

Expression of Interest in doing MECO at Fermilab
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This is to convey my interest in developing a proposal to Fermilab to search for coherent conversion of muons to electrons in the field of a nucleus. It would be the most sensitive search for a charged lepton flavor violating (LFV) process. In the Standard Model, LFV takes place through virtual neutrino mixing at a rate below the threshold of any possible experiment. In contrast, extensions to the Standard Model typically allow such processes, often at high rates, and limits on LFV processes significantly constrain possible models for new physics. In this sense, the powerful neutrino program at Fermilab addresses issues of mass and mixing of leptons in a Standard Model context and LFV experiments address these questions in a non Standard Model context. Our new experiment would be sensitive to rates predicted in many specific models (e.g. grand unified supersymmetry) and would probe new physics at a mass scale up to $3000 \text{ TeV}/c^2$ in models with new vector bosons or leptoquarks, for example. This is well beyond what will be directly accessible at the LHC or ILC. In most models, searches like $\tau \rightarrow \mu \gamma$ are not as sensitive to new physics, despite larger expected branching fraction, due to smaller τ flux and unavoidable backgrounds. Our experiment would detect LFV if $R_{\mu e}$ is as small as $\sim 2 \times 10^{-17}$, where $R_{\mu e} = \Gamma(\mu^- N \rightarrow e^- N) / \Gamma(\mu^- N \rightarrow \nu N')$. This is $\sim 10^4$ times the sensitivity of previous experiments and has equivalent physics sensitivity at least a factor of 10-100 times better than experiments in other channels like $\mu^+ \rightarrow e^+ \gamma$. We would complete data taking in 2 years of running at full intensity following commissioning of the beam and experiment.

The technique is to produce muonic atoms in a thin target. A muon could then convert to an electron through some new LFV interaction with the nucleus. Energy and momentum are conserved by nuclear recoil, resulting in an electron with energy equal to the muon rest energy less a small Coulomb binding energy and small nuclear recoil energy. The momentum transfer to the nucleus is small enough that a large coherent contribution exists, enhancing the value of $R_{\mu e}$ by a factor equal to the nuclear charge. The experiment could run at very high rates, unaffected by the kind of pileup backgrounds that limit experiments with 2 or more final state particles.

The proton beam, muon beam, and experiment would be based on the implementation developed for the MECO experiment planned for BNL. MECO used ideas from many proposed and operated experiments, and was reviewed extensively for physics motivation, technical feasibility, and cost, always with positive results. For example, a HEPAP sub-panel chaired by Robert Cahn and jointly appointed by Robin Staffin and Michael Turner was charged with advising the agencies on the scientific value of the RSVP experiments. They found that “both MECO and KOPIO are well-motivated, ambitious experiments that target new physics of fundamental importance in promising – and so far unexplored – territory. The theoretical motivations for both experiments are well-founded and the sensitivities they hope to reach are well-matched to predictions of many popular models for new physics.” Commenting on the world-wide interest, they noted that “this level of interest and activity around the world attests to the importance of these searches, and a shared sense that experiments may finally be reaching the sensitivities required for revolutionary discoveries.” A new experiment would certainly run after the LHC reports results of high luminosity running, and the sub-panel concluded that “new discoveries at the LHC would likely heighten interest in the observation of the expected virtual effects in rare decays” and “should the LHC fail to find any evidence for new physics, the RSVP experiments would still be

important for their sensitivity to new physics above the TeV scale.” They also concluded that a discovery of LFV in $\mu^+ \rightarrow e^+ \gamma$ would further motivate MECO in order to understand the source of LFV. The most recent cost and technical evaluation was done by a committee appointed by the NSF and chaired by Stanley Wojcicki. They concluded that “in our judgment, the experiment appears ready to proceed on technical grounds.” Regarding the MECO system of large superconducting magnets, the committee found the “designs to be well conceived, with no significant outstanding issues” and that “the [cost] estimating approaches for all three elements of the magnet system were sound.”

Despite strong reviews, earlier approval by the NSB to fund MECO, and a significant investment of money and human capital, the NSF canceled the RSVP project that included MECO and KOPIO. The Foundation cited increased overall construction and operation costs, particularly in the scenario in which the NSF would be required to bear the full cost of operating the AGS accelerator if RHIC operations ended before RSVP was finished. The MECO portion of the RSVP budget had been relatively stable for the few years preceding this review, with some increase in costs resulting from a better understanding of the cost of large superconducting magnets that was achieved with a detailed design study. The scientific motivation and technical feasibility of MECO were not cited as factors contributing to the cancellation.

MECO has many technical challenges that I do not underestimate. We must build a new proton beam; 8 GeV is the ideal energy to both produce low energy muons efficiently and reduce anti-proton induced backgrounds. It must be pulsed at ~ 1 MHz with very good *extinction*, or suppression of protons between the pulses, in order to eliminate critical potential backgrounds. To make enough muons, we need production efficiency much higher than that of existing beams. MECO adopted a scheme with the production target in a solenoid with tapered field to get high pion and muon collection efficiency, and sections of toroids in the transport channel to sign and momentum select the beam. Removing intrinsic background from decay of muons in Coulomb bound orbits requires very good electron momentum resolution in a high rate environment. This and other considerations motivated putting the muon stopping target in a graded solenoidal field, with detectors downstream of the target. All these well-developed techniques for MECO would be used at Fermilab.

