

## EXECUTIVE SUMMARY.

### Recommendations.

The Panel has reviewed four different proposals for Major Research Equipment (MRE) funding. In its deliberations and in its recommendations, the Panel has been fully cognizant of the very high standards the proposals must meet to qualify for these funds. Even within this constraint, we find that the physics aims of all four proposals meet the criterion of being in the "must do" category. They all address the most fundamental questions in particle physics today and have the potential to revolutionize our thinking about the workings of our universe. Our different recommendations for each proposal reflect other factors we have taken into account besides the physics promise, especially potential competition from other programs and experimental state of readiness.

MECO. We recommend that the Foundation proceed with the funding of the MECO proposal. In our view, MECO is in the strongest position, at this time, to make a significant impact on particle physics. MECO addresses the issue of lepton conservation, one of the most important questions in particle physics today. A positive result from that experiment would have a profound effect on our understanding of the fundamental constituents of matter and of the forces that govern their behavior.

KOPIO. Even though we recognize the very high physics potential of KOPIO, we do not recommend funding at this time. There is another group interested in performing an experiment at Fermilab with goals similar to KOPIO, but using different methods. We believe that the issues of optimum location and method as well as of manpower have to be resolved, preferably by the two groups, before a firm funding commitment is made. We strongly urge the two interested groups to work together towards a single proposal to perform this crucial experiment. The EPP Program of the NSF should follow this process closely with the view to consider the resulting proposal for possible future MRE funding.

BTeV. The Fermilab management as yet has not decided whether to approve the BTeV experiment, that decision awaiting a thorough evaluation of the scientific case, technical feasibility, and proponents' cost estimate. We recommend that the Foundation's decision on this proposal be delayed until the results of that review are available and a new proposal, reacting to any significant comments and suggestions from that review, is submitted.

Muon Storage Ring R&D. The muon storage ring R&D proposal is still in too preliminary a state to warrant commitment of significant funds. However, the impact of this R&D on particle physics, if successful, would be so significant that we recommend that some initial funds be provided to allow a broader community participation in the work leading towards generation of a mature R&D proposal.

### Physics Impact.

We now elaborate on the potential physics impact of the proposals submitted. The Standard Model of Particle Physics is remarkably successful, insofar as it has ability to explain a wide range of observed phenomena, spanning an energy from fractions of eV, in atomic physics, to hundreds of GeV in high energy colliders. But we know that this cannot be the ultimate theory because it is internally inconsistent at energies beyond current reach and it leaves unanswered deep questions about the physical world.

It is generally recognized that there are several areas of experimental investigations which address the key unanswered questions in the

Standard Model and whose successful pursuit would very likely lead to major breakthroughs in our effort to develop the ultimate theory. In the forefront of these questions are the origin of mass, the nature of electroweak symmetry breaking, the origin of CP violation, the validity of lepton and baryon conservation, and understanding the properties of neutrinos and their mixing matrix.

The proposals we have reviewed all address one or more of these important questions. Therefore, they all have the capability of making an enormous impact on our understanding not only of particle theory but also of the evolution of our universe. Accordingly, the physics goals of the four proposals belong in the "must do" category.

The MECO experiment is a bold and unique initiative that directly addresses one of these fundamental questions of contemporary physics: the nature of lepton flavor and whether or not it is absolutely conserved.

Within the Standard Model, lepton flavor conservation is an "accidental" symmetry. It is supported experimentally by the fact that all the previous searches for lepton violation have yielded null results. But we know of no fundamental reason why this symmetry should not be violated by physics beyond that described by the current Standard Model. Indeed, most extensions of the Standard Model, such as those based on leptoquarks, compositeness, or supersymmetry, lead to the conclusion that lepton flavor conservation is not exact. They predict, for example, direct conversion of muons to electrons when muons are captured in an orbit around a nucleus. The rate  $R$  at which this process occurs can be characterized by the rate of muon to electron conversion relative to the rate at which muons convert to neutrinos. The predictions for  $R$  span a wide range of values, depending upon the details of the various theories, but some of them give values of  $R$  as large as  $10^{-14}$ .

