



Department of Energy

Washington, DC 20585

September 17, 2002

Dr. Michael Witherell
Director
Fermi National Accelerator Laboratory
P.O. Box 5000
Batavia, Illinois 60510-5000

Dear Dr. Witherell:

This letter reports on the annual program review conducted March 19-21, 2002. These reviews are an important element in the oversight of Fermilab by the Department of Energy (DOE). They provide peer review of the Fermilab program and are helpful to the Division of High Energy Physics (DHEP) in determining our priorities. This letter conveys our impressions of the Fermilab program, together with our expectations for the future. While relying heavily on the review, DHEP also receives information from additional sources that contribute to our conclusions.

The 2002 review was successful, in the sense that there was considerable constructive interaction between the Laboratory, the outside consultants, and the DOE staff. The high level of thoughtful preparation and polish in the presentations was recognized and appreciated by the Review Committee.

On the other hand, there were several serious issues raised, particularly about the progress of Tevatron Run II and the response of Lab management to these issues. The outside consultants were forthright in their comments about the Lab's program and did not spare Lab management from their views. I appreciate the willingness of you and the rest of Fermilab's senior management to address these issues directly and candidly with the Review Committee. Several useful recommendations came out of the review, some of which I will touch on below. We look forward to a continued partnership towards making Run II a success.

General

Unfortunately, the most important events for Fermilab in the last year revolved around construction difficulties on the NuMI project, the major effort undertaken to address and resolve these difficulties, and the continuing disappointment of Tevatron luminosity. As you noted at the review, your tenure at Fermilab has been marked by the need to respond to several "crises," from the Run II detector upgrades to NuMI, and now Tevatron luminosity. The latest crisis is the most difficult and the most important. The worldwide High Energy Physics (HEP) community, and other communities outside of physics as well, are now paying close attention to the Run II story.

Lab management should be congratulated for navigating through the NuMI crisis, but now must apply their skills to the current crisis. And while attention must be paid to charting the



Laboratory's future, the clear focus of the Lab's efforts must be on the immediate and near-term issues. We heard you say repeatedly during the review that Run II was your highest priority and that everything possible was being done to make the necessary resources available. We are still concerned that these priorities, and the seriousness of the current situation, are still not being clearly conveyed to or accepted by all Laboratory staff. We realize there are differences of opinion here, but several conversations with staff during and subsequent to the review have not abated this concern. This strikes us as an "all-hands" situation, but there still appear to be areas of the Lab where the response to Run II is either "not my problem" or "nobody asked me." Neither answer is acceptable.

You began a Lab-wide culture change when you assumed the role of Fermilab Director in 1999. Your efforts to put the Run II detector upgrades on a firm and believable schedule were much needed and largely successful. The handling of the NuMI crisis was also direct and forceful and so far successful. In this case, you can build on these changes by engaging the entire Lab in the Run II effort, recognizing that the Lab is not merely a collection of independent projects with their own budgets and agendas, but a holistic program with priorities, goals, and issues that affect all stakeholders. Only the Director can convey that message, and lead the entire Lab to engage that challenge.

Collider Run II

Collider Run II provides an historic opportunity for discoveries in particle physics between now and the start of the Large Hadron Collider (LHC) at CERN. Therefore, making Run II successful must be Fermilab's highest priority. Our Division has also made it one of the highest priorities for the national HEP program, at the expense of other parts of the national program.

The response of management to the Run II luminosity issue was perceived to be slow and lacking sufficient urgency. Whether this is true or not, perhaps in this case perception may be more important than reality. We acknowledge that the appointment of Mike Church as Deputy Head of the Beams Division, with direct responsibility for Run II luminosity, and the active involvement of Steve Holmes in Run II operations issues, and now as Acting Head of the Beams Division has been effective in identifying necessary tasks and dealing with the many accelerator physics and engineering issues in a systematic way. In particular, Mike's detailed plan for luminosity improvements, coupled to performance milestones, appears to be an appropriate and effective tool for organizing work and tracking progress. The challenges will be marshalling the personnel resources necessary to execute the plan, and maintaining the schedule of luminosity improvements. Calls for volunteerism have proven to be insufficient to deal with the personnel needs, and dedicated redirection of effort appears to be necessary.

