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REPORT OF THE HEPAP SUBPANEL ON MAJOR DETECTORS IN NON-ACCELERATOR PARTICLE PHYSICS

MAY 1989

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Thomas Fields, Argonne National Laboratory
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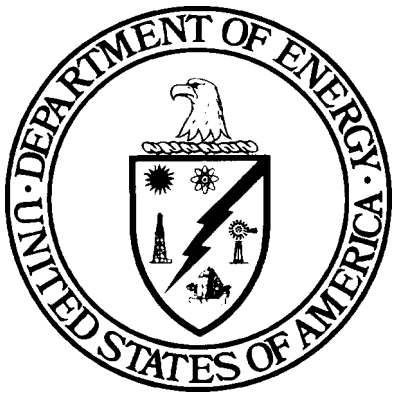

U.S. Department of Energy
Office of Energy Research
Division of High Energy Physics
Washington, D.C. 20545

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
LABORATORY FOR NUCLEAR SCIENCE
CAMBRIDGE, MASSACHUSETTS 02139

June 6, 1989

Dr. Robert O. Hunter, Jr.
Director, Office of Energy Research
U. S. Department of Energy
Washington, DC 20545

Dear Bob,

I am writing to transmit the report of the Subpanel on Major Detectors in Non-Accelerator Particle Physics. The report was approved by the High Energy Physics Advisory Panel (HEPAP) at its meeting at Fermilab on May 16th and 17th.

As is brought out in their report, the Subpanel has reviewed three major research proposals which had been submitted to the Department of Energy and/or the National Science Foundation: DUMAND II, GRANDE and Fly's Eye Upgrade. Results of the review, with evaluations and recommendation for the funding agencies, are included in the report.

I believe the Subpanel's report should be very helpful in advancing the U. S. High Energy Physics Program in this growing field at the interface between particle physics and astrophysics.

Yours Sincerely,

Francis E. Low
Chairman of HEPAP

Yale University

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Campus address:
502-4 J.W. Gibbs Laboratory

May 31, 1989

Professor Francis Low
Department of Physics, 6-301
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, Massachusetts 02139

Dear Francis;

This note is meant as a formal letter of submission covering the report of the "HEPAP Subpanel on Major Detectors" which HEPAP has now received and accepted.

The almost complete consensus of the strong minded senior physicists who made up the Panel that mark the conclusions of the report, is to me edifying and suggests that those conclusions are in accord with the considered opinion of our community.

Sincerely

Bob

Robert K. Adair
Sterling Professor of Physics

Report of the HEPAP Subpanel on Major Detectors in Non-Accelerator Particle Physics

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Germantown, March 20-21, 1989

Introduction

The subpanel on Major Detectors in Non-Accelerator Particle Physics was formed in February 1989 as the result of a letter from Robert Hunter, Director, Office of Energy Research, to Francis Low, Chairman of HEPAP. A copy of the letter is included in the Appendix to this report. The letter referred to the previous report of HEPAP Subpanel on High Energy Gamma Ray and Neutrino Astronomy which had found that several groups of scientists were working on promising new ideas and proposals in non-accelerator high energy physics and astrophysics; this report recommended that a panel be formed to evaluate large projects in these areas of science when specific proposals were received by the funding agencies. In concurring with the recommendation, the request to establish this new Subpanel included the following specific charge: Within the context of changing world wide high energy physics activities and opportunities, review as necessary and evaluate the following major research proposals which have been submitted to the Department of Energy and/or to the National Science foundation: DUMAND II, GRANDE, and the Fly's Eye Upgrade.

In addition to reviewing the written proposals and their addenda, the Subpanel has held meetings and heard presentations from proponents of the three proposed projects at the Department of Energy, Germantown, Maryland on March 20-21, 1989. A copy of the agenda is included in the appendix.

Executive Summary

In light of recent progress in particle physics and astrophysics, it is clear that the understanding of either the early universe or particle physics at very high energies, requires insights from both disciplines. The programs discussed here represent endeavors to answer questions raised in this important interface between particle physics and astrophysics.

The scientific programs encompassed by the DUMAND II, GRANDE, and High Resolution Eye proposals offer at least two outstanding possibilities for major discovery and/or opening a major new field. In addition, a variety of programmatic research would be undertaken. These two especially interesting possibilities are:

- The production by astrophysical point sources of extensive air showers (EAS) having muon content inconsistent with current understanding of the interactions of very high energy primary photons.
- The opening of a new field of extra-solar high-energy neutrino astronomy, with special attention to possible point sources, as a unique probe of astrophysical processes generating high energy hadrons.

