CALICE Oversight Committee – Questions and Answers

Report section 4.2 para 1
How serious is the loss of studies due to digital readout? Is there a recovery plan? How will you learn about mutual inductance oscillation safety in large systems?
and
3) Can we explore a little more the consequences (section 4.2) of having only the digital FE for the studies of the ECAL? What does "digital" exactly mean here? Why are studies with a variable threshold not able to reproduce some of the information that an analogue chip would (much more easily) provide?

This section was obviously not as clear as it could have been; apologies. There is no intention of only reading out the standard ECAL with digital (i.e. binary, single bit) readout.

The LAL/Orsay group is producing three ASICs in parallel; an analogue ECAL, an analogue HCAL, and a digital HCAL version. The input section for each will differ but the output part, which talks to the downstream DAQ, will be as common as possible. Hence, in working with the digital HCAL ASIC, then we are able to understand many of the technical DAQ issues in clock, control and readout of data, which will then be applicable to the ECAL ASIC when it is fabricated. However, we will not be able to measure much of the ECAL ASIC analogue performance with the digital chip, of course, and this will have to wait until 2008. By working with the digital chip, we hope to be better prepared for the ECAL chip when it is available. The main impact will be that we will only be able to feed back results to the LAL/Orsay designers for one ECAL ASIC development round rather than the two rounds originally planned.

Report section 5.3
The successful treatment of a cluster as a m.i.p. suggests that the pixel size is smaller than needed, and the system is overdesigned. Please make a crude study of the effect of doubling the pixel size on (a) physics performance [merge two pixels into 1 in readout to get a quick impression (b) cost – is readout easier?

The studies with clustering are done for photons with 10-20GeV energies as these are the energies most relevant for jet reconstruction at ILC energies. For these, then the above statement is probably correct that the physical pixel size is smaller than required. However, another issue driving the physical pixel size is pileup leading to non-linear behaviour in much higher energy electromagnetic showers, where the MIP density in the core of the shower becomes comparable to the pixel size. The current pixel size was chosen to reduce the non-linearities at these energies.

We are not able to get results from the suggested study in the few days available before replying to these questions, but will try to include some results in the presentation on the day of the OsC meeting.

There is another issue related to size, which is the charge diffusion and collection. A larger pixel with the same number (currently four) of collection diodes will require more time to collect the charge and will have a lower collection efficiency. Adding more diodes, so they are spaced apart by the same distance as for the current design, would get round both these problems. However, this would lead to a significant increase in the input capacitance and hence noise. Therefore, increasing the pixel size and keeping a reasonable signal/noise may not be easy. Note, the DAQ rate will be noise dominated so this will also have downstream implications also.

The cost of the MAPS calorimeter will only depend very weakly, if at all, on the pixel size. The wafers will require the same number of masks (i.e. processing steps) and the cost is not influenced by the complexity or number of features per unit area. It is true the downstream DAQ readout may have a higher rate (although note the comments in the previous paragraph) but the current rate estimates are not hitting the limits of the DAQ readout so any extra cost would be small. The major gain in increasing the pixel size would be in the reduction in power, as effectively the same circuitry (and hence power) would be required per pixel but this would be spread over a larger area.

Report section 6.1

Glue: what studies of accelerated aging – how big are the samples? What studies of radiation hardness? Can we see the report sent to LCWS in May?

The report at LCWS was verbal to other collaborators. A written report is in progress and should be available for the OsC meeting. OsC questions will be covered in the report. We intend to leave the long-term tests running from now in any case – and that will include the "virgin" glue joints. However, as was always our intention, we now wish to concentrate on mechanical assembly issues.

Timescale

Some external factors are pushing things later. What risks are there of losing milestones due to drift beyond Mar 2009?

