone conclusion that if GLAST is built and launched, it will make a number of significant discoveries.

This rich program is based on the highest energy gamma rays studied in space and complement the studies beginning at about 1 TeV on the earth's surface from Mount Hopkins. There is a considerable energy gap (a few GeV to about a TeV) between these measurements and therefore, it is not possible to study how most of the sources seen by EGRET cut off without new instruments that close this gap. The motivation of GLAST is to extend the range of the space gamma ray studies up to about 300 GeV (or until it runs out of statistics), and with an instrument having better resolution. It should be noted that the VERITAS proposal is aimed at extending the ground measurements down and to overlap the data from GLAST.

The physics of GLAST is primarily aimed at high energy particle astrophysics topics, similar to EGRET. This will be done both by increasing the number and detail of objects available for study (through the increased sensitivity) and by measuring the energy spectra to higher energy than was possible with EGRET. A rich sample of data will be acquired, including several hundred AGN's (Active Galactic Nuclei). Study of high energy behavior of Gamma Ray Bursts (GRB) and of the diffuse gamma radiation are also very important astrophysical goals. A particularly interesting study from the point of view of high energy physics will be a search, with some capability to resolve lines in a diffuse backgound, for gamma-ray lines (above nuclear energies) which could be a signature for WIMP annihilation. The nature of such energetic sources, i.e. their spectrum at all wavelengths and their time structure can provide important data of cosmological significance. The excellent energy resolution could reveal the presence of narrow lines in the spectrum which would be indicative of novel interactions at the level of the elementary particles. The direct connection with high energy physics is in understanding the acceleration mechanisms of these high energy gamma rays, as well as the discovery possibilities of finding monochromatic sources of high energy gamma rays that could result from elementary particle physics possibilities like WIMP-WIMP annihilation producing supersymmetric particles.

The technology is unique to HEP and the questions addressed about the nature of the acceleration sources are relevant to particle physics. The cosmology investigations (e.g., absorption of gammas in the infra-red background) and line searches are directly part of particle physics. We should also keep in mind that with the enhanced capabilty over previous instruments they may discover new types of high energy gamma ray sources and thus open new areas of investigation of interest to both HEP and astrophysics.

The combination of GLAST at lower energy and VERITAS at higher energy complement each other well. The role of GLAST in cosmology is substantially weaker. The dark matter annihilation lines and string radiation signatures are highly speculative. The good energy resolution of the GLAST calorimeter seemed driven by the requirement of good energy resolution for these speculative annihilation lines. Can the instrument be built for substantially less if, say, a 10 percent resolution calorimeter were in the design? Likely the search for sources and measuring the spectrum would not suffer.

The collaboration to develop GLAST is a combined HEP/NASA collaboration with SLAC and Goddard Space Flight Center being the respective centers of those activities. The effort at SLAC and with significant University participation is on the design and development of the detector instrument.

A detailed conceptual design of the instrument was presented and is very impressive in its integrated approach to the instrument and the use of modern high energy physics instrumentation to allow high resolution measurements of gamma rays in space. Details like triggering and data handling have been well thought out, as well as reliability and the robustness required in a space mission.

What is needed for this project is a large gamma-ray detector with wide dynamic range, good imaging capability, good angular and energy resolution. This combination is a natural for SLAC, both the institution and this group, because of their extensive experience with photon calorimetry. The partnership with key members of the EGRET team makes a very strong proposal.

The design of GLAST uses stacks of Si-strip detectors to track the electron-positron pairs produced when a photon converts, and a CsI calorimeter. A recent design development is the use of transverse (rather than longitudinal) logs of CsI.

This is a good idea and gives the calorimeter a number of significant advantages:

The ability to point gamma rays impinging the calorimeter from the side.

The ability to study the longitudinal development of the shower in thecalorimeter for better background rejection and a good measure of shower leakage.

The GLAST design team is extremely competent and has done a wonderful job in producing a coherent design for the detector. While there is still much work to be done, there is confidence that the team is up to the task. International collaboration is also essential for such a large project. While the authors have made a beginning at assembling a strong group of international collaborators, this is at present an aspect of the collaboration that needs attention.

There are a few concerns:

1) The GLAST instrument has been designed with specific goals in mind, viz., to improve the angular resolution, effective area, and field-of view of EGRET by significant amounts. However, this is a strawman, which has been satisfied. However, it is not at all obvious that this is the best approach to accomplish the physics goals. For example, much better angular resolution at ~100 MeV is needed to unravel the puzzle of the unresolved EGRET galactic sources - the field-of-view (and, perhaps the effective area) is irrelevant. The field-of-view and effective area are crucial for studying gamma-ray bursts, while the angular resolution is unimportant. The study of the diffuse extragalactic emission may require all three attributes. There should be studies of how possible detector arrangements might affect the physics reach of GLAST. For example, it may be that having better angular resolution for some events (by using thinner converters for part of the instrument) at the expense of conversion efficiency might be a good compromise. The collaboration has not yet done this, but said that they recognize this as a necessary job. It would be helpful to HEP if other particle physics issues could be identified and tested against detector design.

The science that GLAST will address has been reviewed extensively by the astrophysical community. Given the success of the EGRET mission this is a natural follow on and hence the project has been endorsed by all involved in high energy astrophysics. In particular it has received the endorsement of the Gamma Ray Astronomy review panel set up by NASA 2-3 years ago. Although the science put forward at the SAGENAP meeting contained a larger portion of particle physics related material, the major aim of the mission is astrophysics. The neutralino searches can be accommodated without any serious loss of sensitivity to the astrophysics; however, one would be hard put to make these a major justification for the mission.

Although SASII/COSB/EGRET have inevitably taken the cream off the top of the MeV-GeV gamma-ray sky there is still much to be done. Now we have a firm roadmap and know what to expect. Hence there are several important and predictable results; there is also the possibility of some new and surprising results (one thinks of the contribution of the Einstein mission to x-ray astronomy after the UHURU survey satellite).