The new proposals considered by the Subpanel represent substantial escalations in the proposed cost of U.S. cosmic ray facilities. Though each proposal was accepted by the Subpanel as describing an attractive and technically feasible program that would most likely contribute substantially to our understanding of nature, the Subpanel was charged with the requirement of weighing the new level of costs of these programs against other possibilities for major progress in particle physics. With this constraint in mind:

The Subpanel found the DUMAND II proposal attractive and it seems to be affordable. However, the collaboration group does not now appear to us to be sufficiently strong. Assuming that the collaboration can be strengthened and that a detailed technical review establishes costs to the DOE which are reasonably consistent with the proponents' estimates, with one dissent, the Subpanel recommends that the DUMAND II proposal be funded.

The Subpanel does not find that GRANDE is likely to conduct neutrino astronomy substantially more effectively than DUMAND II but GRANDE would likely offer a unique capability in the area of EAS detection. How-

ever, GRANDE is very expensive and before advising a commitment at this level of cost, the Subpanel would prefer to have the results of present major efforts in these areas to better assess the importance of major further investments. Hence, the Subpanel unanimously recommends that the GRANDE proposal not be approved at this time. However, the Subpanel encourages further efforts to develope this impressive new technique on a smaller scale.

The Subpanel feels that the magnitude of the proposed Fly's Eye Upgrade construction project is large in proportion to the amount of qualitatively new information likely to be gained. Hence, the Subpanel recommends unanimously that the High Resolution Eye proposal not be approved. However, the Subpanel encourages the exploitation of the high resolution technique on a smaller scale.

1 General Considerations

In light of the progress in particle physics and astrophysics in the last two decades, the importance of substantial efforts exploiting the deep relations between these two disciplines has become evident. The very small distances and complementary high energies now important in particle physics reflect conditions in the early universe where the temperatures and densities corresponded to those high energies and small distances. Hence, any understanding of either the early universe or high energy particle physics, requires the understanding of both. Elementary particles may contribute to the dark matter that accounts for most of the mass of the universe. And there appear to be natural accelerators in space that generate very high energy conventional particles – and perhaps new particles. The acceleration mechanisms are of astrophysical interest while the particles may illuminate questions in particle physics. The programs discussed here represent endeavors to answer questions of this kind raised in this important interface between particle physics and astrophysics.

The scientific program encompassed by the DUMAND II, GRANDE, and High Resolution Eye proposals offers at least two outstanding possibilities for major discovery and/or opening a major new field. In addition, a variety of programmatic research would be undertaken. These two possibilities are:

• The production by astrophysical point sources of extensive air showers (EAS) having muon content inconsistent with current understanding of the interactions of very high energy primary photons.

• The opening of a new field of extra-solar high-energy neutrino astronomy, with especial attention to possible point sources, as a unique probe of astrophysical processes generating high energy hadrons.

The first possibility is suggested by recent observations by EAS arrays, and the second by observations of gamma-ray induced showers together with models for the acceleration processes. The more programmatic research includes studies of point sources of gamma rays of various energies, studies of EAS primary composition and energy spectrum, and topics in particle physics including the search for neutrino oscillations.

The new proposals considered by the Subpanel represent substantial escalations in the proposed cost of U.S. cosmic ray facilities. Though each

proposal was accepted by the Subpanel as describing an attractive and technically feasible program that would most likely contribute substantially to our understanding of nature, we were charged with the requirement of weighing the new level of costs of these programs against other possibilities for major progress in particle physics.

The two following sections state the *Recommendations* of the Subpanel concerning the disposition of the three proposals and a summary of the *Discussion* of the Subpanel. In the cases of GRANDE and of the Fly's Eye, the recommendations represent a consensus; one subpanel member disagreed with the recommendation of the majority concerning DUMAND II.

2 Recommendations

2.1 DUMAND II

The Subpanel found the DUMAND II proposal attractive and it seems to be affordable. However, the group does not now appear to us to be strong enough to carry out the proposed work. Assuming that this reservation can be addressed satisfactorily, and that a detailed technical review establishes costs to DOE which are reasonably consistent with the proponents' estimates, the Subpanel recommends that the DUMAND II proposal be funded.

A Dissent from one Subpanel Member

I consider that there are technical difficulties faced by DUMAND II which are not solved by the proposed design. Moreover, there is no assurance that neutrino sources exist, or that if they exist the DUMAND II detector would be sufficiently sensitive to detect them. Hence, DUMAND II should not be approved.