We heard at the review that Fermilab is having difficulty attracting and retaining top accelerator physicists to tackle these problems, and that the existing expertise in the Beams Division is overburdened, but also uneasy with accepting help from outside, whether that be other Fermilab staff who work outside of the Beams Division, or completely outside of the Lab. While we understand that training of new recruits--whether they have an accelerator physics background or

not--can be time-consuming, it is an investment the Lab must make to ensure its long-term health. We have been encouraged with the efforts since the review to enlist help from Stanford Linear Accelerator Center (SLAC), Lawrence Berkeley National Laboratory (LBNL), Brookhaven National Laboratory (BNL) and elsewhere, and if we can be of any assistance in getting talented people to Fermilab for either short- or long-term appointments, please let me know. Efforts to secure outside expertise should continue, and we expect your personal involvement in attracting top people will be required. Further, we expect that only you can initiate a "culture change" in the Beams Division that would allow them to accept more assistance, including solutions from outside Fermilab.

At the time of the review, typical instantaneous luminosity values were roughly $1.2 \times 10^{31} \text{ cm}^{-2} \text{ sec}^{-1}$, about half of the goal; and weekly or total integrated luminosity achieved were also low by about the same factor. We are encouraged to see that as of this writing, instantaneous luminosity is above $2 \times 10^{31} \text{ cm}^{-2} \text{ sec}^{-1}$, a significant improvement; yet actual luminosity still lags the goals by a factor of ~ 2 . This is despite considerable effort by many talented people. The trend of a continuing substantial lag relative to the goals does not build confidence that the Church plan can be executed on schedule. We will hold an interim review of Run II progress this fall in order to assess the status of Run II luminosity performance and implications for the accelerator and detector upgrades planned for Run IIb. Details of the review are being developed.

Though we will wait for the Run II review to make final judgments on technical issues involved, we received a clear impression at the annual review that the Lab does not possess an adequately sophisticated, systematic understanding of the Tevatron complex. The accelerator theory group does not appear to be engaged in Tevatron commissioning, and we saw little evidence of beam simulations that could help understanding the technical issues. The overall level of beam diagnostics and feedback may require significant improvement in many areas, to achieve Run II luminosity goals. We think the long-term future of Run II will depend on the Lab developing a program that will place the understanding of Tevatron operations on a firm foundation of data and theory, and provide the basis for continuing improvements.

Both CDF and D-Zero collaborations were able to show convincing sub-detector performance pictures at the review, and they should be congratulated on their successful commissioning efforts. But while both the CDF and D-Zero, with leadership and guidance from Lab management, clearly did heroic service in achieving the Run II startup schedule, it has become clear in the intervening year that the detectors were quite far from ready on day one. In the case of D-Zero, detector readout was still not complete even at the time of this year's review, and the trigger and data acquisition systems were performing far below the level necessary if the Tevatron had performed as expected. The CDF detector has generally been in a better state of readiness, and has had some significant successes, including the cutting-edge silicon vertex trigger. But major components of the CDF silicon systems, both detectors and infrastructure, were still not operational at the time of the review. While all large, complex detector projects have some level of startup difficulties, we believe the Run II upgrade experience is an important cautionary tale to consider in the light of Run IIb.

At the time of last year's review, planning for Run IIb was only just beginning, and this was the cause of some concern to the reviewers. We were pleased to see that planning for the CDF and D-Zero silicon tracker replacements has become quite advanced in the intervening year, and that the collaborations have seen the wisdom of pursuing many common design elements to reduce costs and speed the schedule. Unfortunately this serious conceptual design effort has led to much higher--if more realistic--estimated costs for the silicon trackers, and the collaborations have only recently considered other detector replacements that may be necessary for Run IIb. Planning for these latter items is still at an early stage. Further complicating this picture are the Run II luminosity performance, the uncertainties in the schedule and technical planning for specific Run IIb accelerator upgrades, and the related issue of the CERN LHC startup schedule. The need for the detector replacements hinges on the questions of how much total luminosity the Tevatron can be expected to deliver, and if and when the Tevatron bunch spacing will be lowered to 132 nsec from the present 396 nsec. As part of our review of Run II progress this fall we will need to examine these issues and determine the next steps for the detector upgrade projects. At this time, the plans for Run IIb accelerator upgrades are much less developed; they will need a significant evolution over the next few months to provide useful input to this decision process.