There is the question of whether the study of GeV sources is really the concern of the DOE High Energy program. Having argued this case for TeV gamma-ray astronomy it is not too hard to extend the argument to GeV energies. However, when one starts to talk of 5,000 sources, one is certainly entering the realm of astronomy and it DOEs require some further justification, in particular since this is a new departure for DOE. High Energy Gamma-Ray Astronomy utilizes the production, propagation and absorption process of photons of GeV energies and above ... and hence has some overlap with the mission of the DOE HEP program. However, the subject DOEs not bear much on the basic constituents of matter or fundamental forces of nature which comprise the main thrust of the DOE-HEP programs.

The flux sensitivity claimed for GLAST is not in dispute. However one should regard their high energy sensitivity with some caution. The upper limit is claimed as 300 GeV but at that energy they will detect only 30 photons in a year from the strongest known steady source at that energy (the Crab Nebula); in contrast Whipple can detect that many photons of 300 GeV in just 15 minutes (and VERITAS in a lot less!). For the weaker sources they will have little sensitivity above 100 GeV. However the overlap between GLAST and the new ground-based observatories is important and may be the basis for some of the most interesting science.

Detector

The proposed instrument is a state of the art detector on silicon tracking and total absorption crystal calorimetry. This is a highly reliable and widely used modern technology in accelerator particle physics experiments. It is ideally matched to the scientific goals of GLAST.

While alternate technologies are being explored, the proposed design is probably the only sensible one, even if it is slightly more costly than the others. Furthermore the experience of the team that designed the prototype provides assurance for the success of the final detector. However, the schedule is aggressive and depends on support from foreign institutions which is at present uncertain.

Technique.

In the detection technique proposed (silicon strip) versus the alternative technologies, there is some unhappiness amongst those was champion alternative technologies (scintillation fiber, micro gas counters) for the Tracker or Calorimeter who feel that they may not have gotten a fair chance to propose their technologies. There is the perception that because DOE is potentially a major player in the support of GLAST that the deck is stacked in favor of a technology that has DOE support. The EGRET experiment was a joint venture (within the US) of Goddard and Stanford. There was competition between the two groups for the successor mission. On the prospect that SLAC would take over the management of the mission, the Goddard group seemed to be fully in agreement. One might suspect that the Goddard group may not play as major a role in the experiment as before.

Logistics

GLAST has been strongly endorsed as part of the named future missions of NASA in FY '99 (in the Structure and Evolution of the Universe ``Theme"). NASA provides the major funding for the mission. The instrument costs are to be provided roughly 1/3 by DOE/HEP, 1/3 by foreign collaborators and 1/3 by NASA. The contribution requested from DOE/HEP is in the range of 30-40 Million $. Therefore, GLAST would represent a second major collaborative venture between DOE/HEP and NASA, but at a significantly higher cost level. The opinion was expressed that future science projects might necessitate such interagency cooperation and support; nevertheless, if the project is approved at this level, the impact of this major commitment on the accelerator based HEP program should be examined at a level beyond SAGENAP. Another feature of the GLAST proposal is that some of the principal U.S. participants as well as the engineering and management effort would come from SLAC. This implies an important broadening in the activities and scientific direction of SLAC, which however seems to be fully endorsed by the SLAC directorate. For SLAC to maintain a leading role in GLAST it must somehow broaden its own scientific team involved in the experiment. This might necessitate the appointment of faculty with experience in astrophysics and increased participation in GLAST by the SLAC scientific staff.

2) The GLAST detector is to be built collaboratively by DOE, NSF, NASA, and foreign agencies. It will be an expensive enterprise and cost overruns are likely. It is crucial that the US continue to have a broad non-accelerator program - its breadth is one of its strengths. It is important that the commitment to GLAST not be made at the expense of the rest of the non-accelerator program. The offer by SLAC to make GLAST a priority

part of their program is a sign that may be indicative of the desire to expand the non-accelerator program. In any event, DOE and NSF must understand what role they are playing in GLAST and what their potential liabilities might be. One must play according to the rules of NASA, and we don't have good experience here.

3) Although the GLAST proponents claim that the possible alternative design options (sci-fi, e.g.) are not viable, NASA must make the design and construction subject to open competition. It is unsure about whether SLAC is committed to GLAST if an alternative design is chosen. This could be a litmus test of their commitment.

The harder question is the DOE and NSF's role in this project. In a sense the NSF question is also somewhat easier than DOE. NSF can have a significant role by supporting groups to work on the project as well as perhaps supporting some small to moderate piece of the hardware. This research is close to the type of work being done by many NSF supported groups and fits in to the program. DOE is different. While this project is being done using high energy physics techniques and detectors, it isn't traditional high energy physics. Nonetheless, it is the kind of science which more and more is drawing high energy physicists to it. Still, there are many questions that need to be answered about the process and a few about the detector.

The role of SLAC in this project presents several questions that need to be carefully considered. First, is it appropriate for SLAC to play the role of the instrument builder in this experiment? Second, if they take on that role, should DOE pay for it? Third, how do the other participants collaborate? Finally, how will SLAC fit into the NASA project management scheme?

It is appropriate for SLAC to take responsibility for the construction of a part of the instrument. They have the expertise and the capability to do the job and do it right. (Their design work is indicative of this). They also make an argument that this fits in with the lab schedule - after Babar and before the NLC. They have expertise in silicon, and contrary to the case made for the open design competition, many in the community believe the decision to go with silicon has been effectively made.

There are a number of issues associated with this project which are more political than scientific. They will be listed here, but it is the role of the collaborators and/or the funding agencies to address them. Specific issues are:

1) The relationship between DOE and NASA. Are there any science projects of this magnitude at the interface of interests of the two agencies? How much will NASA requirements raise the cost of the DOE contribution?

2) The role to be played by international collaborators. The design of the instrument should not be compromised to gain resources from international collaborators. It would be useful to understand better exactly what the requirements of foreign participants will be.