There is clearly a risk that some of the milestones very close to the end of the project will not be met. Those that we currently believe could be in this category are as follows:

- **Workpackage 1**
  - **WP1.37** Submit paper on hadron results (Mar 2009). This is dependent on the success of the FNAL test beam run in 2008 and requires the agreement of our collaborators that the analysis is complete.
- **Workpackage 2**
  - **WP2.7** This has been retired because our French colleagues have not been able to deliver the ASICs.
  - **WP2.12** The second phase of ASIC testing, is therefore at risk since we will only have one round of testing. However the ASICs are not a UK deliverable and therefore the only risk that the UK carries is that they are delivered and we cannot test them, which we believe is very unlikely.
  - **WP2.34** Complete draft of TDR section. It is no longer planned to produce a TDR by this date and we will instead produce a Linear Collider Note.
- **Workpackage 3**
  - We are currently about two months behind schedule because of the additional work on the development of the deep p-well process. While we hope that

experience from the first round will enable us to catch up the lost time we recognise that may still not achieve the Mar 2009 end date.

  - We have already identified that a failure of a fabrication round would require a further round and produce increased costs. In this unfortunate circumstance, it would then be highly unlikely that we would achieve the Mar 2009 deadline for completion of testing.

- **Workpackage 4**
  - None.
- **Workpackage 5**
  - **WP5.37** Report on hadronic modelling studies with testbeam data. This is potentially at risk as this is linked to data taking at FNAL and production of such a document requires approval by the Collaboration as a whole, therefore is not entirely within the control of the UK groups. Any delays in test beam schedule would have a direct impact on this.

Finance

What are the most significant unknowns affecting cost?

Might you bid for release of unspent working allowance to take some items beyond Mar 2009?

We have identified two risks with associated costs in the Risk Register. The only other significant cost uncertainty is the test beam system for the final MAPS beam test. While £60k has been assigned to this work we estimate that it is uncertain to about £10k.

We would like to explore with the OsC how any unspent working allowance might be committed to the project and when. In particular, if it appears likely that we will require further PDRA effort on any aspect, how and when we can commit this.

1) Figure 2 left. There look to be some minor discrepancies around the mip peak between data and MC. Is there a small excess of energy in MC compared with data?

The total energy and number of hits are in fact very well modelled. The discrepancy noted by the OsC largely affects the width of the MIP peak. The simulation shown here did not take account of channel-to-channel variations in noise, or of the statistical uncertainties in the gain calibrations in data. We suspect this is likely to account for part of the discrepancy.

2) Figure 4. Given the parameterisation of the data and MC is it possible to tabulate the fitted parameters c, alpha and beta and to see how well these compare between data and MC? (It all looks fine though but this might make it easier to interpret.)

The general shape, and its evolution with energy is well modelled. However, there is a small systematic shift, with the showers developing slightly earlier in data. Our current suspicion, supported by some other discrepancies seen between data and simulation (for example Fig 5 of the report), is that there may be showering of the beam particles in additional upstream material which we have not yet simulated. This is a subject of active investigation.

4) WP3. I note the second sensor design review is now expected Dec 2007 but I think this still implies a very busy schedule for evaluation of the first sensor. The test programme includes, laser, source and testbeam but I would have thought a focus on the results needed for the second sensor design review might be prudent. In particular, I think that as the signal/noise for a minimum ionising particle is not comfortably high, it will be important to evaluate the noise and absolute (mip) calibration as a priority. (ie be sure of getting results with a source since I worry that cosmic runs could take far too long). My worry is that many factor can contribute to making the noise higher than specified and this is difficult to get right in the device simulation. Getting system noise down also takes a lot of effort.
Checking key performance specifications against the device simulation should, to my mind, be the priority, particularly before any further designs are started in earnest.

Firstly, apologies, but there is a typo in the heading of Sec. 5.5.5; milestone ID14 should be titled "Second sensor **interim** design review" to distinguish it from ID15, the final design review, which is discussed in Sec. 5.5.6. Hence, the final second sensor review is actually in Mar 2008 rather than Dec 2007. However, the question could still be considered relevant as even aiming for a review three months later, the test schedule is still not relaxed. We very much agree understanding the performance is the highest priority and the tests, detailed in the PDR in May 2006, are aimed at those measurements. The noise as a function of threshold will be the first thing measured. However, this is of little use until it is calibrated against the MIP efficiency as a function of the threshold. We regarded this as needing both the source and the cosmics tests. The source will be ^{90}Sr and so will use low energy electrons. These will deposit an energy not trivially related to MIP deposits but can give a reasonably high rate and so can give a relative calibration across the sensor. The cosmics will give a much lower rate but can then be used on the whole sensor to give an absolute calibration. The laser system is to calibrate directly the sensor-level simulation in significant detail in terms of the charge spread, diode absorption, etc. All three sets of tests will run in parallel at Birmingham (cosmics), Imperial (source) and RAL (laser), and so prioritising them should not be necessary unless we hit serious problems with yield and only have a small number of functional sensors.