2.2 GRANDE

The Subpanel does not find that GRANDE is likely to conduct neutrino astronomy substantially more effectively than DUMAND II; and GRANDE is considerably more expensive. It is in the area of EAS detection that GRANDE would be most likely to offer a unique capability. However, before advising commitment to an EAS detector of this cost, the Subpanel would prefer to have the results of present major efforts (*e.g.* the CASA/MIA arrays and the European programs) to learn more about point sources of EAS in the TeV region and above, including the important issue of muon content.

Hence, the Subpanel recommends that the GRANDE proposal not be approved at this time. However, it is impressed by the new EAS technique which this proposal introduces, and encourages further efforts to develop it.

2.3 High Resolution Eye

The proponents estimate the total new equipment cost (in 1988$) to be almost $20,000,000 exclusive of escalation and contingency. The Subpanel feels that this magnitude of the proposed construction project is large in proportion to the amount of qualitatively new information likely to be gained.

Hence, the Subpanel recommends that the High Resolution Eye proposal not be approved. It is strongly supportive of the continued operation of Fly's Eyes I and II, and is interested in the possibility of performance upgrades more modest than those in the present proposal, including the possibility of exploiting the high resolution technique on a more modest scale.

3 Discussion

Although this report of the discussions is meant to provide a broad – and hopefully useful – overview of the members concerns, the following does not necessarily represent well-argued consensual positions of the Subpanel but reflects positions and emphases of the different members.

The last subsection, labeled "Further Comments", presents positions taken, and comments made, by Subpanel members that are directed to general problems that are perceived rather than to any of the three proposals. Again, these statements do not generally represent consensual positions adopted by the Subpanel as a body.

3.1 DUMAND II

This group proposes to deploy nine strings of (24) 16 inch diameter phototubes, with characteristic spacings of 10 m for the phototubes and 40 m for the strings, under 4.7 km of sea water off Hawaii. The primary goal is detection of astrophysical point sources of neutrinos. Here the ocean serves both as shield and as the active element of the detector.

Other capabilities include studies of atmospherically produced muons and neutrinos but not of EAS. The proposed equipment cost is (in 1988$) $9,100,000 exclusive of contingency, escalation, and some additional oceanographic support. However, it seems likely that there will be substantial foreign participation and the cost to the DOE is estimated as less than $5,000,000.

The DUMAND II proposal has certain strengths and advantages:

The proposed array is extendable to much larger size if further observations and the array performance warrants this.

As a high energy neutrino detector, DUMAND II will be able to see almost all of the sky including Cygnus X-3 and Hercules X-1 and its cost per unit sensitivity is reasonable.

Assuming, with the proponents, that a reasonable fraction of the total cost can be borne by non-U.S. high energy physics sources, the magnitude of expenditure involved is not excessive for a next step in the attempt to observe high-energy extraterrestrial neutrinos. We consider the extensive international participation (from Japan, Germany, and Switzerland) as an important strength.

The project has evolved over a long period and significant progress in research and development has been achieved.

The proposed approach has disadvantages and we see some problems:

At present the group does not appear to be nearly strong enough to carry out the proposed work. Major growth seems to be needed, particularly in engineering and project management, and in young Ph.D. level talent able to work intensively on commissioning the array and extracting the results. We are concerned over the loss of the Irvine and Purdue groups and hope that a reconsideration of those decisions may take place. Also, with the toll of years spent on the past efforts, and the length of time required to finish the project, special thought must be given to a continuity of administrative leadership into the next decade. Though the planned contribution by the University of Hawaii in engineering help is most attractive, this does not negate the importance of a Hawaii investment in faculty positions for principals. Eventually, the DUMAND collaboration should be sufficiently strong so as to support two independent data analyses.

Typical event signals involve a few photoelectrons distributed among few phototubes. When the effective tube background rates from K^{40}, bioluminescence, and ordinary dark currents are considered, accidental coincidence rates may be troublesome and extra tubes could greatly increase the margin of safety. Hence, we suggest that a somewhat higher density of phototubes be considered, albeit at additional cost. Perhaps a contingency string should also be included in the ultimate plans.

The estimated fluxes of point-source neutrinos are predicated from measured gamma intensities and a large neutrino per gamma or λ factor. The value of λ is uncertain and the existence of point sources of very high energy photons has not been established beyond doubt. If $\lambda < 10$ *e.g.*, the detection of neutrino point sources may be beyond the sensitivity of the DUMAND II system. (The same comment applies to the neutrino aspect of the GRANDE proposal.)