We understand that you are fully committed to the success of Run II, since it is the highest priority and most visible component of the Laboratory's program. Our Division is likewise fully committed to this program, assuming it continues to hold a significant likelihood for new discoveries and exciting physics. However, our ability to provide additional resources to solve possible problems encountered in Run II is severely limited by overall budget constraints and other high priority items. The Run II efforts, including detector replacements and accelerator upgrades, must have sufficient contingency built-in to deal with issues as they arise. Any cost or schedule difficulties encountered will have to be addressed via redirection from other areas of the Fermilab program. You presented a preliminary plan of how you plan to deal with the likely scenario for FY 2003 (the President's budget request) at the review. Our recent follow-up discussion on detailed budget issues was very useful and similar meetings should be held on a regular basis in the future.

Neutrino Experiments

The Fermilab neutrino program is strong, and poised to be a world leader in accelerator-based neutrino physics. After a year of mounting delays and difficulties with NuMI underground construction, the re-baselined project appears to be on a sound footing. The MINOS detector construction is proceeding apace, and commissioning and detector calibration of installed modules is occurring even as new ones are being put in place. The MiniBooNE experiment is ready to take data, and will soon learn whether just three families of neutrinos are sufficient to describe all known phenomena, or whether something very new and exciting is happening in the neutrino sector. We look forward to initial results in the coming months.

You, your management team, and the NuMI/MINOS collaboration deserve congratulations for much hard work in rescuing that effort and putting it on a sound footing. However, our enthusiasm is tempered by the knowledge that there are still a few more years before the project

is complete. We note that NuMI will not be completely “out of the woods” until the tunnels and halls subcontractor has completed his work, and after the subcontract for tunnel outfitting and surface buildings has been awarded. As we commented above for Run II, there are no additional resources available to address any issues that may arise. The Laboratory will have to continue its proactive oversight of this project until it is brought to completion. We have confidence that the experience of last year’s re-baselining will not be soon forgotten and that the discipline that Fermilab management brought to the project will be maintained.

A comment on the future of the neutrino program is in order. There has been much interest lately in alternative (e.g., off-axis) detectors for NuMI, upgrades to the proton source for dedicated high-flux neutrino experiments, and muon storage rings as neutrino “factories.” We encourage studies of future options in this area, which may become a high-priority research frontier in the not-so-distant future, if the physics opportunities so dictate. But today, our priority clearly rests with Run II, and the High Energy Physics Advisory Panel (HEPAP) long-range planning subpanel has chosen a Linear Collider (LC) as the highest future priority for the field. Timely and cost-effective completion of NuMI/MINOS is required, and its physics program is an integral part of HEP in the coming decade; but executing this program cannot have a significant negative impact on Run II, nor can its physics scope be significantly expanded until the questions surrounding an international LC project have been resolved.

The LHC at Fermilab

We know from recent dedicated reviews of the U.S. LHC Accelerator Project, the U.S. CMS Project and the U.S. CMS Research Program that Fermilab has been doing an excellent job in fulfilling its management and oversight roles in these efforts. The Review Committee heard abbreviated reports on these efforts at the annual review, and we were all similarly impressed. Both Fermilab and the U.S. HEP community can rightfully be proud of the effective and important role we are playing to help ensure success of the LHC. Longer-term, we expect Fermilab to act as host laboratory for the accelerator and CMS components of the U.S. LHC Research Program, and the Lab is already playing a vital role in developing the software and computing infrastructure for CMS that will ensure a leadership role for U.S. physicists in that experiment. We also heard at the review that Fermilab plans on dedicating an entire floor of Wilson Hall as the U.S. CMS research center. We view this as a positive development that will only enhance the U.S. role in CMS, and we will work with you to make it a reality.

The DOE and the National Science Foundation (NSF) are currently working hard to try to address funding challenges for the LHC research program in the near-term, roughly FY 2003-05. These difficulties are driven by the need to ramp-up detector pre-operations and computing activities now, when the construction project has not yet ramped-down. We appreciate the Lab’s assistance, in its oversight role, in optimizing the U.S. LHC Research Program (both accelerator and CMS pieces) during this difficult transition period.

