CALICE Oversight Committee - Questions and Answers

Report section 3.1 para 1

Could you please expand on the differences in detector systems between the 2006 and 2007 CERN runs.

See section 4.3, para 2. In brief:

- Almost full transverse instrumentation of the ECAL, which is important for containment of hadronic showers. Only the front six layers were not fully equipped in 2007.
- Full instrumentation of the AHCAL with 38 layers instead of 24.
- Ability to translate and rotate ECAL and AHCAL, for angular and position scans.

Report section 3.1 para 2

Could you expand on similarities and difference between the scintillator and silicon ECALs. Is this a reasonable comparison?

A scintillator-W ECAL is a possible alternative to silicon-W. The version which our Japanese colleagues are proposing uses crossed $4\times1\text{cm}^2$ mini-strips. We believe the pattern recognition abilities, and hence particle flow performance, will be much inferior, but it is a much cheaper alternative. Hence, it seems important to test it together with the HCAL, for both electromagnetic and hadronic performance.

Report section 3.1 para 3

How many MAPS sensors/pixels and what area is proposed?

This depends on yield, but if there are enough, we would want to equip two layers. The dedicated fabrication run planned for the second round will produce 12 wafers, each with around 40 sensors, giving a total of around 480 sensors. The ECAL sensitive area in the technical prototypes will be around $20\times20\text{cm}^2$ so this would be the area we will cover. With sensors of size $2.5\times2.5\text{cm}^2$ then this will be around 64 sensors per layer and so we will need a total of around 128 sensors. This should be possible with a reasonable yield, but if the yield was low, we would cover one full layer (so as to still be sensitive to the tails) but only populate a smaller area at the centre of the second layer. The pixel size will be similar to that of the first sensor, which is size $50\times50\mu\text{m}^2$ so this would be around 10^7 pixels per layer.

Report section 3.1

Will the US funding crisis, severely impacting on FNAL operations, have implications for the 2008 test beam operation?

We have been assured that the FNAL test beams will be available. The beam operations are not funded from the ILC budget at FNAL and so should run throughout 2008 as expected. The fact that CALICE, as an external user, is an ILC project is not relevant to the FNAL funding situation.

Report section 4.2 para 2

You list 3 defects of the DESY run – double showers, halo and missing instrumentation.

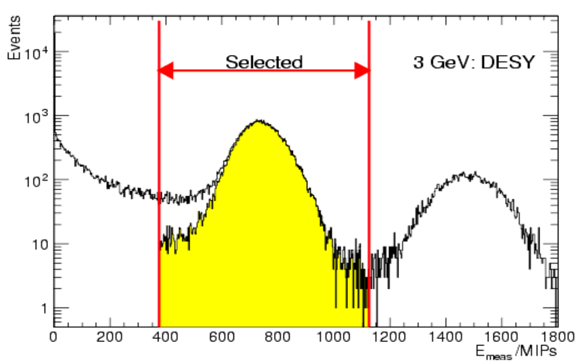
(a) Is it possible to estimate how much better the results will be if these problems are removed?

It depends what you mean by "better". Corrections can be made for the missing layers, but this depends on Monte Carlo – just what we really want to test. The issue with double showers and halo is that their energy overlaps the single-electron signal, so it is difficult to quantify exactly what their effect is.

(b) Do the double showers provide information on reality in a dense jet or are we reliant on Monte Carlo to estimate the degradation of performance?

Yes, these are a very interesting sample of events to study. The problem is that at these low energies the two-electron and single-electron peak overlap, so their separation using an energy cut, though pretty effective, is not perfect. What we would really like is a cleaner sample of single electrons, and then we can use the DESY sample to explore algorithms for nearby shower separation, and validation of this aspect in MC.

(c) Any plots to illustrate the difference discussed in (b)?



The open histogram is data and the yellow histogram is MC. In general the low energy halo, and the extent to which it leaks into the signal region, is the greater worry. The cut to separate two particles is not too bad, though it gets less clean at lower energies of course.

(d) What are double showers? What is their likely origin?

At DESY, this was due to pairs of beam electrons in the same bucket. In the plot above you can see that the energies of the two peaks are in the ratio 1:2. There is a different double shower problem at CERN, which appears to be associated with bremsstrahlung far upstream, giving a lower energy beam particle accompanied by a photon, so that the total energy seen is still effectively the beam energy.

Report Section 4.2 para 2

Discusses difficulties with comparing DESY and CERN runs. Is there any indication that they don't compare well?

Yes, but this is only based on comparing the lowest energy CERN beam (6 GeV) with the highest energy at DESY (6 GeV) as this was the only common energy is the two datasets. Neither beam is clean. Hence, it is hard to make an unambiguous statement. This is why the FNAL beam would be so helpful.

