

**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
PRINCIPAL INVESTIGATOR:	Marcel Demarteau

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**1) Scientific and/or technical merit of the project;**

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?

The proposal under review outlines a series of research initiatives aimed at producing low-cost crystal scintillators for use in future, total absorption calorimeters. The advantage of such total absorption calorimeters for electromagnetic calorimetry has long been established in such detectors as Crystal Ball, L3, and CMS. The use of total absorption calorimetry for hadronic calorimeters has long been viewed as cost prohibitive. However, many studies have shown the desirability of jet-by-jet energy corrections in future experiments, such as at the ILC. Dual readout of both the scintillation and Cerenkov light signals in a crystal-based calorimeter would provide the information necessary to make such jet event corrections, significantly improving jet energy and mass resolution. Precision jet measurements, particularly dijet mass measurements, are viewed as a key benchmark in the design of experiments for the ILC.

None of these ideas (crystal calorimetry, dual readout) are particularly new, which the proposal authors acknowledge. What is novel, and important, is the proposed systematic study of the scintillation properties of some previously poorly studied inorganic scintillators, the partnership with manufacturers to target a production cost of \$2/cc for the preferred crystal scintillator(s), the development of low-profile light detectors with sensitivity to both the scintillation and Cerenkov light from the scintillators, and the detailed study of hadronic shower simulations and hadronic response in inorganic scintillators.

I view the proposed research as having a high probability of producing important new results in three focus areas. If the production cost goal is reached, it could potentially have a transformational effect on how we design calorimeters for future collider experiments.

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?

Given the common use of inorganic crystals in electromagnetic calorimetry, the extension of this idea to hadronic calorimetry (or more appropriately homogeneous calorimetry, which the authors refer to as “HHCAL”) based on low-cost inorganic scintillators would clearly be an attractive technology for any future collider detector. Even if the cost target for the inorganic scintillators is not reached, the companion development of appropriate light detectors for dual readout would still be a technology that could be applied to future detectors, as would any improvements to hadronic shower simulations produced in the course of this project.

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

The major thrust of the proposed research, of developing and characterizing low-cost inorganic scintillators, is based on partnerships with one or two Chinese companies. These are Shanghai Institute of Ceramics (SIC) and Shanghai Scintbow Crystal Co., Ltd. It would appear that this is the area where the greatest risk of failure may lie. There appears to be prior collaboration between the authors and these two firms. However, it is not clear how the authors will direct these companies’ efforts to lower production costs, other than visits to the sites. Presumably, some of the costs will be subject to fluctuations in the costs of raw materials; fortunately there is little dependence on rare-earth compounds (other than for doping) whose costs are notoriously volatile. Characterization of the commercially produced crystals will be done at CalTech, with results relayed back to the manufacturer(s). The authors propose development of scintillating PbF<sub>2</sub>, PbFCI, BSO, and PbWO<sub>4</sub> crystals, but the actual production will be at SIC. It is also not clear why \$2/cc is viewed as the threshold for cost effectiveness. Based on the author’s figure of 100 m<sup>3</sup> needed for the HHCAL, this translates to a \$200 million calorimeter, based only on crystal costs. Still, that does not take away from the fact that the development of a new generation of inorganic scintillators will be enormous use to the field. I just do not believe that the authors have made a case for how they will reach the target cost, and why they picked that value.

The development of dual readout photo-detectors is also in conjunction with other groups, namely the LAPDD (“Large-Area Picosecond Photo-Detector”) project at ANL. This includes developing new photocathode materials, in a strategy that the authors describe as “theory driven.” This work will be done at ANL, and thus will be more closely under the direct supervision of the principal investigators. This photocathode development is important and represents a much needed new research effort in an area that the field has inadequately addressed in recent years. The authors propose to study both existing solid state photo-detectors such as commercially available SiPMs (which will likely not work for dual readout), new detectors developed in conjunction with the LAPDD project, and filtering techniques. It is likely that some combination of photo-detectors and filters will give the desired dual readout capabilities.