The efficacy of the water Čerenkov technique has been proven through the success of IMB and Kamiokande. However, though advances in oceanographic techniques allow a level of optimism – and we are impressed with the level of cooperation and support elicited from the oceanographic community – the problems of establishing and maintaining apparatus in 4000 meters of water remain a challenge.

Operations at sea will surely pose difficulties and the project must be

prepared for deployment problems that could lead to overruns in time and cost. Any funding agreement that is adopted should contain milestones and provisions for regular reviews. The collaboration needs to be sure that proper guarantees are in place for the use of an ROV in deployment and maintenance. We also suggest that it might be desirable to analyze the operation of the first three strings before installing the last six.

3.2 GRANDE

This group proposes to deploy a 250 × 250 × 70 m deep opaque plastic bag containing pure water in an Arkansas water-filled quarry at 175 meter elevation. The bag would contain four optically isolated layers with planes of 8 inch diameter phototubes (7000 total) on a 6 meter horizontal grid. The top layer would be sensitive to combined electromagnetic, hadronic, and muonic EAS signals, while upward-facing phototubes in a lower layer would be sensitive primarily to downward-going muons. Downward-facing tubes in the lower layers could observe upward-going muons from interactions of neutrinos penetrating the Earth. The proponents estimate a total equipment cost, exclusive of escalation and contingency, of (in 1988$) $30,500,000.

For EAS studies, the GRANDE proposal has the following advantages:

GRANDE introduces a qualitatively different type of EAS detector, sensitive to all EAS components, with a very large active area and a large dynamic range from $\approx 10^{13}$ eV to $\approx 10^{16}$ eV. In these measurements, the unique 'total' calorimetry of GRANDE plays an important role though the light yield may be dominated by the contribution of the muons in the showers making it difficult to isolate the electromagnetic component.

A measurement of the spectrum from a source over the range from 5 TeV to 500 TeV from a single instrument might discriminate between photon radiation from electron acceleration and from π^0 decay.

GRANDE has the ability to segregate EAS with significant muon content, owing to the very large active muon detection area or, conversely, to study the muon-electron composition. This would be an advantage for study of conventional astrophysical point sources of photon-induced EAS.

GRANDE has the following disadvantages as an EAS detector:

For small (a few TeV to a few tens of TeV) EAS, the proposed location near sea level would raise the threshold relative to location at high elevation and would be subject to greater fluctuations in detected signal for fixed primary energy. For example, the proponents stated at the review that the Cygnus array has a lower EAS threshold than the much larger CASA/MIA array now under construction, because of its location at 7000 foot rather than 5000 foot elevation.

For large (EeV) EAS, the limited spatial extent (250 m square) of the array complicates the analysis because the core location (typically outside the 250 meter square area) would not be as precisely determined as would be possible in an array of greater spatial extent. This limitation could be reduced by supplementing GRANDE with a conventional air shower array outside the lake area.

At present there is no experience in building or deploying a very large light-tight, water-tight, expensive bag extending to great depth outdoors.

As a detector of extraterrestrial neutrinos GRANDE would have the following advantages relative to DUMAND II:

In a typical signal, GRANDE would have more photoelectrons distributed over more phototubes.

GRANDE would have slightly greater effective viewing area for Southern Hemisphere sources.

Relative to DUMAND II, GRANDE would have some disadvantages as a neutrino detector:

GRANDE must reject the huge background of muons produced in the atmosphere. Though the backround-to-signal level of about 10^{11} seems tractable, it is difficult to be sure that all possible backgrounds are considered at that level.

GRANDE cannot be easily extended to a much larger size.

The sky coverage of GRANDE is more restricted owing to its location at higher latitude and its smaller nadir angle cutoff. In particular, there are

serious restrictions on the viewing of some of the better known Northern Hemisphere sources.

The Subpanel was impressed by the size, capability, and strong committment of the GRANDE group of physicists, but its proposed equipment cost to U.S. high energy physics sources of $30,500,000 is very large. We note that this cost would be many times greater than the U.S. cost of DUMAND. Perhaps this group might find it possible to design a more modest EAS detector that might be later expanded. We note that the Irvine and Maryland contingents are also deeply involved in a proposed multi-year continuation of IMB and also in the continued operation of the Cygnus array.

The concept of GRANDE is novel and clever, and GRANDE may become a powerful new tool for EAS studies. HEPAP might well reexamine the fields of neutrino astronomy and, especially EAS, addressed by the GRANDE proposal in two years, after information from CASA/MIA, Cygnus, and other programs, better defines the problems to be addressed.