Report section 4.3 para 3 line 3

Roughly what level of proton contamination is expected?

It's not contamination – the proton data are very interesting in their own right to allow us to compare data with Monte Carlo for both mesons and baryons. We estimate the proton contribution is typically 15-25%, depending on energy, in the range 30-80 GeV.

Report section 4.3 para 4 line 11

'longer timescale' – how long? See also question above for differences on 2006 and 2007 running above

It is hard to be precise on publication timescales, but probably by the end of 2008. The differences between the 2006 and 2007 systems (described above) mean the 2007 samples will be more useful for most studies as the calorimeters had better coverage and depth information.

Report section 4.4 para 2 line 3

Just to check, what does average hadron energy mean in the context?

It is an indication of the mean energy of hadrons in jets for multijet events at a linear collider (the kind of events like t-tbar, ZHH, WWνν) which are of greatest physics interest. The jets themselves are clearly higher in energy, typically from 50 to 100's of GeV.

Report section 4.4 para 2 line 7

'Long-term stability checks against the CERN results' – intriguing. How useful?

We are designing a detector to operate for ~10 years, so when we have the chance to make comparisons over a two-year period using our prototypes, this seems rather useful. The most direct comparison will be of the calibrations using muons, for which the problems with the beams discussed above are not relevant. For the ECAL, the first calibrations were done with cosmics in 2004 so a comparison over four or more years should be possible in this case.

Report section 4.6

How confident are we that there are not going to be other calls on this allowance in addition to the two RA extensions?

The document gives the current best understanding of our needs and hence *is* the call for RA extensions. If the project remains on the schedule given in the Gantt charts then we do not foresee any further calls. Clearly, if there are changes as the projects progress through FY08/09, then we cannot exclude a further call, but this would only be done if it could be funded within the remaining WA budget.

Section 4.6

When does the PDRA post come to and end currently?

G. Mavromanolakis' post ends on 30 June 2008.

Report section 5.2

Can you indicate for us what sorts of issues are delaying the ASIC development?

This is mainly overcommitment on the part of the (French) engineers. The group took on the ECAL ASIC but then decided it would be useful to also have the DHCAL and the AHCAL readout ASICs designed in a consistent way. This will make the UK DAQ interface to the three detectors much more uniform and we encouraged this development. The DHCAL chip is the easiest as it is binary readout and that was produced quickly. The group then decided to do the AHCAL chip next as the internal ADC requirements (in terms of dynamic range and noise performance) are significantly easier than for the ECAL. Hence, the ECAL chip got pushed until last and a fully-functional version of this is now in design, with submission planned for September 2008 and the ASICs back in November. This pushes tests into 2009. The ECAL ASIC schedule is consistent with the overall EUDET schedule and hence our schedule given in the Gantt chart, but will have only one iteration, not two. While this is clearly more risky, the ASIC shares a significant amount of design with the DHCAL and AHCAL chips and the risk is considered acceptable.

Time scales look late. What happens if there is a further 3-month delay beyond project end-date of 31 March 2009?

The development of the ASICs is constrained by the timelines of the EUDET project. Therefore we expect the schedule presented here to be met. If the development were to slip, then we would either have to drop this task or include it in any future bid. This would clearly also delay the installation of the ECAL in the EUDET structure. Whether the beam tests would proceed with only the AHCAL and DHCAL, with the ECAL joining later, would have to be decided at the time; clearly the UK is not the only party involved.

Report section 5.3 para 2

What do we lose by omitting the second round of tests? Does it mean improvements will be devised but not tested? Are there risks associated with loosing the second round of tests?

The results of the tests made on this board will feed directly into the prototype being build by the CALICE collaboration as part of the EUDET project. Therefore as tests will anyway be made on a far more realistic detector, continuation of our model slab programme has become redundant. We are therefore moving our effort to contribute to significant parts of the prototype.

Report section 5.5

Saying that we could not use an off-the-shelf solution so we take on more responsibilities rings alarm-bells. What drove us to this? What resources needed? What risks?

One of the aims of this workpackage is to use off-the-shelf solutions wherever possible. The clock and control aspect currently looks difficult, timing in lots of different components all with different timing structures, so we plan a bespoke solution. As the bespoke solution is the standard way of the doing things in HEP experiments, this presents no major issues. The UCL group has vast experience in providing clock and control systems (ZEUS vertex detector, ATLAS SCT) so for this relatively small system only basic electronics will be required.

Report figure 3 on p 9

I can see the gain is higher in the left-hand figure but is it really more uniform? How is uniformity measured?