The beam test is in principle not needed for the development of the second round of sensor design. However, there is a long-standing issue (in fact raised by one of our original PPRP referees, Steve Watts) concerning the rate of soft photons in electromagnetic showers. The MIP deposits being detected in the MAPS are around 3keV, compared to 60keV in standard diode pad detectors. Hence, MAPS are more sensitive to the details of the soft photon flux in electromagnetic showers. While the GEANT4 simulation indicates the rate is low compared to the MIPs, there is a concern that the simulation has not been checked at such low energies in energetic showers and that there might be a higher flux than expected. This in itself would not prevent the use of a MAPS calorimeter as the flux would be expected to scale with the shower energy. However, if the photon spectrum is steeply falling over this energy range, then it would require a very uniform threshold on each pixel (or, more correctly, a very uniform average threshold for all the pixels hit within each shower) in order not to degrade the resolution. An alternative would be to line the rear side of each tungsten converter plate in the calorimeter with a thin foil of material which has a high critical energy so as to absorb the soft photons emerging from the tungsten. We think it is essential to get information on such effects as soon as possible and so intend to proceed to a beam test using the first round sensors.

5) WP4. How consistent is the glue joint aging with that which caused CMS to abandon this method of making the HV contact and led to major concerns in ATLAS? Is the conductive glue the same as those studied in ATLAS? Should the dots be left unpowered for very long durations to check for the CMS/ATLAS aging problems. (Manchester people clearly have all the ATLAS experience to draw on in addressing this.)

and

4. Section 6.1: Can we have an update on progress in understanding the resistance behaviour of the virgin glue joints?

We believe most of the glue worries undergone by the CMS and ATLAS tracker communities were primarily due to artefacts of measurement involving the use of DVMs . These tend to use low voltages < 1V (to minimise current on low resistance ranges). We have observed that untouched new glue joints show initial high resistance behaviour if measured at low voltage. It is believed this is due to the existence of nanometer scale oxide films. Measured once at higher voltages, typically > 2V, the film is disrupted and conducting paths are permanently established giving the expected low resistance.

It is conceivable that joints left unpowered for very long times in lab atmospheres could redevelop such effects. It would indeed be instructive to look at glue joint resistance after a very long times (> a year). In the case of the ATLAS detectors we have a database of glue resistance values taken in the construction phase. It would be interesting to revisit them; however once assembled we have no direct electrical access to individual glue joints, since there are four detectors in parallel with series resistors. Any measurement would require the effective disassembly /destruction of the module. Similarly we cannot directly measure the glue joints in the CALICE beam test prototype. However, our test boards have been designed so we can directly remeasure individual dot resistances some time in the future.

As we understand it the CMS decision was to use wire bonding as well as the glue as a ‘belt and braces’ solution.

ATLAS Fwd SCT used Traduct 2902, as did CMS initially, which then switched to Epotek 129-4. In CALICE, Epoteck E4110 is used, following GLAST. However, we believe the resistance effects are generic. It is important to realise that the once off application of a small bias voltage seems to reset these problems.

5) WP5. It would seem that LDC/GLD vs SiD comparison would be most interesting from a CALICE perspective as the benefits to cost of the SiD solution for the calorimetry must be one of its attractions. Clearly, the angular resolution compared

with a large tracking volume is compromised but might UK physicists be able to help see what the SiD solution would cost in terms of calorimetry performance?

Making a comparison of the relative performance of SiD vs. GLD/LDC is clearly an area which UK physicists will be involved in, both for calorimetry performance per se and for more critical measurements based on jet energies. This is not a trivial comparison to make on a "like-for-like" basis given differences in the reconstruction algorithms and software frameworks between the concepts. However, for the next OsC report, we expect to have quantitative results.