Finally, the proposed simulation and inorganic scintillator hadronic response studies present no significant risk of failure. Any attempt to improve hadronic shower simulations is welcomed, and will undoubtedly result in some improvements to existing detector simulations.

## **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 proposed research covers three distinct areas: Inorganic scintillator development in conjunction with industrial partners, dual readout photo-detector development, and improvements in hadronic shower simulation and simulation of hadronic response of inorganic scintillators. The appropriateness of the proposed work in each must be evaluated.

For the scintillator development task, much work has already been demonstrated in identifying appropriate crystals for future study. At least four crystals have been identified, although each has challenges in production and/or light yield. A clear plan is laid out for production and testing of these candidate crystals. Existing lab facilities are in place at CalTech to make the necessary characterizations measurements. However, the linkage between these studies and the effort necessary to lower the production cost, which is the key aim stated for this proposal, is not very detailed. Much confidence is placed in the expertise of SIC and Scintbow.

The approach to photo-detector development, with parallel efforts to test commercially available sensors and to develop novel sensors in conjunction with LAPDD, is logical, cost-effective, and entirely appropriate. It is almost impossible that some important new development, whether in photo-cathode design or MCP detector development, would not come out of this work.

For the hadronic shower simulation studies, both detailed comparison of simulation codes and new experimental inputs from testbeam studies are proposed. This is very ambitious, and relies essentially on only two people: Wenzel and Para. Wenzel will perform detailed comparisons between GEANT4 and MCNPX, while Para will test crystals in low energy proton beams at JINR. Results from these studies will then be used to test shower simulations, in particular non-linearity of hadronic response.

### **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 principal investigators are researchers of the highest caliber, with decades of experience in calorimetry and particular expertise in inorganic scintillators. This includes, between them all, work on a number the calorimeters in several different experiments. The crystal development will be under the direction of Ren-Yuan Zhu, who is an expert in this area with an extensive record of R&D in inorganic scintillators.

The facilities outlined for the characterization of inorganic scintillators at CalTech is extensive and suitable for the purposes of this project. The other portions of the proposed R&D effort, including photo-detector development and simulation, will be carried out at two national labs, ANL and FNAL, and will draw on the extensive resources there.

### **4) Reasonableness and appropriateness of the proposed budget.**

Due to the nature of this collaborative project, the proposed budget is lengthy and complicated, including awards to CalTech, ANL, and FNAL, with a subaward from CalTech to SIC in Shanghai. The proposal received by this reviewer only contained detailed budget pages from ANL and FNAL, although the CalTech budget was specified in the CalTech budget justification. It was somewhat difficult to piece these together and to make sure the number added up to the total request each year; fortunately I did get the totals to match those given on the cover page. The requested budget is generally in the range of \$140k to \$209k per year per collaborating institute, with only a 4 month request from ANL in year 1. This seems generally appropriate to the scope of the R&D project described. One could be nit-picky and question whether crystals and photo-detectors should be listed in the FNAL budget, or if these could be

procured from the CalTech and ANL budgets, but this amounts to a small portion of the budget.

Many figures in the budgets are very “round”. For example the CalTech portion works out to exactly \$150k each year, despite variations in the internal breakdown of this total (a remarkable feat of spreadsheet mechanics!) However the totals appear to have been carefully thought-out, and requests for individual items such as travel (spelled out in an acceptable level of detail in the ANL and FNAL sections) are appropriate.

In addition to the three principal investigators, who receive small amounts of salary support from this project, the proposal would also fund portions of the time of three engineers at CalTech, 40% of a postdoc at ANL, and 40 to 60% of the time of a computing professional at FNAL. This level of manpower certainly is appropriate to the level of activity, although I was a little surprised to see only engineers supported at CalTech, and no graduate student support. I would have thought that the characterization studies would make a good project for a Masters or beginning PhD student.

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

The proposal is very wide in scope. Reading it, I kept vacillating between the opinion that it should be three separate proposals, or that it should be part of a large calorimeter development consortium. I think that the series of workshops current and proposed which are described in the proposal, should be used as the basis for building a larger HHCAL or TAHCAL R&D consortium. This reviewer would in fact be interested in joining such an effort!