3.3 High Resolution Eye

The Fly's Eye system is ingenious, unique, and effective. The Utah group proposes to replace the existing Fly's Eye I and II with 45,000 phototubes distributed among (174) 2 meter diameter mirrors divided among three locations at corners of an equilateral triangle with 17 km sides. The goal is to improve the angular resolution per phototube from 5.5×5.5 to 1×1 degree. This would reduce the background from other (extended) sources of light in the night sky, extending the effective area for detection of point sources of photons with energies of about 10^{17} eV by about one order of magnitude, improve the resolution of air shower maxima by about one-half an order of magnitude, and extend the coverage of the energy spectrum from a limit of about 10^{20} eV to about 10^{21} eV where it might be possible to see the Greisen cut-off. The physics, an extension of the present Fly's Eye program, includes the study of the energy spectrum, composition, isotropy, and air cross section of ultra high energy cosmic rays, and possibly, searches for extraterrestrial neutrinos near the horizon.

The Subpanel commends the success of the Utah group in bringing the

original Fly's Eye idea to fruition as an active program providing valuable information that is complementary to that obtained in more conventional EAS arrays. It feels that the proposed High Resolution Eye is a reasonable extrapolation in technology to the original device – though appreciably greater in scope and complexity – and it regards the proposed research program as interesting. However, the Subpanel feels that the magnitude of the proposed construction project is large in proportion to the amount of qualitatively new information likely to be gained, and large for the size of the Utah group. Indeed, much data from Fly's Eyes I and II remain to be collected and analyzed and the Subpanel suggests that further integration of this program with the CASA/MIA effort might be very profitable. The present Fly's Eye has now been running only for about a year – after the problems with the efficiency of the mirror system were solved – and there is much to do with that apparatus. For example, the observation of muons and electrons in coincidence with air fluorescence events of energies near $2 \cdot 10^{17}$ eV is of special interest in providing information about EAS and about the composition of the incident radiation, and in providing improved calibrations of surface measurements.

The Subpanel does favor the possibility of more modest improvements of the resolution and/or other aspects of the detector at a level of about one or two million dollars. The group is small and does not include any outside collaborators. We suggest that any large expansion of this project will require a broadening of the group.

3.4 Further Comments

We hold that it is important that there exists a high level of continuity in DOE/NSF scientific policies concerning the interface of particle physics and astrophysics. Although there are legal and administrative constraints on continuing committees and subpanels which we recognize, we consider that the continuity in policy which we believe is important must derive from a continuity in advice. We consider that in the recent past the DOE/NSF policies have shown the requisite consistency which has followed from a consistency in the form and substance of advice. We hope that the program officers of the DOE and NSF will be able to find ways of insuring such continuity into the future.

We consider that the general make up of advisory subpanels such as this, including both high-energy physicists who are interested primarily in non-accelerator experiments, often with strong astrophysical aims, and physicists whose interests are focused on accelerator experiments directed towards the center of particle physics is useful and should be continued.

All of the proposed experiments use a very large number of photomultipliers. The DOE should encourage the purchase of a large portion of these tubes from U.S. manufacturers rather than from abroad so as to support and extend a viable photomultiplier technology base in the U.S.

An acceptance of these programs would require a major escalation of funding in this field. We consider it important that the addition of new large programs does not excessively constrain the support of smaller programs, which are also important and often give very good value for the dollar.

Department of Energy
Washington, DC 20585

FEB 01 1989

Professor Francis E. Low, Chairman
High Energy Physics Advisory Panel
Laboratory for Nuclear Science
Massachusetts Institute of Technology
77 Massachusetts Avenue, Room 6-301
Cambridge, MA 02139

Dear Professor Low:

The High Energy Physics Advisory Panel's Subpanel on High Energy Gamma Ray and Neutrino Astronomy found that several groups of scientists were working on promising new ideas and proposals in non-accelerator high energy physics and astrophysics. In their report, the Subpanel recommended that a panel be formed to evaluate large projects [such as the Deep Underwater Muon and Neutrino Detector (DUMAND) and the Gamma Ray and Neutrino Detector Experiment (GRANDE)] in these areas of science when specific proposals were received by the funding agencies. We agree with the conclusion and concur in the recommendation. Accordingly, we request that the High Energy Physics Advisory Panel establish an ad hoc subpanel with the following charge:

> Within the context of changing worldwide high energy physics activities and opportunities, review as necessary and evaluate the following major research proposals which have been submitted to the Department of Energy and/or to the National Science Foundation: DUMAND II, GRANDE, and FLY's EYE UPGRADE.
>
> In carrying out this review, it will be helpful if the subpanel would specifically:
>
> 1. Evaluate the quality, technical feasibility, and scientific significance of these research activities.
>
> 2. Assess the potential of these research activities for making scientifically important contributions to the overall high energy physics program.

