High-Resolution Hadron Calorimetry Review

Executive Summary

A review of the Total Absorption Hadron Calorimeter (TAHCAL) was held on August 2, 2010. The review committee heard presentations on the detector concept (Adam Para), the crystal development (Marcel Demarteau), the readout electronics (Paul Rubinov), and the simulation project (Hans Wenzel). The proponents were asked to discuss the current status of the detector R&D and the near-term/far-term project plans under different funding support scenarios.

The proponents described their past and current work on TAHCAL and outlined a R&D program that would culminate in the test of a 1-2 m^3 prototype. Their previous work has produced interesting test results on dual-readout calorimetry and photo-detectors and electronics for the aforementioned. In addition they described the work that has been completed on simulation studies of this type of calorimetry. Developed within the SID software framework for the SID ILC detector, the dual readout calorimetry simulation is already at a very mature stage with the group showing comprehensive simulation results for calorimetric performance (single particle, electromagnetic response and physics analyses) of "test" systems based on the known crystals BGO and $PbWO_4$. The group is requesting a ramp-up in effort in the first two years of the program to roughly 6 FTE and $500k in M&S per year. This would support software, calorimetric prototyping, electronics development and materials (crystal) development. A significant additional ramp-up in years 3-5 would be required in order to achieve the goal of producing the 2 m^3 prototype (estimated at a cost of $20M).

While the committee agrees there is general interest in continued R&D into high-precision hadron calorimetry, the case for the proposed research plan was not made. The Fermilab group's focus thus far has been on the photo-sensor, readout, and simulations, with only minimal involvement in crystal development. At this point, however, those aspects of the project depend on the choice of crystal, whose near-term development is less than certain. For the overall R&D in this detector technology to be successful, a stronger effort in crystal development is needed. Fermilab management must assess whether the laboratory is interested in leading this effort, since the laboratory would need additional expertise in this area to successfully lead this research front. For photo-sensor and readout electronics development, the committee advocates a more synergistic relationship with other near-term experiments to develop prototypes that can further the R&D without launching parallel efforts. The simulations software seems very mature. Further development in this area would be difficult without knowing the crystal parameters. The Fermilab group is the international leader in this area. The laboratory should put in sufficient resources to document and maintain the existing software.

The proponents were asked to address specific charges. This report summarizes the committee's findings and recommendations to the charges.

Charge

1) Assuming the physics case has been made for high precision hadron calorimetry, is this R&D worth pursuing by Fermilab? Objections could be related to cost or unobtainable performance by materials, for instance. In addition, please assess whether our efforts would be unique in the national program, or redundant.

Findings
There is a very clear physics case for high-precision hadron calorimetry at a future energy-frontier lepton collider. The dual-readout calorimetry technique has been pursued by the Fermilab group, as well as nationally and internationally by others, and is a very promising candidate technology. Simulation and test beam work indicate that this technique has a good chance of reaching the desired resolution goal, although there still are some significant technological hurdles that must be met. The committee agreed that this is certainly an area of investigation that is worth pursuing by Fermilab. There is a small, but dedicated group at Fermilab that is committed to this effort, but at the moment it is not clear that they have the critical mass required to make significant contributions in all areas to the on-going international effort. The technical work that has been done by the group in the areas of SiPM studies and readout electronics development is certainly not unique. On the other hand, the software development has been noteworthy and the group has taken a leadership role in the international community. Currently the group lacks the expertise to contribute to the most technologically critical aspect of the R&D, namely the development of new crystal scintillators with the required scintillation and Cerenkov properties.

Comments
Without a significant addition of scientific effort to the group, it would be difficult for Fermilab to take the lead internationally in this R&D and make major progress on all fronts of this project. This increase of effort would have to include broadening it into crystal development and strengthening the project management component within the technical areas of the project. The crystal development aspect of the R&D is the most critical component and will drive all other aspects of any future effort. If Fermilab is to increase (or even continue at the current level) the work in this area, an expansion of scope into crystal development would be needed. Furthermore, in general, R&D on high-precision hadron calorimetry is not time critical in that the ultimate application is for an energy-frontier lepton collider, which is probably 20 years away and no near-term experimental applications or needs have been identified. It would be difficult to make the case for redirecting resources from other detector R&D areas.

Recommendations
Fermilab management must evaluate whether 1) this work is important enough to redirect resources away from other projects, 2) the laboratory is willing to expand the effort into crystal development and 3) the laboratory is willing to make a multi-year commitment to the effort. If the answers to 1-3 above are negative, then it is difficult to recommend an expansion of the effort (or even continuation beyond the level needed to document and maintain what has been done in the past).

2) What are the results of the last 2 years of effort? Were the resources used (personnel and M&S) commensurate with the deliverables?

Findings

Over the past 2 years, the group has been involved in software development and simulation studies, test-beam studies with protons of small $PbWO_4$ and BGO crystal matrices read out with PMTs and SiPMs, the evaluation of photo-detectors and readout electronics, and a collaborative effort on total absorption hadron calorimetry R&D within a large international community. The simulation and software work is well developed and has reached a point where further progress would first require input from the crystal development effort in order to specify the expected parameters of the final detector (crystal). The proton beam test has yielded some interesting preliminary results on the applicability of SiPMs in this type of detector and has produced measurements of the scintillation and Cerenkov light from BGO. These data seem to indicate that the rise time of the scintillation light is slow compared to the Cerenkov signal. This could be explained either by the slower development of hadron showers, or, more interestingly, by a slow turn on of the BGO scintillation signal. If it is the latter, then it would be a very interesting result and should be confirmed with an electron beam. Finally, there was some work on readout electronics, consisting mostly of evaluating designs from other projects.