Accelerator R&D

Fermilab has maintained an active program in accelerator R&D for possible future facilities, as is appropriate for the flagship U.S. lab in HEP. It has certainly not escaped notice that the U.S. will no longer have an energy-frontier facility when the LHC starts running, and that the Fermilab program of the next decade (2010 and beyond) is at best ill-defined right now. Steve Holmes stated at the review, as he has before, that Fermilab wants to maintain strong research programs in the “enabling technologies of HEP: magnets and RF,” in order to “keep its options open” for future facilities. While this is a laudable goal and an intelligent strategy in uncertain times, we believe recent events dictate a more focused approach.

The HEPAP subpanel released its Long Range Plan early this year, and clearly supported a TeV-scale, high luminosity LC as the highest priority for the future of the field. Similar conclusions were reached by recent high-level planning exercises in Europe and Asia, and the international community is now moving forward to try to realize this ambitious and exciting project. While HEPAP also endorsed increased efforts in many areas of accelerator R&D, these were clearly at a lower priority than the LC.

Given this clear recommendation and the highly constrained budget situation we find ourselves in, it seems appropriate to carefully consider prioritizing Fermilab’s accelerator R&D efforts, accepting that the Lab cannot lead research activities in all areas. For example, several reviewers commented that Fermilab is not yet ready to play a leading role in LC R&D, and indeed the scale and technical complexity of the full LC project is probably beyond any single laboratory’s capacity. Thus, Fermilab’s goal should not be excellence in all areas of LC technology, but development of special expertise that is complementary to that which already exists elsewhere.

In this context, there are already strong R&D efforts in superconducting Radio Frequency (RF) systems in Europe, and some expertise in the U.S. at Jefferson Lab and Cornell that does not need to be duplicated at Fermilab in today’s fiscal climate (specialized expertise needed to develop the CKM beamline is an exception). On the other hand, Fermilab’s work in supplying warm RF accelerating structures for the Next Linear Collider (NLC) “8-pack” test is crucial to meeting the schedule for that important technical milestone, leverages in-house engineering expertise, and develops new capabilities that Fermilab can exploit in the future. It must be given high priority among Fermilab accelerator R&D efforts. In the longer term, the Engineering Test Facility for a Linear Collider seems an intelligent choice for further LC work, albeit at a somewhat lower priority.

We understand that Fermilab’s recent LC R&D efforts have been very much limited by the funding “cap” in place on LC R&D. We have recently received language in the House Energy and Water FY 2003 appropriations bill that encourages DOE to work with OMB to remove those barriers. If this can be achieved, we expect that the Fermilab LC effort would be re-optimized, within the overall budget context, consistent with its high priority. Clearly such an optimization also has to respect the priorities of the national LC R&D efforts, which is one reason for the creation of the U.S. Linear Collider Steering Committee (LCSC). We expect to work with you

and the LCSC in the coming months to develop an R&D program that would allow the U.S. to, in the words of the HEPAP subpanel, "prepare to bid to host" an international Linear Collider.

In other areas of accelerator R&D, Fermilab has undertaken a significant role in R&D for muon-based accelerator concepts, including neutrino "superbeams" and the muon storage ring discussed above. While we are open to the possibility of such scenarios in the Fermilab's long-term future, the question of whether the physics dictates such a program in the U.S. will probably not be resolved soon, and any expansion of the Fermilab accelerator R&D program in this area is contraindicated by budget constraints and the higher priority of the Run II and LC efforts. As with the LC program, Fermilab cannot afford to maintain expertise in all areas. The key question is how to minimize overall effort while still allowing progress on necessary technical milestones and proof-of-principle demonstrations. Our impression is that the Muon Collaboration has significant R&D issues that must be resolved and needs continued help in focusing their efforts. Your assistance in improving this situation is appreciated.

Fermilab has also taken a leading role in R&D toward future generation proton colliders. This R&D has a natural synergy with the accelerator component of the U.S. LHC Research Program. We are currently developing funding guidance for the latter effort, which we will communicate to you and the program manager soon. We note here that the Fermilab superconducting magnet R&D effort must also exist in the larger context of the national R&D program in superconducting magnets, which is being addressed by a separate review panel, chaired by Bruce Strauss from our office. The recommendations of this panel will be discussed in a separate communication. Finally, we did not hear about any significant work on super ferric magnet R&D at the review (other than that related to LC design), and we hope this is an indication that that effort is being ramped down to a minimal level.