3) Data rights. The high energy physics community and the astronomy community have very different cultures regarding access to data. The issues associated with data distribution should be settled at a very early stage.

Foreign Support

The lack of formal commitment from the foreign groups who are expected to be equal partners with DOE and NASA in this venture was noted. In contrast with Auger, which had a very detailed international collaboration, the GLAST collaboration seems very loose and uncertain. It is perhaps a measure of the difference in funding of space projects that sums of tens of millions of dollars could be discussed so casually by the proposers; it was striking that there were no foreign representatives present at the SAGENAP meeting (again in contrast to Auger). This was the weakest part of the proposal and that DOE would be well advised to insist on more formal pledges of support before making a commitment of this magnitude.

There are a number of cautions which were raised in the discussion which will need to be watched as the project progresses. Among them are:

1) Is the proto-type testing on a truly realistic timeline for final construction in view of the fact that they are still considering new variants of the design which have not yet even been computer simulated?

2) Progress must be made soon on technical integration of the US and non-US contingent for the instrument that preserves its scientific integrity without the cost of loss of collegial involvement of all. This can surely be done but it must happen soon. A starting avenue has been made through some of the collaborative work on the silicon. And they have shown an awareness that this is an issue so there are no signs that failure on this score is pre-ordained.

3) NASA protocols on open access on the data, which is so different from the norms in particle physics, should be carefully examined. Since this is an HEP data gathering instrument and the HEP persons having devoted their ingenuity, time and money to its design, development and construction they should have a better guarantee of priority of analysis and publication of the data. Maybe the agreement on the Ting space station experiment is a good starting point for discussion with NASA.

4) While it is clear that the SLAC, et al. capability is there for the instrument and they will be the "blood,sweat & tears" of the construction mentioned in (3) they are thin when it comes to being in a position to exploit the data right off. They should be concerned that HEP gets its money's worth.

Recommendation

There is strong support among the members of SAGENAP for the scientific goals of GLAST and the involvement of the high-energy physics community in it. The scientific goals should be sharpened somewhat and a clear understanding achieved of how GLAST fits in the non-accelerator program.

The question about DOE paying for equipment is a question difficult to answer. Given the good prospects for international cooperation and the potential role of SLAC, DOE has an opportunity (but not an obligation) to play a significant role as a partner in this interesting project. The question is: Would NASA fund the project and pick SLAC as the prime contractor if DOE chose not to provide construction funds? This is hard to answer and it ties to the issue of whether NASA will take to DOE cooperation to heart. These are questions that the DOE should look at carefully.

If SLAC is given this project, it is essential that they work cooperatively with the other DOE, NSF and foreign supported groups. An advisory board should be setup to oversee the project, not controlled by either NASA, DOE or SLAC, but representative of all of the participants.

As to the last question of how SLAC and the NSF supported groups would fit into the NASA management scheme - some clear guidelines should be expected as to what the scope of the participation would be and how costing and oversight would take place. Its important that if DOE chooses to support this project, it have a clear idea of what the final costs will be. Equally important, GLAST cannot be allowed to kill off the rest of the non-accelerator program.

Given the endorsements that GLAST has received already there is little doubt that GLAST will fly irrespective of DOE support. The justification for DOE support is surely that it will accelerate the mission, will use technology developed at DOE labs and provide a major research project for a lab during a slack period. If NASA can find $30M to support the detector development (out of a total budget of $300M for the mission) one cannot believe that by stretching the mission slightly that it could not fully fund the detector. Hence the situation here is quite different than Auger; if DOE/NFS DOEs not approve Auger then the project is dead, at least for the next ten years. In contrast, if DOE opts out of supporting GLAST there is every reason to believe that there would be sufficient pressure on NASA to fund GLAST entirely (but perhaps not as quickly).

Traditionally although NASA has taken on the mantle of the patron of gamma-ray astronomy it has reserved this support for space telescopes. In the many reports that

NASA produced on gamma-ray astronomy in the past it has largely ignored the possibility that gamma-ray astronomy might be pursued from the ground.

This is an issue for a project such as VERITAS where some of the collaborating groups have traditionally been supported by NASA in space research; they have been notified by NASA that they can count on no support from NASA for their participation in VERITAS because "NASA DOEs not support ground- based research".

There is some concern that if this program is funded by DOE, there will be a definite tendency within NASA to turn to DOE for the development of every future high energy gamma-ray or particle mission. It could be seriously detrimental to the development of high energy (cosmic ray) space research if funding falls between two agencies. DOE should go into this with its eyes open.

Even more important is to develop a clear agreement on how the cooperation between NASA and DOE will work, both at the group level (because both NASA groups and DOE groups are in the collaboration) and at the agency level (because of the different procedures for approving proposals and mounting missions/experiments). If DOE decides to fund GLAST at the level requested, a memorandum of understanding needs to be developed between DOE and NASA that spells out how the mission will proceed and minimizes the chance that the project will be cancelled after a large investment has been made in the detector. It should make clear that expenses resulting from NASA safety and review requirements will be borne by NASA. The MOU should also take account of the international aspects of the collaboration.

If SLAC is to funnel the DOE funds to GLAST then the funding picture is quite different from direct funding via the University program. It might be politic to have it reviewed also by HEPAP. GLAST is probably the largest project to come before SAGENAP and is also the one whose mission is the furthest removed from High Energy Physics. There might be a backlash if there is not greater community consultation.

This is a very good experiment and is a well chosen place for high energy physics (DOE) participation in a collaborative mission with NASA in space. GLAST has been very well reviewed in NASA for the science and a mission with these goals in part of the long range plan. The GLAST collaboration appears to have a sound plan to develop such a mission, first with an R&D phase and then with the mission itself. It is a good activity for SLAC as an opportunity between the B-factory and a future linear collider. However, the level of DOE funding for such a mission is still to be determined and the amount should reflect the fractional particle physics component of the physics goals. There is concern that cost containment of space missions will be difficult, since it contains elements (QA, reliability, robustness, etc) that go beyond the normal HEP/DOE experience. If DOE enters into this partnership, an approach to these problems must be found that assures the costs to the DOE will not significantly escalate.