Comments

The simulation/software work produced first-rate results and is currently at a very mature level. The effort here is certainly commensurate with the deliverable.

The detector prototyping and test beam work has made some measurements that hint at some interesting new results. Much of the effort went to building up the infrastructure for test systems, prototypes, and actual test beam work. It is somewhat harder to understand the total amount of effort that went into this part of the R&D, but it does not seem excessive for the deliverables

3) Assuming Fermilab continues to invest in this R&D, what role should we play? Should we attempt to lead the national program through scientific involvement? Should we limit our investigations to only a few areas (i.e. software, test beam, etc.)?

Findings

The Fermilab group has worked on photo-detector development and readout, electronics and Monte Carlo simulations. No work on crystal development has been done, beyond establishing an agreement to purchase doped Lead Floride and BSO crystals that will be developed by Shanghai SICCAS High Technology Corporation. The test beam, photo-detector and electronics work is at a low-level when compared to some of the other efforts in this area, such as the Dream Collaboration. The Monte Carlo work done at Fermilab, however, represents a major contribution to the global effort.

Comments

As discussed above, for the Fermilab TAHCAL R&D to make major progress and for the group to contribute significantly to the global effort in all areas, a much larger effort is required, including an expansion of the crystal and material development component of the work. A limited role in this could be considered, such as providing test-beam facilities and test stands, for example. Fermilab's involvement in GEANT IV development is appropriate and well suited to the laboratory's expertise. However, continuing this software development for TAHCAL is not justified at this time, since the tools are already well advanced and the next iteration would require input from the crystal development program, which will not be available for some time. The electronics readout and ASIC development can also advance independently of the crystal development, but will progress more rapidly through closer collaboration with other nearer-term projects that are using the same technology.

Recommendations
Limited involvement in the R&D by providing test beam facilities and possibly by developing some test stands for evaluation of small detectors is appropriate for Fermilab. TAHCAL SiPM photodetector studies and electronics readout R&D could benefit from synergistic activities with other groups at the lab investigating similar technologies and should be encouraged. Expanding the R&D into the crystal and material development would be in Fermilab's interest, but would require additional M&S and new personnel with the right background and a more coherent organization. The committee believes that the TAHCAL R&D project may not be the optimum program from which to further this development effort. However, there are other experiments with an interest in crystal calorimeter development and collaboration on these projects would be a reasonable direction for the laboratory to take.

The proponents have indicated that they could lead the national effort in crystal development. While such a program would be of interest to the laboratory, the level of technical expertise within the group would need to be greatly extended for this to be successful.

4) Evaluate the proposed 2 year and 5 year plans. Are the goals consistent with the role suggested by the answer to question 2? What is achievable with the current level of support (both labor and M&S)? What would be achievable with the proposed increased support? Do we have the personnel to carry out the program? Where are we weak? How can we resolve this weakness?

Findings
For the next 2 years, the request is for 6-7 FTEs per year plus $500k in M&S per year. This level of support would sustain the simulation/software development, build additional calorimeter prototypes for bench and test-beam tests, continue development on readout electronics, continue work on crystal development by funding the work outside Fermilab, and would provide some support for outside collaborators.

In years 3-5 they request support to build a full scale (a few m^3) prototype of a TAHCAL and put it in a test beam. The estimate for this task is $10-20M.

Comments
Regarding the plan for a full scale prototype in years 3-5, it is the view of the committee that such an expenditure is not justified on such a demonstration device. There are several reasons that the committee took this position, one of the most important being that without the context of

a real detector with definite physics objectives, mechanical constraints, and specific material characteristic constraints, a demonstrator project would not provide much new know-how to the community. The physics of hadronic and electromagnetic showers is generally well understood, and we agree with the proponents that the unknowns are in details of the material response, which, by definition, cannot be generalized. Consequently, although the committee agrees that high-precision calorimetry will be important for HEP, neither the case for this particular type of calorimeter, nor for the additional benefits of such a large-scale investment, has been made.

In years 1-2 a large part of the effort request is for simulation/software work. Although the committee was very impressed with the amount and quality of the work that had already gone into the development, we do not consider the proposed increase in effort to be justified outside of the construction of a full-scale calorimeter. We do recommend that the work that has been done be properly documented and that the resources be made available to do this.

Dedicated readout electronics for ~1000 channels of Silicon PMs for the fully active calorimeter was to be developed. As part of this project a dedicated test station for large-scale testing of SiPMs was proposed. In the light of our recommendation not to proceed with the large scale calorimeter, we do not consider this line of activity to be justified at present. SiPMs are being investigated by several groups at the lab for specific detector applications and the case was not made for starting a separate parallel effort in this direction outside of the Total Absorption Calorimeter project. Work on the front-end electronics that accepts the SiPM (or other photo-detector) signal is not time critical, and development activities in other programs support this need. However, collaboration between the TAHCAL community and groups focusing on near-term projects that use similar technology should be encouraged in order for progress to continue on these front.

At the current level of support, the group could continue moderate studies of crystals and photo-detectors and possibly mount some limited test-beam activities. However, as indicated above, we do not support continuing the simulation/software effort at its current level beyond the level of effort needed to complete the documentation.

As was indicated, the group does not have the ability to contribute to the crystal development effort. We feel that this is an important area that Fermilab should be involved in.

Recommendations
We recommend that the laboratory invest in building up crystal and material development (physics & technology) expertise in-house. However, for Fermilab to succeed in this effort, serious consideration should be given to organizing this program and providing an appropriate level of scientific leadership.