The left hand diagram clearly shows a larger signal magnitude. However, it also shows a relatively smooth drop of signal size over the size of the pixel (50µm) and there is no visible structure on smaller scales. The right plot shows two peaks corresponding to two of the four charge collection diodes which are separated by 30µm. Hence the signal response is varying by close to a factor of two within this range, indicating a much large non-uniformity.

Where are the pixel boundaries and are all 3 pixels included in the readout?

The sensor has six metal layers which fully cover the circuit side of the sensor which is then blind to optical illumination. Hence, all measurements are done from the substrate side. This has no features (even the sensor edge is not visible) and so there are no fiducial marks to indicate exactly where the laser is relative to the pixels. The position has to therefore be deduced from the response; for the left hand plot, the peak of response is the centre of the pixel being measured and for the right hand plot, the centres of the green peaks indicate the positions of the charge collection diodes.

This measurement was done by measuring the signal size from a scope and so was only semi-automated. (This is very time-intensive and is why the area scanned in the two cases is not exactly the same.) When the measurements were done, there was only the necessary equipment (low-noise amplifier, etc.) to measure one pixel output at a time so each plot shows the response of one of the three pixels to the laser. It is planned to automate these measurements and repeat them to cover a larger area corresponding to all three pixels, while reading all the test pixels out.

Report section 6.1

Given the time since arrival of the sensor, I would have thought a number of other key bench tests must have been carried out using the integrating sphere facility at RAL or other uniform light source to measure dark-field, pixel-to-pixel gain uniformity and linearity, response speed and to compare these with specification? Are standard calibration techniques (photon transfer for example) possible with binary readout? Is there any on-chip calibration? How do the all important noise and gain compare with specifications?

As described above, the sensor is optically blind when front-illuminated so illumination must come from the backside. This precludes the use of visible light for testing as this would be absorbed by the thick (several hundreds um) substrate. Infrared can still be used if an appropriate wavelength is selected. The sensor architecture does not allow the implementation of image sensors standard calibration techniques because of the binary readout. For an accurate measurements of the parameters mentioned above, we have to rely on a pulsed laser or on radioactive sources.

The gain and noise have been measured directly using the analogue outputs of the test structures. The noise was measured directly from the analogue output to be 3.5mV. The gain was measured from the response to an ^{55}Fe source which gives 6keV X rays, corresponding to 1600e^- in the sensor. The signal size measured is 210mV, which is not too far from the expected value of 290mV. For comparison, a MIP would give 1300 e^- in total, although due to charge diffusion, on average only around 800e^- are collected in the centrally hit pixel. In the worst case, only 300e^- per pixel are collected if the hit is right in the corner of four pixels.

One major outstanding issue is that the gain seen in the test pixels has not yet been confirmed in the bulk pixels. Fig. 4 in the document shows the noise in the bulk pixels is at the expected level, if not a little better; the predicted 5σ threshold of 150 counts would give a noise rate below the target of 10^{-6}. However, this figure also shows no significant structure as a function of threshold in the response to the source, suggesting that the expected MIP peak is either too low to be observed or has been washed out by charge sharing. It is hoped that analysis of the beam test data may clarify this issue but results are not yet available.

The laser system should also be able to help with these measurements. However, so far, the readout system which will be used has been occupied with the analogue measurements of the ^{55}Fe source discussed above and with taking the data for the uniformity measurements, as shown in fig. 3.

Report section 6.1.3

Is there now a better understanding of the double band structure in Fig 5?

On further analysis, this was found to occur in several runs and so cannot be a mechanical shift of the sensors relative to each other. It still needs to be understood. Some corruption of the on-sensor memory bits has been observed due to a problem with the SRAM voltage used to write to the memory (the only significant design error found so far), but this has been seen at the 10^{-3} level and is isolated in particular regions of the sensor. Hence, while this ghost band could be due to a single bit occasionally being mis-set, giving an apparent constant shift in the position, the rate of occurrence and the fact that it happens over a wide range of positions in the sensor would seem to be inconsistent with the direct corruption measurements.

Report section 6.2

Why has it been decided to carry out the ECAL tests within the EUDET structure?

We are not sure if the question means as opposed to the existing beam test structure, or as opposed to a stand-alone test. If the former, then this is simply that the existing beam test structure will no longer be in use by this time as the CALICE-wide focus will have shifted from the physics prototype tests to the EUDET technical prototype tests. In addition, the MAPS sensors have to run with ILC-like bunch train timing, which is what is planned for the EUDET DAQ and is completely different from the triggered operation of the current beam test system. Hence, the integration should be much simpler.