In summary, DOE and NSF should seriously consider supporting this obviously worthwhile and exciting project, but it must carefully enter into agreements with the participants that ensure that the costs remain reasonable and that all groups have a say in the project management.

A straight forward approach would be to cap the DOE equipment funding at some modest level. DOE should explore “self-limiting” mechanisms to ensure that the funding cap cannot be violated very much, if at all. The level of funding could be gauged by the size of the Non-Accelerator Program as a whole, and the relevance of the project to the central goals of High Energy Physics.

The question of DOE “sponsorship” of GLAST is more subtle. If the DOE equipment contribution is modest, it makes no sense for DOE to assume leadership responsibility for the GLAST instrument. Notwithstanding SLAC’s obvious management expertise, management of the GLAST project would be a major distraction for the laboratory. Rather, with a modest equipment contribution, a lab-to-lab MOU between SLAC and Goddard or between SLAC and the GLAST collaboration involving a modest number of deliverables seems more appropriate than a DOE-NASA MOU, with its attendant ponderosity. The DOE approach to this sort of involvement in non-accelerator projects by accelerator labs is to keep it modest. This general approach seems appropriate for GLAST as well. Particularly because GLAST has so few goals related to understanding of the basic constituents of matter and fundamental forces of nature, a major investment seems inappropriate. Nonetheless, even if the DOE investment is modest in size of expediture, DOE/HEP should expect that SLAC will not simply lend its technical know-how and infrastructure to GLAST, but would also become intellectually involved in it. This approach fits in with the spirit of the Non-Accelerator Program and is consistent with the views of the SAGENAP members.

C. Super-Kamiokande "Maintain and Upgrade" Proposal

The Super-Kamiokande detector is an important follow up to the Kamiokande detector. The investment by the Japanese, the professional manner in which it was constructed and commissioned, and the early results are extremely impressive. This device is playing a central role in determining the physics of the solar neutrino puzzle, as well as the atmospheric neutrino anomaly. Ultimately, it will lead to the most sensitive searches for proton decay in most possible decay channels.

The U.S. participation has involved responsibility for the veto array surrounding the fiducial volume and participation in data analysis and physics. The U.S. group initially developed an independent analysis from the Japanese and this proved useful in extracting early physics as a cross check on results. More recently, the U.S. analysis effort is integrated into the effort of the Japanese group.

The proposal presented at SAGENAP was for hardware toward the upgrade scheduled to occur in 1999. The total amount requested is $1.1M, of which about half is for new photomultiplier tubes (both to replace failed tubes and to augment the array in some areas where it is deficient). The remainder of the request is for hardware to upgrade the computer systems, the high voltage distribution, radon system, electronics and the calibration system. No arguments were presented to motivate these upgrades in terms of improved physics performance, and instead, they appear to be directed mainly for replacement of phototubes, updating computers and electronics, etc. The amount requested seems high for this type of maintenance and the justifications not very detailed. One would guess they could pick and choose where to make this investment and do the most important tasks for significantly less money. The principle of continuing equipment support at a level averaging a few percent per year of the U.S capital investment is a well justified continuing investment in this successful experiment.

As for the long term future of Super-Kamiokande, it is pretty clear that a few year program to pursue the present physics goals will be productive and important. It is unclear whether a long term program is envisioned or possible, and how the long baseline neutrino experiment from KEK fits into these plans, and what the U.S. role or commitment is to that part of the program.

The US collaborators in the Super-Kamiokande project present a proposal for $1.117M for maintenance and upgrade of their detector. The Japanese are planning to put roughly ten times this amount into the maintenance and upgrade of the detector, which is the same ratio of Japanese to US funds in the original construction.

The Super-Kamiokande collaboration has been immensely productive, and the US participants have played an important role in the total success of the project. They have made predictably good measurements of solar neutrinos and have a good start on a new level of search for proton decay. Most importantly, they have continued the investigation of atmospheric neutrinos begun in Kamiokande I-II-III, and are seeing very strong evidence for neutrino oscillations. Continuing the work on atmospheric neutrinos, which is both statistics and systematics limited, is adequate justification for supporting the request for additional funding.

They need to replace failed phototubes in the anti-coincidence section, improve the high voltage delivery system, provide a real-time monitor of optical attenuation length, replace the radioactive nickel calibration source, provide some spare fast electronics modules and improve the radon removal system in the areas occupied by personnel. In another category is the need to keep pace (in the technical sense to handle data) with the improvements made by their Japanese colleagues to the major data acquisition system for the detector itself.

At the SAGENAP meeting the collaboration presented new data on proton decay, atmospheric and solar neutrinos. The results are truly impressive both in the quality of the work and the nature of the results. They are breaking new ground on very important questions in particle physics. With the capability now to measure energy at the 1% level they are getting close to providing the first test for possible spectral distortion for the B8 neutrinos. They have strong hints (and powerful analytical tools) of possible neutrino oscillations in the atmospherics. They also surveyed the various difficulties which have led them to propose the present upgrade request. They made a cogent case as to how failure to follow through on the bulk of these upgrades will be reflected in degradation of the experiment and the US participation in it. About half the cost is in phototubes and their replacement must occur on a timeline starting soon. All their requests seem to be well motivated and legitimate. Their budget in detail can't be too far off since so much of it is basically "catalogue priced". This is a great experiment and a real bargain.