We also heard about various smaller efforts in what might be called advanced accelerator R&D, including the high-brightness photo-injector facility. The reviewers were generally enthusiastic about this effort as an example of a major lab using its infrastructure to leverage a smaller-scale facility that supports the university community and trains graduate students in state-of-the-art accelerator technology. Fermilab management should strive to ensure that these facilities are used to support unique activities such as flat-beam studies, and do not merely reproduce research in areas (e.g., plasma-wakefield acceleration) that have a long history at other institutions.

BTeV

It seems appropriate here to begin from the recommendation of the HEPAP subpanel on Long-Range Planning, as submitted to DOE and NSF in January 2002:

The BTeV project cannot be funded with the scope and timetable originally envisaged. The collaboration and Fermilab are considering revised plans that, if approved by the Fermilab PAC, should be brought to P5 for evaluation later this year.

At the annual review we heard about these revised plans, including a one-arm spectrometer design with some enhanced detector capabilities. The project would also be delayed so that fabrication does not start until after NuMI and Run IIb ramp down, and interaction region magnets from CDF or D-Zero areas would be re-used to save costs. At their April 2002 meeting, the Fermilab Program Advisory Committee (PAC) did indeed recommend that the Laboratory approve the revised BTeV proposal. You accepted this recommendation. We note that the consultants at the annual review were, quite frankly, significantly less impressed with the revised BTeV proposal, both in regard to its physics merits and the likelihood that it could be completed on an aggressive schedule. While they had much less time to consider the proposal than the PAC did, and were not charged with making decisions on its scientific merit, we believe these comments by respected members of the HEP community illustrate that the revised BTeV proposal is still quite controversial. That said, we have no *a priori* reason to disregard the considered opinion of the Fermilab PAC, and we respect the significant effort they have invested in evaluating BTeV over several years.

The question is how to proceed from here. You have already had several discussions with the Department about establishing BTeV as a DOE project, and our understanding as of early this year is that we would schedule a DOE/HEP review for this fall, which if successful would lead to a construction project baseline review in spring of 2003. In view of the recommendations of the HEPAP subpanel Report, the review this fall would be a meeting of the Particle Physics Project Prioritization Panel (P5). The Panel would have to evaluate the scientific merits and technical challenge of BTeV in the context of the national and international HEP program, as proceeding with BTeV would foreclose other options in the U.S. HEP program, and must compete successfully with the LHC-b experiment at CERN.

This situation has been further complicated by the recent difficulties you have encountered in executing Run II, and the problematic history of NuMI, even though the latter appears to have been resolved successfully. If independent scientific and technical reviews conclude that DOE should proceed with BTeV, even at the expense of other elements of the U.S. HEP program, we must have full confidence that Fermilab can deliver the project on time, on budget and with the promised capabilities, and we must convince higher levels of government that our confidence is well-founded. That will be a significant challenge. A final DOE/HEP decision regarding BTeV may have to be delayed until Run II issues, including Run IIb upgrades, have been resolved to our satisfaction.

Other Initiatives

Since last year's review, the CKM proposal was given scientific approval by Fermilab, while the KAMI proposal was not approved. Again we think the PAC did a conscientious and thorough review of the technical challenges and scientific promise of both experiments. The brief update on CKM R&D progress given at the annual review was encouraging, and as we noted above the Superconducting RF research effort at Fermilab should be focused on this application. Though the scope of CKM is relatively small, and its schedule less aggressive than BTeV, it is still a

major investment in the context of the national HEP program. Thus we expect that a P5 review to assess its value to the national HEP program would occur by the end of 2003.

The complementary rare kaon physics program at BNL has suffered significant setbacks due to budgetary restrictions. It now appears that the precursor to CKM, the BNL E-949 experiment, will likely receive much less data than planned. While this is an unfortunate loss for the HEP program, and especially the Fermilab staff who participated in E-949, this event should not preclude the development of a strong rare kaon decay program at Fermilab. We would expect a rare kaon program using the Main Injector to be an important component of the Lab's program in the next decade.

Theory

The Fermilab Theory Group is an important resource for the community, and maintains broad interests across many areas related to particle physics. Moreover, many members of the theory group possess the rare combination of talents needed to communicate the latest theory ideas to experimenters and understand the complications real experiments encounter when trying to interpret theoretical predictions. The recent additions of Boris Kayser and John Beacom only reinforce a strong group overall in an important area (neutrino physics) that was previously understaffed. The Review Committee was in general very complimentary.