If the question is asking about a stand-along test, then firstly, there will be a stand-alone test before the EUDET test (as described in section 6.2) , but it was never the plan to have enough MAPS sensors to equip the 20-30 layers required to fully reconstruct showers. In addition, one of the goals of these tests is for a direct comparison of the diode and MAPS responses in the same structure and that would clearly not be possible in a stand-alone test.

You say there has been a three-month delay but no further slippage and then ask for a six-month extension. How certain is the June 2009 startup for ECAL tests? If it is not delayed what will be your contribution and your benefits? What if it slips three months?

The ECAL tests could be delayed (for instance by delays to the ASIC as discussed above) but the schedule presented is the one we are currently working to. Clearly, if it slips further, then it would have to be includes as part of the follow-on proposal assumed when the document was written. The three month delay is to the UK part of this programme compared with the original schedule. The beam test with the other ECAL has moved significantly because of the change in the overall CALICE plan; EUDET did not exist at the time of the original proposal. This is what was meant by “other external influences”; apologies it was not clearer. Hence, the extension is to cover the change to the schedule for the final beam test, which is not determined by the UK alone.

Last paragraph of this section – surely you are asking for 11 months not 9? Are the costs correct?

Apologies; there is a typo here. The 9 months and the cost are correct, but the last line should read "to the end of July 2009" not "June 2009". However, we may have misunderstood the question as June would have been an 8 month extension since, as stated, the post is currently funded to the end of October 2008; we don't follow why 11 months would be correct, even with the typo. Apologies if we are missing something here.

Note, it would obviously be ideal to extend the post to well beyond the beam test, e.g. the end of 2009, to fully cover the analysis as well. However, firstly, we are being conservative with the use of the WA and secondly, the document was written when there was an assumption that there would be a follow-on proposal and (hopefully) grant being submitted in 2008 which could pick up these programmes in FY09/10. If this occurred, the RA post would clearly transfer to the new grant.

Report section 8

Please update us at the meeting re your physics and simulation studies

The physics and simulation studies have continued to make good progress, in particular with the algorithm development of PandoraPFA now being firmly established as *the* de facto standard against which other emerging PFA codes are compared.

Specific adjustments in the project plan to completion include:

- Updating significantly the fraction completed for ID 9, to better reflect the level of performance achieved by this sophisticated algorithm.
- Extending ID 10 until Aug. 2008, for consistency with the anticipated schedule (as at Dec. 2007) for submission of the Letters of Intent (LoI).
- Completion of ID 17, recognising the remarkable inroads made to the SiD group in particular via the MAPS WP for calorimetry.
- Tasks 21—24 are removed from the forward plan, given the actual uncertainty with future funding, it will not be realistic to direct the necessary effort into these tasks. They have lower overall return to the project than any of the other high priority activities, in particular PFA algorithm development and testing, MAPS implementation and simulation of the Dec. 2007 MAPS testbeam at DESY and simulation/tests of hadronic interaction models using the combined CALICE testbeam data.
- Progress on ID 23 (previously ID 27) has been updated substantially to reflect the remaining required work – significantly, the Mokka implementation of the design has yet to appear in a standard, publically tagged release of the code.
- Support for MAPS testbeam has been reduced in time, to complete by Sept. 2008.
- ID 33 (previously 37) has been updated to reflect the fact that the means to carry out large scale MC production of samples to support the analysis of 2006/2007 testbeam data has been established and is now being tested at DESY.

While plans for a merger of these activities with LCFI have been dropped for the near term, the situation regarding the future of this WP is still uncertain to a large extent. If STFC continue in their stated action of not allowing ILC-specific work to be funded, then development of PFA may still continue as this activity is largely carried out without direct support from STFC anyway. While the algorithm development per se is generic, its testing and benchmarking is necessarily detector concept specific and therefore directly linked to ILC. It is obviously not possible to benchmark complicated algorithms without a specific implementation of the detector model. Further updates will be made as more information on the funding becomes available.

General

When the project RAs have completed their work and left:

(a) Will the remaining staff be able to complete final publications?

This will be possible as there is significant academic effort active in both the beam test (for WP1) and MAPS (for WP3) analyses. For WP2 and WP4, the expertise is mainly in the Rolling Grant staff who are expected to remain. For WP5, the work is to a very large extent driven by academic effort, most obviously from M. Thompson, and this will continue, with the emphasis on contributions to the detector concepts more than publications.

(b) Which individuals will retain the expertise long-term for eventual use?

As usual, the expertise will be retained by the academic and Rolling Grant staff. Ideally, we would also keep on the RAs if/when a further grant is awarded but we cannot of course guarantee that they will not take jobs elsewhere.