The current limits on the lepton number violation, from muon to electron conversion, and from muon and  $K$  decays, are around  $10^{-12}$ . Thus the MECO experiment, designed to reach a sensitivity of  $10^{-16}$ , is of great interest because it offers the only possibility in the foreseeable future to probe this question down to these levels. A positive MECO result, showing that charged lepton flavor symmetry is not exact, in effect that "muonness" and "electronness" are not absolute qualities of nature, would be a stunning result, the kind of news that attracts attention of media outlets all over the world.

Two of the proposals, KOPIO and BTeV, address the topic of CP violation with totally different techniques. The subject of CP violation is of immense importance to particle physics and cosmology. It implies that the laws of nature, at their deepest level, depend on the direction of time. CP violation is a profoundly quantum mechanical effect, with deep philosophical implications for our view of space and time.

The subject has a more practical side as well. Some amount of CP violation seems necessary to explain the observed predominance of matter over antimatter which, according to our current understanding, was established in the very early universe. Whether or not the Standard Model contains enough is a matter of some debate, but the present consensus is that more is necessary. This suggests that some new source of CP violation is required to make up the difference.

These considerations lead to the conclusion that CP violation should be studied in all its forms. First, because it might well point to new physics beyond the Standard Model, and second, because it was an essential ingredient in the cosmic stew that made up the early universe.

In particle physics, CP violation can be studied through the production and decay of K and B mesons. We believe that it is essential to make precise measurements of CP violation in both systems. A direct comparison of the results will test the Standard Model and might possibly reveal the new physics that lies beyond it.

Present-generation experiments have demonstrated the existence of CP violation in the K system, but they have not yet found it conclusively in the B system. Nor have they made an accurate measurement of the CP-violating phase, a key parameter whose precise measurement will shed light on the origin of CP violation. It is essential to measure this phase as accurately as possible in both systems. The KOPIO experiment seeks to perform such a measurement in the K system; the BTeV experiment in the B system.

The fourth proposal we considered was an Expression of Interest (EOI) for R & D towards a Neutrino Factory Based on a Muon Storage Ring and eventually towards a Muon Collider. This EOI is the first step in a long range program that, if successful, would open up new exciting experimental possibilities in high energy physics. Its success would have enormous potential for addressing several of the key questions in particle physics: the nature of mass, the mechanism of electroweak symmetry breaking, the nature of neutrinos.

The ultimate goal of this proposal is to develop technology to build a muon collider which may well be the optimum path to the high energy frontier beyond that provided by the LHC. There is little doubt in the particle physics community that extending the energy reach is the key to our further progress in the field. Regardless of what LHC may find, the further elucidation of these potential discoveries will require new high energy machines, capable of complementary investigations.

En route to this ultimate goal, there is another and only slightly less challenging objective: the construction of a muon storage ring to be used as a source of neutrinos. Such a storage ring would give well collimated, high intensity fluxes of neutrinos which would significantly extend the range of possible investigations in neutrino physics. It would allow detailed studies of neutrino masses and neutrino mixing matrix, essential ingredients in our efforts to understand the ultimate nature of matter.

#### DETAILED DISCUSSION OF THE FOUR PROPOSALS.

We address next each proposal in turn, following the guidelines provided to us by the NSF, namely:

- I. What is the intellectual merit of the proposed activity?
  - A. How important is the proposed activity to advancing knowledge and understanding within its own field or across different fields?
  - B. How well qualified is the team of proposers to conduct the project?
  - C. To what extent does the proposed activity suggest and explore creative and original concepts? How does the proposed activity compete with similar projects?
  - D. How well conceived and organized is the proposed activity?
  - E. Are the proposed costs and schedules reasonable at this stage of the project?
  - F. Is there sufficient access to necessary resources?