There was also some discussion of ways to improve the Theory Group, most involving increased resources. We well understand that in these times, growth of any area of the Lab is difficult to achieve, even in important areas. Our concern is the relative lack of junior staff appointments to ensure the ongoing vitality of the group, especially as Fermilab changes focus from Standard Model to Beyond the Standard Model physics. A strong program of guest scientists and "Frontier Fellows" only partially addresses this issue. We strongly encourage Fermilab management to take steps to open as many Associate Scientist positions in theory as possible.

Astrophysics

The modest Fermilab program in particle astrophysics is leveraging lab resources to provide management experience, leadership and "critical mass" to several efforts in both theory and experiment (Auger, Sloan Digital Sky Survey (SDSS), and Cryogenic Dark Matter Search (CDMS)). These activities bring welcome diversity to the Fermilab program and support exciting new science at interdisciplinary frontiers. Recent successes in deployment of Auger and CDMS modules, and ongoing new results from SDSS, were one of the highlights of this year's review. We support these activities, and strongly encourage Fermilab to continue to nurture these efforts as a small but vibrant part of the Lab's program.

Environment, Safety & Health

The Lab's commitment to environmental safety and hazard minimization was clear at the review. Safety and security issues were at the forefront this year, and that trend will likely continue.

Injuries suffered by subcontractor personnel during the NuMI excavation in 2001 were one focus of activity, and the Lab appears to be taking appropriate measures to prevent future incidents. For future reviews it would be informative to see data on how the lab is reducing "off-normal" occurrences, improving staff training, and identifying and resolving safety issues.

We are aware that the changing DOE requirements on Safeguards and Security in the wake of September 11 have been a challenge for your ES&H staff to administer. At the same time, Fermilab scientific staff must recognize that they hold a privileged position in the national R&D program and that compliance with reasonable security provisions is both necessary and a sign of their commitment to that program. We will continue to work with you to tailor a security policy that is appropriate and effective for all Office of Science laboratories.

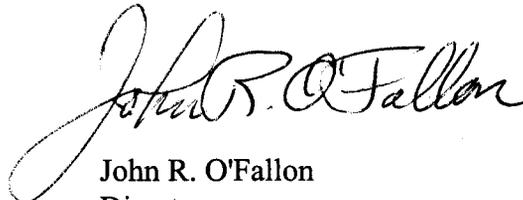
As you know, there has been much emphasis within the Department of late on program performance, assessing performance via metrics, and relating these to budgetary decisions. As we in DOE/HEP move into FY 2003, we will have a new structure for our budget and reporting codes which we hope will provide more transparency and clarity to our budget requests. As the largest U.S. HEP Lab, Fermilab's budget is a very significant fraction of that overall request. To make these requests successful we will need your help in translating Fermilab's internal budgets into a common basis that we can use to formulate and communicate the budget decisions of our Division. Though we have already had productive discussions of your WBS-based internal budget, there are still many detailed questions about breakdowns of, e.g., Run II operations and support, Run II upgrade projects, and accelerator and detector R&D that need to be addressed.

In turn these budgets need to be linked to prioritized research plans for the near-term and farther future, based on physics issues, personnel needs, and Fermilab's capabilities, with various decision points or options based on the outcome of some outstanding questions. These would include Tevatron performance and the Fermilab-LHC transition; possible LC scenarios; and future developments in neutrino physics. This exercise should be "bottoms-up" and include research thrusts in priority order, so that one can clearly see what different budget levels "buy" in terms of physics. I encourage you develop such a plan, in consultation with our office, so that we can discuss it in detail later this year.

All in all, it is clear that Fermilab is maintaining an excellent and diverse research program at the forefront of many areas in high-energy and accelerator physics, even in difficult budgetary times. We all look forward to working with you and your staff in continuing this tradition of research excellence. However, there are clear areas of concern, particularly the Run II luminosity issue, which must be addressed and successfully resolved. We remain confident that with proper leadership the Fermilab staff can meet these challenges. My office is available to help in any way we can, to the best of our ability.

If you wish to discuss any part of this letter, please feel free to contact me or Michael Procario, the Fermilab program officer.

Sincerely,

A handwritten signature in black ink that reads "John R. O'Fallon". The signature is written in a cursive style with a large, looping initial "J".

John R. O'Fallon
Director
Division of High Energy Physics

cc:

J. Monhart, Fermi Group

S. P. Rosen, SC-20