Given the existence of an 8 GeV beam from the booster, the MECO collaboration considered Fermilab as the site for MECO nearly 10 years ago. Fermilab physicists studied the possibility and concluded that producing a 1 MHz pulsed beam and slow extracting from the booster were not technically feasible. When the collider program ends, the situation is changed. The accumulator, debuncher, and recycler will become available and can be used to re-bunch an 8 GeV beam and provide for slow extraction.

A possible operating mode, developed by Fermilab physicists, would use the existing accelerator complex, stacking multiple booster batches in the accumulator ring, transferring beam to the debuncher, rebunching the beam, and slow extracting to a new experimental area. Studies of many of the beam issues have started, partly in support of upgrades for the neutrino program and partly specific to developing a beam for a conversion experiment. It appears as if the modifications to the accelerator complex, summarized here, are modest in cost and not overly technically difficult. Please excuse any inadvertent misattributions of credit in the following. Plans for increasing the booster throughput have been developed for the neutrino program. C. Ankenbrandt and M. Popovic have shown that beam can be brought from the booster into the accumulator by passing through a fraction of the recycler ring, eliminating civil construction for this transfer line. Stacking multiple booster batches in the accumulator has been developed by D. McGinnis and others, and is an essential part of the accelerator complex upgrade for NOvA. Transfer to the debuncher is a straightforward modification of the current anti-

proton operations. Ideas from D. Neuffer and E. Prebys exist for coalescing the beam into a single bunch and resonantly extracting it. The revolution time in the debuncher provides a nearly ideal bunch spacing of $\sim 1.6 \mu\text{sec}$. Ideas for improving and constantly monitoring the extinction exist from MECO, and new ideas are being explored. The location of the experiment itself has been considered by D. Bogert. Although the MECO experiment was well studied and optimized, it will be many years between the time MECO was designed and a new experiment is done at Fermilab. I believe the basic MECO technique is sound and would reinvestigate details of the implementation, potentially improving the sensitivity.

To reach a sensitivity of one detected event for $R_{\mu e} = 2 \times 10^{-17}$ in an experiment like MECO, I estimate it will require approximately 4×10^{20} protons on target during the high intensity data collection period. In the scenario in which the booster runs with 4×10^{12} protons per pulse, we would get this integrated luminosity in 2×10^7 seconds (2 years running) using 5 Hz out of the booster. We understand that the NuMI program plans to use 12 booster cycles in 1.33 seconds, which would provide 8 Hz for MECO, if no other program is running and 15 Hz repetition rate is achieved. It is prudent to plan for possible shortfalls in performance of the accelerator and experiment by developing flexible operations plans.

Plausible ideas for necessary modifications to the accelerator complex exist now or the modifications are thought to be doable using known techniques. Nonetheless, technical and cost risks exist that should be retired as soon as possible if this program is to proceed. Some important tasks would benefit from work at Fermilab in the coming year to extend some of the design work that was done for MECO and that is being done now at Fermilab. The list will likely evolve but currently the following seem most important to prove the feasibility of Fermilab as the site for MECO:

1. Further study of the re-bunching scheme including limitations on the bunch intensity
2. Development of an extraction scheme that improves the extracted micro-pulse width
3. Design of a proton transport with secondary extinction devices giving small spot size at a target
4. Study of radiation shielding issues in the accumulator/debuncher complex
5. Design and tests of beam extinction devices in the storage ring and in the proton beam transport

Cooperation with long term activities at Fermilab such as muon collider R&D could be mutually beneficial. However, meeting the formidable technical challenges of MECO requires an effort that is strongly focused on developing our beam and detector if the experiment is to succeed.

A number of university groups are interested in the possibility of doing a muon conversion experiment at Fermilab. More are needed. There is also interest among Fermilab physicists, with active participation mostly by people interested in muon collider development. A broader base of support from Fermilab physicists is needed. We certainly had difficulty attracting active, committed, collaborators for MECO when the support was uncertain. I hope that the Laboratory's endorsement of a plan for further studies will encourage physicists at Fermilab, universities and other labs to join this effort.

The cost of MECO in FY2004 dollars was about \$89M, including $\sim 29\%$ contingency calculated with the TRW model, for the secondary extinction devices, beam line, experiment, installation, and management. The time scale for constructing MECO, vetted by numerous review committees, is about 5 years, driven by the SC magnet construction. At Fermilab, additional resources would be needed for changes to the accelerator complex, the proton beam-line, and civil construction associated with the proton beam-line and experimental hall; little useful guidance is available for these costs from the BNL experience. Given the encouragement of the Laboratory, I hope that a collaboration can be formed soon, a letter of intent written in 6 months, a proposal written within a year, and a plan brought to P5 in 1-2 years.