II. What are the broader impacts of the proposed activity?

A. How well does the activity advance discovery and understanding while promoting teaching, training, and learning?

B. How well does the proposed activity broaden the participation of underrepresented groups (e.g., gender, ethnicity, disability, geographic, etc.)?

C. To what extent will it enhance the infrastructure for research and education, such as facilities, instrumentation, networks, and partnerships?

D. Will the results be disseminated broadly to enhance scientific and technological understanding? What may be the benefits of the proposed activity to society?

The MECO Experiment

(Section deleted)

The KOP10 Experiment.

(Section deleted)

The BTeV Experiment.

(Section deleted)

EOI for R&D for MUON Storage Ring and Muon Collider

SUMMARY.

This proposal is the first step in a long range program to study feasibility of cooling and storing an intense beam of muons. The long range goal is a muon muon collider in the energy range of several TeV. A very interesting intermediate step would be a muon storage ring used as a source of a well understood, well collimated, and very intense neutrino beams.

Achievement of these goals would open up new unique opportunities in the experimental high energy physics. It may well be that muon muon collisions will offer the most fruitful way to extend the energy frontier in the future; extending this frontier has been historically the key to progress in the field and we believe that this will remain the case in the future. The many ideas put forth to extend the Standard Model will be tested most effectively at high energies. The LHC may well provide some answers but undoubtedly further elucidation of its potential discoveries will require new machines capable of complementary investigations.

The muon storage ring would be a "neutrino factory" providing new opportunities in neutrino physics. Elucidation of the questions of neutrino mass and neutrino mixing matrix, key questions in particle physics today, may not be possible without new ways of making neutrino beams. The muon storage ring, by virtue of its intense collimated neutrino beams, would offer the possibility of doing truly long baseline experiments, with the neutrino source on one continent and

detector(s) on other continents. This would not only allow unprecedented new measurements to be made but would also stir and excite the imagination of the whole scientific community and the public at large.

The R&D proposed explores original and highly challenging concepts. The risks are great and success is not guaranteed; the potential immense payoff, however, argues for support at this time. In this R&D program new ideas have to be investigated in areas that traditionally have been outside of the domain of particle physics. Thus it is important to embark on interdisciplinary collaboration; universities are ideal places where such contacts are made and interdisciplinary research thrives.

A number of new university groups have expressed interest in joining this effort. We recommend increased support for this work which should allow a larger and broader community to get involved. The goal of this effort should be generation of a formal R&D proposal on a time scale of 1-2 years which would clearly delineate the activities, the resources, and the time needed to answer the key questions. Since this work will include DOE and NSF supported laboratories and universities, close interagency cooperation will be needed in supporting and overseeing this program. A well thought out R&D proposal would be a very strong candidate for future MRE funding.

#### I. Intellectual Merit

A. The proposed activity, which is R & D at a relatively early stage, is in itself both challenging and broad in its intellectual and technical demands. As such it will advance knowledge in a variety of accelerator related areas, and will undoubtedly generate spin-offs in other fields. As far as the two ultimate scientific goals are concerned, if eventually achieved, they will have the potential to bring great advances in our understanding of the microcosm.

B. The team of proponents is extremely well qualified to conduct the proposed R&D. In view of its large scope and its difficulty, more intellectual and technical effort would be beneficial. The team is geographically and technically broadly based and has links, though perhaps not as strong and formal as is desirable, with efforts in the same direction in Europe and elsewhere.

C. The R & D proposed certainly explores original and highly challenging concepts. Whether practical solutions exist to some of the questions is not known but will be answered in the course of the program. There is a similar, although somewhat smaller at present, effort in the same direction in Europe. There is good contact between the two groups, but it would be desirable for them to be at least very well coordinated if not formally amalgamated, both from the point of view of avoiding unnecessary duplication and importantly acting as a nucleus of an eventual global collaboration. It should be noted that if the neutrino factory becomes a reality, then in all probability the accelerator would be in one continent and the detector(s) in other continent(s), clearly a very good basis for a global approach.