3. Recommend technical priorities taking into account the scientific opportunities of the national program of high energy physics.

Assistance and coordination will be provided by the Physics Research Branch of the Department of Energy's Division of High Energy Physics via P. K. Williams and Louis Voyvodic, as well as by the Elementary Particle Physics Branch of the National Science Foundation via David Garelick and Robert Ellsworth.

I would appreciate having the results of this study by about May 1, 1989.

Sincerely,

Original Signed By

Robert O. Hunter, Jr.
Director
Office of Energy Research

cc:
M. Bardon, NSF

AGENDA

HEPAP SUBPANEL ON MAJOR DETECTORS IN NON-ACCELERATOR PARTICLE PHYSICS

March 20-21, 1989

Department of Energy
Germantown, MD 20874

Monday, March 20, 1989 — North Entrance, Room A-410

8:45 a.m.	Executive Session
9:15	DUMAND II Presentation
10:45	Coffee Break
11:00	GRANDE Presentation
12:30 p.m.	Lunch
1:30	FLY's EYE UPGRADE Presentation
3:00	Coffee Break
3:15	Questions and Discussion
4:30	Executive Session
5:30	Adjourn

Tuesday, March 21, 1989 — South Entrance, Room E-301

9:00 a.m.	Executive Session
3:00 p.m.	Adjourn

DUMAND II

Proposal to Construct a Deep-Ocean Laboratory for the study of High Energy Neutrino Astrophysics and Particle Physics

DUMAND Collaboration

July 27, 1988

P. Bosetti
Technische Hochschule Aachen, West Germany

P.K.F. Grieder
University of Bern, Switzerland

B. Barish, J. Elliott
California Institute of Technology, USA

J. Babson, R. Becker-Szendy, J.G. Learned, S. Matsuno, D. O'Connor, A. Roberts, V.J. Stenger, V.Z. Peterson, G. Wilkins
University of Hawaii, USA

O.C. Allkofer, P. Koske, M. Preischl, J. Rathlev
University of Kiel, West Germany

T. Kitamura
Kinki University, Japan

H. Bradner
Scripps Institute of Oceanography, USA

K. Mitsui, Y. Ohashi, A. Okada
Institute of Cosmic Ray Research, University of Tokyo, Japan

J. Clem, C.E. Roos, M. Webster
Vanderbilt University, USA

U. Camerini, M. Jaworski, R. March, R. Morse
University of Wisconsin, USA

August 31, 1988

Proposal to Construct the GRANDE Facility for the Study of Astrophysical Sources and High-Energy Particle Interactions

University of California, Irvine
C. Bratton, W. Gajewski, W. Kropp, C. McGrew, M. Nelson, L. Price, F. Reines, J. Schultz, H. Sobel, R. Svoboda, G. Yodh

University of Hawaii
J. Learned

University of Maryland
J. Goodman, T. Haines

Los Alamos National Laboratory
D. Nagle, M. Potter

Columbia University
R. Novick, A. Szentgyorgyi

Purdue University
J. Gaidos, F. Loeffler, G. Sembroski, C. Wilson

University of Wisconsin
R. March

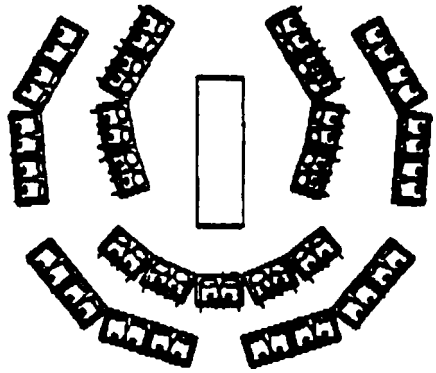
University of Arkansas, Little Rock
A. Adams, R. Bond, L. Coleman, A. Rollefson, D. Wold

University of Arkansas, Fayetteville
M. Lieber

Louisiana State University
M. Cherry, J. Wefel

Drexel University
C. Lane, R. Steinberg

Warsaw University
D. Kielczewska



HIGH RESOLUTION EYE