D. ICARUS

Liquid argon detectors have been popular since the late 1970's when there was an intense development effort at UCI lead by Herb Chen and Peter DOE. Liquid argon ionization detector technology began with the efforts of Willis and others in 1974. Early designs made use of inert inserts within the liquid argon, both as electrodes and as converters for electromagnetic and/or hadronic cascades. People recognized that these inserts degraded the energy resolution of the calorimeters and the first liquid argon shower counters were made with many thin converter sheets to try and minimize this effect. Eventually wire planes were introduced as electrodes. These detectors have totally sensitive volumes and if sufficient spacial resolution is achieved these detectors would have the same unique capabilities and could be favorably compared with bubble chambers. Herb Chen recognized this and a 5000 liter detector was proposed in 1976 in collaboration with Caltech to study neutrino physics at FNAL. It was realized that spacial resolution of a few millimeters with closely spaced wire planes led to technical and financial difficulties so, the idea of drifting ionization electrons over large distances and collecting the induced charge as a function of time was actively discussed. Test detectors were built over the next several years at Irvine and it was eventually demonstrated that the argon could be kept sufficiently clean to drift electrons up to one meter. A test detector was taken to Los Alamos and particle tracks were reconstructed. In 1980, both Caltech and FNAL dropped out of the development project due to other demands on their manpower and resources. The efforts at UCI continued with an aim towards developing a detector for solar neutrino studies. The program ultimately stopped with Chen’s death in the late 80's.

The ICARUS effort started in the early 80's and had a slow development pace. Ultimately, they duplicated the work at UCI and have now constructed a 50 liter chamber and operated it in a neutrino beam at CERN. They have successfully demonstrated the reconstruction of neutrino interactions in the volume. The ICARUS 600 ton proposal has been approved by the Gran Sasso laboratory and there is hope for additional modules to get to 2,800 and eventually 4,800 tons.
The detector funding is essentially all Italian.

Physics potential
The collaboration identifies Nucleon decay, solar neutrinos and atmospheric neutrino studies as their primary goals with a possible long baseline experiment from a CERN neutrino beam as another potential use. The 600 ton module would probably be able to contribute to solar neutrino studies but the other topics will require a significantly larger detector for anything other than confirmation of earlier results. Even the 4,800 ton detector would only have 3 x 10^33 nucleons so it would be marginal for any mode that can be seen by SuperK. As an example, the SuperK limit on neutrino K+ is already at 3 x 10^32 years and should easily reach 10^33. On the other hand, if proton decay exists at some level, the ICARUS detector would be ideal for observing a single event and convincing the community that it really was proton decay. It can also search for modes which cannot be seen by the water Cherenkov detectors. For this reason, a large liquid

argon detector would serve a useful purpose. The 600 ton module is a first step towards that goal.

Liquid TPC's offer the means to achieve massive, high resolution detectors, allowing access to new regimes of neutrino physics and rare decay processes, such as neutrinoless double beta decay and low energy reactor and accelerator neutrino physics. At a reactor for example, the liquid TPC has considerable potential at such a high flux, low energy, DC neutrino source. The device could be primarily triggered using scintillation light (liquid argon is a good scintillator). The excellent spatial and energy resolution would offer a powerful means of background suppression. A modest detector (a few tons) could possibly be operated at an energy threshold as low as 0.5 MeV, allowing a high statistics measurement of the elastic scattering cross section, sufficient to detect the m/E contribution to the cross section as it appears in the Standard Model and to put a more stringent limit on the magnetic moment of the neutrino. In addition, doping the argon with a few percent methane would provide free protons offering inverse beta reactions.

After many years of R&D leading to a three ton 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.

The scientific motivations are more difficult to understand. The contribution to solar neutrino information is modest, at best. A study of nucleon decay with good resolution should have at least 4800 tons of detector, but is still less than one quarter the size of Super-Kamiokande. (The nucleon decay motivation sounds very much like the arguments put forth by the Soudan 2 collaboration in the 1980's.) For atmospheric neutrinos using 2400 tons, Super-Kamiokande is eight times larger, which is important in that the acceptance for contained events is greater as well as giving a larger counting rate. To this reviewer, the best scientific motivation for this work is some possible long baseline neutrino oscillation experiment which may be performed at an indefinite time in the future.

While we have had presentations on Icarus before (and a request for funds last time), this time there was the added interest in its potential for addressing the atmospheric neutrino zenith distribution by measurements on the recoil proton (invisible to SuperK). Even with the few hundred events estimated for the 600 ton (liquid Argon) first module this might turn out (as with Soudan-II) to make a valuable contribution. However, there is now the feeling that here is something we should take more seriously as well as UCLA's participation in it. In the previous request, they focused on the possible solar neutrino physics with the 600 ton module. Because of detector and reaction thresholds, the events would sample only the very highest part of the B8 spectrum. The argument for doing this was not very compelling; there were not many events and would come at a time well after much larger samples had been acquired by SuperK and SNO. There is still the feeling that Icarus is unlikely to make any important contribution on solar neutrinos.

<u>Recommendation</u>

Before discussing UCLA's specific request, something should be said about the detector and its overall program plans. At CERN there has been extensive R&D and beam testing (recently also in a neutrino beam) in as much as a 5 ton prototype. The quality of the track imaging is very impressive and soundly demonstrates the efficacy of the technique. We were presented with a report which suggests that they now have a tested and final successful version of a modular cryostat. This project seems to be going ahead in a step-wise manner (addition of modules to reach higher target mass) as physics goals, funds and other opportunities present themselves. While their original and ultimate goal is to achieve ~6000 tons for the principal purpose of proton decay, they now seem on the way to being approved for ~4800 tons to carry out a long baseline neutrino oscillation experiment with a CERN beam directed to the Gran Sasso. This is overwhelmingly an Italian and CERN enterprise with Rubbia as spokesperson. The project will go ahead regardless of what DOE DOEs with UCLA's request and will do so with or without UCLA's participation as they see fit. UCLA's Cline was one of the original authors on the PRL proposing Icarus and has been a long time, at least intellectual, contributor to the enterprise over the years. We should take cognizance of what this particular request is likely to produce. The request asks for ~$50K per year for 4 years to construct high voltage feed-throughs for the first 600 ton module and for some enhancement of their computer power over that period to participate actively in the analysis. UCLA has apparently, through students and post-docs in L.A. and CERN, actively participated in the high voltage R&D; they propose to continue on through the construction and installation. One DOEs not doubt that the Cline/UCLA group will be active in all the physics at a participation level far exceeding the tiny fraction of the detector's cost contributed. Solar neutrinos aside, there could be some significant results over the years from the various experiments this detector can do. This will be the only US participation in it; it's likely to be worth this "admission ticket" price. If approval is recommended, somebody really has to check the feed-through cost figures.

