HEP REVIEW FORM
Grant Applications Office of High Energy Physics Department of Energy

Proposal ID NO.:	124528
APPLICATION TITLE:	Dual Readout Calorimetry – LAB 11-438
UNIVERSITY AFFILIATION:	Argonne National Laboratory
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Consider, for example, the influence that the results might have on the direction, progress, and thinking in relevant scientific fields of research; the likelihood of achieving valuable results; and the scientific innovation and originality indicated in the proposed research.

1a. Physics Motivation: Is there a clear collider detector based physics motivation? What measurement or class of measurements would benefit from this detector technology?

There is clearly a strong physics motivation for having high resolution electromagnetic and hadronic calorimetry in any new collider detector. Calorimetry is the primary means for measuring the energy of jets in e+e-, hadron-hadron, or electron-hadron collisions, all of which are being considered as future collider facilities. The measurement of jets is extremely important for many types of new physics, including the production and decay of heavy vector mesons, searching for new particles such as the Higgs or supersymmetric particles, studying parton distributions and fragmentation functions, and many other topics. When measuring jets, the energy resolution of the calorimeter is of primary importance. For measuring the total hadronic energy, the energy resolution currently limited to a large extent by the fluctuations between the electromagnetic and hadronic components of the shower. The dual readout approach uses the fact that the characteristics of electromagnetic and hadronic showers are different. Electromagnetic showers are primarily composed of highly relativistic particles (electrons and positrons), which can produce Cherenkov light in a given material, while the hadronic component is comprised of heavier charged particles that may only produce scintillation light in the detector. The dual readout calorimeter uses a homogeneous scintillating material (either an inorganic crystal or scintillating glass) to absorb the total energy, thus providing a uniform response without significant dead material, with a clearly discernable signal for the electromagnetic and hadronic components. The electromagnetic component is identified and measured by its shorter wavelength light (predominantly in the UV) and fast decay time, while the hadronic component is identified and measured by detecting slower (although still relatively fast in terms of overall response time) scintillation light at a longer wavelength. Measuring these two components simultaneously allows a determination of the electromagnetic and hadronic components of each individual shower on an event by event basis and allows a much more precise determination of the total energy. This would clearly provide a significant improvement in the energy resolution for measuring the total energy of showers from jets over currently used hadronic calorimeters.

1b. Generic Research: Is this generic research that can benefit a significant fraction of collider detectors for high-energy physics as opposed to engineering to make a technology work for a particular experiment?

This research is definitely generic in nature and could benefit any collider detector that would use hadronic calorimetry, which would virtually be all such detectors.

1c. Impact vs Risk: How does the risk of failure compare to the magnitude of the potential impact?

There is clearly considerable risk involved in all aspects of this proposal. First, in order for this technique to become viable, it is necessary to reduce the cost of the homogenous detector material to a reasonable level in order to make it affordable. The main candidate materials are scintillating crystals (either doped PbF2, PbFCl, BSO or PbWO4) and the target price is $2/cc. For scintillating crystals, this is a very low number, since most scintillating crystals available today cost many times that. However, the economics of crystal production is not straightforward, especially in terms of their use in high energy physics applications. In the case of the PbWO4 crystals obtained for CMS, this involved international agreements with suppliers in Russia and China, and scientific involvement with CERN and a number of other research institutions. It is therefore not beyond the realm of possibility that similar arrangements could be made to supply large volumes of crystals to a scientific collaboration, assuming that the preliminary research and development costs have been previously covered. It is furthermore clear that no crystal supplier will undertake such an R&D project on their own, especially with the uncertainties in the time scale for any future collier facility. However, as mentioned in Section 1a above, the benefit of the successful development of a homogenous hadronic calorimeter material would be of enormous benefit to the physics program at such a collier. It seems therefore very appropriate that this R&D be funded through a mechanism such as the one put forth in this proposal.

In addition to the risk of producing a suitable calorimeter material at an affordable cost, there is also considerable risk in the development of the new readout devices that are proposed. There is a conservative option which uses presently available silicon photomultipliers (SiPMs) that could be used with little additional development or cost. However, there is a much more ambitious approach to develop large area photosensors (LAPPDs) that would stem from other currently ongoing R&D activities. This would require a very significant effort and an advance in the current state of the art in photocathode production, and indeed, in the fundamental understanding of the underlying physics and material characteristics of these new photocathodes materials. One proposed scheme is to deposit a thin layer of silicon nanoparticles on a silicon substrate and to study the change in the bandgap structure of the resulting composite. Research at this level is certainly far from deploying such devices in a large scale calorimeter. However, if successful, this too would constitute a considerable advance in current detector technology and would be of significant benefit in the end.

2) Appropriateness of the proposed method or approach;

Consider, for example, the logic and feasibility of the research approaches and the soundness of the conduct of the research.

The approach for the R&D that is proposed seems very reasonable, feasible and well thought out. The plan is to produce a number of small samples of potential scintillators and investigate their properties to determine which one (or ones) to pursue to larger scale production. One question might be is that the investigators seem to imply that only one material would be chosen for further development after their initial investigation. However, it may be worthwhile considering developing more than one (although probably not more than two) to the next stage of producing larger samples, since the problems encountered in larger scale production may be different than anticipated from the smaller samples, and a seemingly less attractive option may actually proved to be better when developed further. As mentioned above, the plan to develop new photocathode materials is very ambitious, but the approach itself seems quite reasonable. In addition, the inclusion of Monte Carlo simulations to study the expected performance is also quite beneficial.

3) Competency of the personnel and adequacy of proposed resources;

Consider, for example, the background, past performance, and potential of the investigator(s); and the research environment and facilities for performing the research.

The competency and backgrounds of all of the Principle Investigators is extremely impressive. Dr. Zhu is a world renowned expert in the field of scintillating crystal development and played a major role in the development of radiation hard PbWO4 crystals for CMS, and has been involved in many other large crystal calorimeter projects, including the BGO detector for L3 and numerous other R&D activities. He is also well connected with the crystal suppliers in China and is certainly eminently qualified to carry out his part of the project. In addition, the facilities at Caltech are very well equipped to test and evaluate all of the crystal samples that will be produced.

Dr. Demarteau is also very well known and is the leader of the LAPPD development project at Argonne. His experience and expertise, as well as his connection with other ongoing R&D activities in the area of large area photodetectors, will be of major benefit to the success of this project.

Dr. Para is also very well know in the field of calorimetry and adds a great deal benefit to the project in terms of computing and simulation resources at Fermilab.

4) Reasonableness and appropriateness of the proposed budget.

The overall budget seems reasonable and appropriate for the proposed scale of the R&D effort. Only a small fraction of the labor portion of the budget is for senior personnel, with a modest amount for a postdoc (40% FTE) and a computing professional (~ 50% FTE). In terms of materials and supplies, if anything, the amount directed to SIC may be a bit low ($50K/yr). It is not clear why Fermilab is also requesting funds to purchase crystal samples if they are to be evaluated at Caltech. In general, the Fermilab portion of the budget seems to have rather inflated costs for travel, crystal samples, guests and visitors, etc.

If you have any other additional comments you would like to include, please enter them here:

In summary, this is a very difficult and ambitious project, but one that would be needed to make such a major advance in the field, hopefully leading to a significant improvement in the resolution and performance of hadronic calorimeters for future collider detectors for high energy and nuclear physics. It would certainly have a major positive impact if successful.