D. The collaboration is at an early stage. This is a high risk activity, the R & D is very challenging and success cannot be assured in some potentially critical areas. New ideas are needed in areas that are not the traditional preserve of accelerator professionals. It is hoped that by tapping new resources in universities and laboratories these new challenges can be met.

E. Relatively modest amounts of money injected at this stage would have a large effect and we recommend such incremental support, especially if it allows more groups and individuals to become involved in this effort. The money should

largely be used to empower the universities to contribute in areas where no expertise exists at present and new ideas are at a premium. It clearly is important that the two funding agencies (NSF and DOE) collaborate closely to ensure that the maximum benefit accrues from the investment.

F. The project is being supported at a low level by DOE at present, but as mentioned above, there is a need to expand both the human base and the total level of funding. New NSF supported groups have recently joined the Collaboration but have not been funded at this time for work on this topic.

## II. Broader Impact

A. The R and D activity is state of the art and beyond, so that it will certainly advance discovery in technical and applied scientific areas. If successful and it leads to the construction of a neutrino factory and a muon collider then the discovery potential is vast. The R and D project has to address a large number of quite fundamental questions that are very suitable for the training of graduate students and are likely to attract them because of the undoubted intellectual challenge.

B. The activity is extremely broadly based geographically already and should (and is likely to) become even broader if supported.

C. The broad scope of the work and its perceived importance for the future of the field has already led to intercontinental ties; these should be nurtured and strengthened. The partnership within the US between laboratories and universities funded by DOE and NSF is important in enabling the appropriate talent and facilities to be sought and mobilized.

D. The results of the R and D will undoubtedly be very broadly disseminated. There may well be technological benefits along the way resulting directly from the R and D program, since much of it is at the frontier of knowledge. Should the ultimate goals of a neutrino factory and a muon collider be attained then society will benefit from a deeper understanding of the world we live in.

## Appendix.

The National Science Foundation has appointed a panel to consider 3 proposals as possible candidates for Major Research Equipment (MRE) funding, officially designated as "Panel to Evaluate Major EPP Project Possibilities". The panel membership was:

Jonathan Bagger, Johns Hopkins University  
George Kalmus, Rutherford Appleton Laboratory  
Vera Luth, SLAC

Harrison Prosper, Florida State University  
Stanley Wojcicki, Stanford University(chair)

In addition, Roberto Peccei, UCLA, was scheduled to participate as a panel member but because of illness was not able to attend.

Joe Dehmer, Alex Firestone, and Marvin Goldberg were the NSF representatives present during Panel's deliberations. In addition, Jack Ritchie represented DOE as an observer.

The three proposals were:

1. Rare Symmetry Violating Processes (RSVP) from Brookhaven National Laboratory
2. BTeV Experiment from a Fermilab oriented Collaboration
3. An Expression of Interest for support of R&D oriented towards a Muon Storage Ring and a Muon Collider.

The RSVP Proposal consisted of proposals for two separate experiments, K0PI0

(KOL decay) and MECO ( $\mu$  capture). These two experiments were considered as separate entities by the Panel for the purpose of evaluation. The written versions of all the proposals were submitted to the panel members about a month before the meeting.

The Panel met at SLAC on November 29 and 30 and December 1. The first day was devoted entirely to the presentations by the four groups of proponents. The early morning of the next day was spent in an Executive Session discussing the proposals and formulating questions for the proponents. Subsequently, the Panel met with the proponents in individual groups to obtain answers to the questions that the Panel formulated. The last couple of hours of that day were spent in the Executive Session discussing an outline of recommendations. Assignments were made for writing drafts of recommendations. The final day was spent in writing the drafts of recommendations and then discussing them in Executive Session. The final drafts were distributed by the authors to the chair and other Panel members via E-mail. The chair then finalized the report through E-mail interaction with the Panel members.