E. VERITAS

The VERITAS group gave an overview of the physics and a "straw man" outline of the hardware. Briefly, Veritas is an advanced generation of the very successful Whipple ground-based instrument, and much of the original Whipple group is part of VERITAS. The key element of the instrument is imaging: with this VERITAS could have good discrimination of gamma rays from hadronic backgrounds and good pointing towards sources. One major physics issue is the origin of cosmic rays; one supposition is that shock fronts in supernova remnants are sites of acceleration. The good angular resolution of the instrument can resolve the shock region from the central region of remnants, therefore contributing to this issue. The group did not submit a formal proposal, rather a letter of intent where they lay out the physics goals and baseline instrument. DOE-HEP should decide whether large ground based instruments such as this one, whose purpose is to do high energy gamma ray astronomy, are in their purview. If so, then the VERITAS should be given serious consideration. The combination of GLAST at lower energies and VERITAS at higher energies cover the EGRET-through-Milagro energy range. Non-imaging instruments like Celeste and Stacee will have a hard time competing once GLAST and VERITAS are running.

Background

The VERITAS collaboration will propose to build an array of 8, 10 meter aperture air Cerenkov telescopes; each of which closely resembles the existing 10 meter Whipple telescope. The array approach allows the observation of the same shower from several points of view, thus pin-pointing the shower core with a much better precision than that afforded by a single observation. This approach is an extension of the stereo set-up pioneered by HEGRA a few years ago. In order to off-set the loss in aperture which results from requiring all telescopes to see each event, the collaboration has proposed a novel triggering scheme whereby the telescopes are independently triggered and the overlaps searched for off-line.

Gamma ray astronomy has proven to be an exciting and productive area. Based mostly on the techniques developed and improved by Whipple, an ever increasing number of sources have been observed. The Crab nebula has become the standard candle whereby the sensitivity of instruments are compared. Gamma ray astronomy in the TeV energy range may be an important tool in supernova research, understanding pulsar radiation mechanisms, determining the density of intergalactic light, determining the time of galaxy formation, and of course understanding the origin of the cosmic radiation. The combination of air Cerenkov telescopes in the TeV range and the GLAST instrument at lower energies can cover the gamma ray spectrum from 10's of MeV to 10,000 GeV with an improvement in sensitivity of almost two orders of magnitude over previous generations of experiments.

This collaboration is driven by the Whipple group which has single-handedly developed the field of Gamma Ray astronomy in the energy range around 1 TeV. They have spent many years learning how to discriminate against hadronic background and have slowly improved their technique.

The Whipple group proposed an upgrade to their 10 meter telescope in 1996. The upgrade, called "Granite III - A Gamma ray Telescope for the Next Millennium"was discussed and approved through the SAGENAP process. In that proposal the telescope camera was to be upgraded from a 109-pixel array to a 541-pixel array. The projected sensitivity of this device in the 100 GeV to 1 TeV energy range was similar to that now proposed for VERITAS and it was shown that further exploitation of the topology of their image can further discriminate against their predominately single muon background. At that time they also proposed the further development of an additional 11meter telescope to act in stereo but this was generally felt to be premature.

In this proposal, we have not heard any mention of Granite III. The group DOEs not include its sensitivity in its detector comparisons. One would like to hear what happened to the Granite III development and understand why it is advisable to go forward with VERITAS now and what extra sensitivity it will provide.

We have to be careful that we are maintaining some kind of balance in the overall non-accelerator program. That is, would VERITAS, GLAST and STACEE be devoting to one area a slice of the funding pie too large to maintain health in the rest of the program?

This proposed array of Cherenkov telescopes will be complementary to GLAST, covering the energy range < 300 GeV to > several TeV. The telescopes will be based on the Whipple design. The Whipple telescope has made outstanding discoveries of the blazars Mrk 421 and Mrk 501 as well as studying the Crab Pulsar and other objects in great detail. Filling in the energy gap between EGRET and WHIPPLE is a central goal of VERITAS as well as GLAST. This is necessary to distinguish between hadronic and electronic models of the sources and to achieve a detailed understanding of whatever turns out to be the correct model. Whipple is limited to following a single source for a relatively short time. The VERITAS array will allow a sky survey to be made in the high energy region and it will have significantly greater sensitivity than Whipple, as well as Milagro, STACEE and Celeste. The international collaboration that has formed for VERITAS is strong. There is the potential for significant support from the Smithsonian, which is appropriate for a long-term observatory. As this project moves from letter of intent to proposal, it will be important to define and justify the extent of DOE participation. One connection is that a major goal of VERITAS is to understand one of the most likely potential sources of extragalactic cosmic radiation, a subject that is the focus of the Hi Res and the Auger projects.

The Whipple Observatory has carried out pioneering work in the detection of point sources of x-rays by using the extended Air Cerenkov technique (in the energy range 0.1 to 10 TeV). VERITAS is a proposal to extend this work by constructing an array of eight 10 m dishes. The (augmented) Whipple team which proposes to construct VERITAS is fully qualified to do so and the technique seems adequate. It is proposed that funding be shared between the Smithsonian Observatory and DOE/HEP.

Reservations are of programmatic nature: (a) The program is one of observational TeV ÿ-ray astronomy. While the techniques used have emerged from HEP the connection of the experimental goals with those of the HEP program is less clear. (b) DOE/HEP is supporting a large number of such observational programs in ÿ-ray and cosmic ray astronomy. Is this appropriate? Are all of these programs needed? Should one wait and see what the new initiatives yield before committing to new observatories? (c) In view of these concerns and since Whipple is still in the process of upgrading its present telescope it is perhaps premature to consider DOE/HEP support for further expansion.