A trivial one - can they clarify what the y axis is in Figure 11

The y axis of this figure shows the signal/noise for a MIP, where the signal is that predicted by the sensor-level charge diffusion/absorption simulation and the noise is predicted by the Eldo device-level simulation. The x axis shows the distance from the corner, with the points at x=10 being at the centre of the pixel. The drop of signal/noise near the corner is due to charge sharing between the four pixels; indeed it is seen that the signal/noise is down by approximately ¼ for MIP deposits in the corner compared to the value for deposits in the centre. The most critical thing this figure shows is that the signal/noise is predicted to be greater than 10 for all MIP deposit locations; indeed, for the 1.8μm diode size selected, it is greater than 15 everywhere.

In section 7.5.3 there is a comment that some other physics analyses are in preparation. Can they clarify what that means?

In addition to the two studies noted in Sec. 7.5.3, several groups expressed interest to develop new analyses when we reviewed the progress within the UK early in 2007. Several new channels, each of which would be relevant to test detector performance, were proposed, including $e^+e^- \rightarrow Z^0H$, with $H \rightarrow$invisible and $Z^0 \rightarrow q\bar{q}$ (Imperial), $e^+e^- \rightarrow t\bar{t}$ (complementary aspects at RAL, Cambridge, Birmingham, UCL), and extending the existing $e^+e^- \rightarrow ZHH$ analysis to a six-jet final state (RHUL).
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1. Section 3.3: Could we please have some information about the pion and muon beam energies used in the CERN test beam runs. Are the discrepancies that you mention in the 30 GeV electron data related to the data/MC discrepancy in the MIP peak of the 12 GeV pion data? Any progress with resolving these?

Data were taken with pion beam energies from 6 to 80 GeV. The muon data were taken whilst other detectors were being tested upstream of us, and were recorded with a broad parasitic high energy beam.

The two effects mentioned are not related. The discrepancy for the pions could well be caused by limitations in the MC simulations; for example the average noise was applied to all channels, and statistical uncertainties in the gain corrections were not taken into account. The electron data, increasingly at higher energies, shows a more sizeable discrepancy below the 1 MIP peak. This is suggestive of some crosstalk or coherent noise effect, which is not yet understood. This work is ongoing.

2. Section 3.5: Is there an update on the signal induced pedestal shift?

Not since the document was submitted. It appears as a cross-talk effect at the wafer level, correlated with signal, but appearing for a few wafers, apparently randomly in space and time. Investigations continue.

3. Section 4.4: Have the problems with the packet loss now been understood?

Not yet. We suspect losses in the Linux kernel as it has known deficiencies handling RAW Ethernet packets. Investigations will continue over the summer.

5. Section 6.2: When will the thermal studies resume?
and
6. Section 6.3: Which areas are the UK likely to contribute to in the ECAL assembly?

The proposed plan for mechanical work for the rest of the funding period is as follows:

- Long Slab Assembly
  - PCB sourcing from UK company (dummy PCBs for this work)
  - PCB stitching – electrical contact properties and mechanical robustness
  - Sensor mounting
  - Ground-foil wrapping
  - Instrument with resistors/thermistors to measure mimic ASIC heat production and measure representative temperatures in EUDET module
- Dummy H-structure production (simulates tungsten/carbon fibre structure)
  - To use as a base for slab assembly
  - Most likely use steel for test assembly work
  - If time allows, make enough dummy structures to carry out thermal measurements on EUDET module.

This proposal is still under discussion with our French colleagues, but we anticipate that it will be largely unchanged. Once details have been finalised we will update the Gantt chart to reflect this. As such, we will not be carrying out more thermal simulations but moving towards making real measurements on the EUDET prototype. We anticipate that developing these assembly techniques for the GLD/LDC LoI will position the UK to be able to take on final assembly of a significant part of the ECAL if the concept moves forward to construction. It could also lead to the UK designing and constructing the ECAL cooling system, but with the current manpower assigned to mechanical work we cannot undertake this at present.