Although this was not a formal proposal, the scientific case very well discussed and extremely strong. VHE gamma-ray astronomy truly has come into its own as a flourishing field with many exciting observations under its belt. VERITAS is a natural next step, exploiting the mature technology of imaging air Cerenkov counters with a well-thought out plan to lower the energy threshold; this will nicely merge with the goals of GLAST. Significant funds have been committed by the Smithsonian Institution making this project particularly attractive. VERITAS would ensure continued U. S. leadership in this field.

This was a useful "heads up" presentation on the expected proposal to SAGENAP at a later time of the multi-dish extension of the Whipple air Cerenkov technique. Just as the Whipple work to date is certainly appropriate and significant physics for HEP and SAGENAP so too is VERITAS. Operating from the ground and with the air cerenkov technique, it improves the sensitivity and takes off (to higher energies) where GLAST leaves off. There is a conscious effort on both projects part to overlap slightly and so to close the present gap in our knowledge of the gamma-ray spectrum. The recently approved STACEE project is an exploratory experiment in the middle of this overlap range.

Recommendation

VERITAS is a proposal to build a next generation gamma ray telescope to follow up the work of Whipple. The energy threshold will be reduced to below 100 GeV and this will overlap the acceptance of GLAST, which will allow tracking the energy behavior and cut-offs of the high energy gamma ray sources. The energy resolution and angular resolutions will also be improved by factors of three, making this device powerful for studies of sources, like AGNs, look for correlations between x-ray and gamma ray flares, study gamma ray bursts, look for neutralino annihilations, etc. This seems like a natural next generation project, has funding from the Smithsonian for development and has expanded the size and strength of the collaboration. The issues of the optics, pointing, etc., are understood. The trigger needs more work. A simple extrapolation of the Whipple instrument trigger to VERITAS would not fully exploit the instrument capabilities and in particular would squander some of the energy reach at the low end. They should be encouraged to pursue this work toward a formal proposal.

F. Soudan II

Physics

Soudan II was built to search for nucleon decay. In particular, since Soudan II is small in comparison to IMB, it was to concentrate it's search for nucleon decay in those channels where the water Cerenkov detectors were thoughtto be background dominated, such as the supersymmetric mode, proton to neutrinoand a K^+. After a 3.6 Kt exposure Soudan reports a limit on the mode proton to neutrino, K^+ of 4.3×10^{31} years (no background subtraction). This is to be compared with the existing background subtracted limit from IMB of 5×10^{32} years. Given the approximately 20% uncertainty on the background, the IMB limit is still one order of magnitude better than Soudan. Early results from Super-Kamiokande are already equal to those of IMB but with very little or no background and are expected to improve by at least a factor of ten. The notion of making a background free search with a small detector is fine but clearly the discovery window has closed and there is not much to be gained in searching for nucleon decay with Soudan II.

The atmospheric neutrino anomaly was first observed in 1986. Up to about a year ago, only the water Cerenkov detectors saw the effect, with inconclusive results from the iron calorimeters such as NUSEX, Frejus and Soudan. It was thought to be important to clarify the iron calorimeters results since the neutrino cross-sections on oxygen are not measured in this energy range. Now that results from Super-Kamiokande are available, the zenith angle distributions give an effect which is independent of the neutrino cross-sections so the iron calorimeter check is less important. Nevertheless, it has been gratifying to see the latest Soudan II results confirm the muon deficit. At their presentation to SAGENAP we saw a new analysis presented by Soudan II. Taking good advantage of their fine grain resolution for the first time, they presented an analysis of those neutrino events where the recoil proton can be reconstructed as well as the lepton. Since the detector is small, they have very few high energy events. This presents a problem since the direction of the lepton in low energy events is not well correlated with the initial neutrino direction. By reconstructing the direction and energy of the proton as well as the lepton, the initial neutrino direction is well constrained. Unfortunately there are still a small number of events to deal with and the background to this data set is at the 25% level, but it represents a niche in the observation of atmospheric neutrinos that no one else can currently look at.

Comments

The new vitality shown by the Soudan group was impressive. The new analysis technique for the low energy neutrino events is very nice and should give us some new information after all the bugs are shaken out. Have they looked carefully at the geo-magnetic effects at their latitude, what effect DOEs possible errors in background subtraction have,...?

The Soudan II collaboration has proposed that it keep operating until the MINOS project comes into being in a few years. The most important scientific motivation for this action is to operate the only detector other than Super-Kamiokande that is sensitive to atmospheric neutrinos. The additional exposure will be analyzed for nucleon decay, but this is not adequate motivation for support.

The additional motivation for continuing operation is to utilize this well tested detector when the first beam for the MINOS project is available. Discontinuing operation of the detector now will have costs associated with keeping the detector from becoming contaminated. Further, some of the required funding is coming from Fermilab through the MINOS project. The remaining funding requested is relatively modest.

Two important things have happened since we met with the SOUDAN group two years ago. The first is in the reorganization of their data analysis part of the program (maybe "change of leadership" should be the phrase but perhaps that reads too much into Mann's fine presentation) and the second is the new role they can play with respect to atmospheric neutrinos. The detector is running very well apparently and they presented to us proton decay results based on 3.56 K-ton-yrs (from a total of 4.05 K-T-yr). They have produced a paper recently accepted by PRL on the same data and at the Spring APS they presented results p--> nu-K for 3.72k-t-y --- that is really significant progress in an area for which they were chastised last time. The new results from SuperK on atmospheric neutrinos favor an oscillation solution rather lower in delta-m-square than the earlier Kam-III experiment. Part of the measurement involves the zenith angle determination of events. Angular resolution in the water Cerenkov method involved the muon only. In SOUDAN they have shown a capability to also include the recoil proton in the zenith angle fit. Applying these measurements in their own oscillation parameters determination they disagree with the new SuperK result and agree with the older K-III result. While this is not yet a definitive result they are showing the ability to provide a complementary experiment on a very basic and important question. There are other practical consequences to resolving this mass difference question since the viability of some of the long baseline experiments depend upon the putative mass range. They would like to keep the funding level they currently have to achieve 5k-t-y and be ready for MINOS. The details of what constitutes the level and nature of the phase-down support for SOUDAN-II is closely tied to shifts among the present Minn.-Argonne-Tufts and CDMS-II/FNAL and MINOS/FNAL responsibilities in the mine. At least the ability to contribute to the atmospheric question warrants working out some kind of mutually acceptable mechanism to keep them viable for that purpose. Later, just keeping the gas flowing so as not to ruin the chambers seems FNAL's problem.

The arguments given by the Soudan group are persuasive for providing enough support to keep the detector running until the long-baseline neutrino beam turns on so that Soudan II can be used for a first look at the beam before the MINOS detector turns on. In the meantime, the fact that the Soudan detector can often detect the recoil proton in neutrino interactions, means it can fully reconstruct some quasi-elastic events and thus improve on the angular resolution of Super-Kamiokande for quasi-elastic events in which only the charged lepton is detected. Who knows what to make of the suggestion made by the Soudan group during their presentation that the mass difference squared from their data is rather higher than that suggested by analysis of the Super-Kamiokande data? They correctly point out that the current Super-Kamiokande value is noticeably lower than that reported for the Kamiokande data. It is certainly important to get to the bottom of this, and it will be useful to have two groups working on the problem with different techniques, even though the Soudan statistics is not in the same league with Super-K.

Recommendation

Results were presented from the Soudan II detector which is now approaching an exposure of 5 kiloton-years. The data are of good quality and have adequate track and particle ID information. In the proton decay area they are not competitive with Super-K. In the atmospheric neutrino field the data are consistent with the Super-K results (and the earlier IMB data) and thus are of interest. The principal reason for continued operation is to provide a smooth transition with an operating detector into the era when the MINOS experiment begins and the first accelerator neutrinos are detected in the Soudan mine. The cost of operating the detector is on the order of 180 K$ per year and at the least one must anticipate 4 additional years of operation. The request seems reasonable but it might be more appropriate that these costs be charged against the MINOS budget rather than to non-accelerator physics.

The Soudan II detector is working well and is the only operating fine grain detector in the world. It is making a contribution in searches for some particular decay modes of the proton (e.g. $p \to nK^+$, and K^0 two body modes). In addition, the experiment has produced new verification of the atmospheric neutrino anomaly. This experiment should certainly be supported to finish the present program, which will probably take another year or so.

APPENDIX A

Scientific Assessment Group for Experiments in
Non-Accelerator Physics (SAGENAP):

Barry Garish, Caltech

Gene Beier, University of Pennsylvania

Tom Gaisser, University of Deleware

Jordan Goodman, University of Maryland

Cy Hoffman, Los Alamos National Laboratory

Robert Lanou, Brown University

Adrian Melissinos, University of Rochester

Leslie Rosenberg, Massachusette Institute of Technology

Hank Sobel, University of California at Irvine

Trevor Weeks, Smithsonian Institute

DOE:
P.K. Williams

NSF:
Patricia Rankin

NASA:
Vernon Jones

APPENDIX B

Agenda

Scientific Assessment Group
for Experiments in Non-Accelerator Physics

Holiday Inn
Gaithersburg, MD
April 14-16, 1998

<u>Tuesday April 14</u>

8:30a.m. Executive Session

9:00 a.m. Pierre Auger Project Proposal

9:00 a.m.	Overview	J. Cronin
9:15 a.m.	Auger Science	
J. Mathews		
9:35 a.m.	Auger Detector Design	C. Pryke
10:15 a.m.	Auger R&D	D. Nitz
10:35 a.m.	Work plan	J. Beatty
10:55 a.m.	International Organization	A. Etchegoyen
11: 10 a.m	Wrap up	J. Cronin
11 :30 a.m.	Discussion	

12:30 p.m. Lunch

1:30 p.m. GLAST Project Proposal

1:30 p.m.	Overview	E. Bloom
2:00 p.m.	NASA Science Interest in GLAST	N. Gehrels

2:30 p.m.	GLAST Physics	S. Ritz
3:10 p.m.	Baseline GLAST Instrument	B. Atwood
3:50 p:m.	Break	
4:05 p.m.	GLAST R&D Progress	R. Johnson
4:45 p.m.	NASA/Astrophysics Perspective	P. Michelson
5:25 p.m.	SLAC's Role in GLAST	D. Leith
5:40 p.m.	Management, Budget, Schedule, Conclusions	E. Bloom

6:00 p.m. Discussion

7:00 p.m. Adjourn

<u>Wednesday. April 15</u>

8:30 a.m.	Executive Session	
9:00 a.m.	Further Discussion on Auger	
10:00 a.m.	Further Discussion on GLAST	
11:00 a.m.	Super-Kamiokande "Maintain and Upgrade" Proposal	E. Kearns
12:00 p.m.	Discussion	
12:30 p.m.	Lunch	
1:30 p.m.	ICARUS Proposal Presentation	D. Cline
2:00 p.m.	Discussion	
2:15 p.m.	VERITAS Project "Heads-Up"	
2: 15 p.m.	Overview	J. Gaidos
2:30 p.m.	Physics	M. Catanese

2:45 p.m.	Hardware	S. Swordy
3:00 p.m.	Soudan II Phase down: Progress and Potential	A. Mann
3:00 p.m.	Physics with Soudan II	A. Mann
3:25 p.m.	Soudan Detector for Minos: Utility, Game plan.	E. Peterson

Further Discussion on Super-K, ICARUS, Soudan II

4:30 p.m. Executive Session

6:00 p.m. Adjourn

Thursday. April 16

9:00 a.m.	Executive Session
12:00 p.m.	Lunch
1:00 p.m.	Executive Session
4:00 p.m.	Adjourn