Proposal 317 - CALICE: Response to PPRP questions

1 Physics and performance

1. *It is claimed that the top quark mass can be measured to a precision of 200MeV from an energy scan, yet the average energy lost to ISR is 25GeV at 500GeV, as well as beamstrahlung effects. How is the accuracy of the top quark measurement consistent with the uncertainty from the latter effects?*

   This is one of the results of the TESLA TDR studies [1], which certainly took into account both ISR and beamstrahlung effects.

   This measurement would be done from a threshold scan, which reduces the effects of these processes. Firstly, near threshold, any interaction with a significant loss of energy from either of these processes would not be able to give a $t\bar{t}$ pair and hence the effect is limited. Secondly, to a good approximation, both ISR and beamstrahlung are independent of the small changes to the centre-of-mass energy ($\sim$ 10 GeV at a energy of 350 GeV) used in such a scan. Hence, while there may be some energy loss, it would contribute an overall shift, not a local change in cross-section as a function of energy. The turn-on energy of the cross section curve can be determined to quite high accuracy ($\sim$ 100 MeV) for a scan with an integrated luminosity of 100 fb^{-1}.

   The extraction of the top mass from the curve then requires the unfolding of these effects, but this uses knowledge of the energy spectrum; the event-by-event variation is unimportant. The unfolding does not rely on theoretical calculations, as the effects of both ISR and beamstrahlung can be measured experimentally from the acollinearity of Bhabha events. This gives a luminosity-weighted measure of the energy loss, which is exactly what is needed.

   The QCD corrections to the top production are relatively low ($\sim$ 100 MeV) as there are negligible hadronic effects, because of the rapid top decay, and the q^2 of the subsequent b quark system is very high. This theoretical error may be further reduced in the 10-15 years before this measurement is made.

   The absolute energy scale is also needed, and the TDR includes a spectrometer which should determine the beam energy to an accuracy of better than 10^{-4}, which translates to 35 MeV around the $t\bar{t}$ threshold.

2. *Comparison to LEP hadronic calorimeters is really very naive: this should rather be done to HERA or LHC-type calorimeters for which resolutions of* $30 - 40\%/\sqrt{E}$ *are obtained at lower extrapolated cost. What would the gains of the energy-flow approach be in this case?*

   Estimates of the resolutions for hadronic jets and single particles for various non-LEP experiments are given in Table 1. For some cases, these are rough averages, e.g. over the barrel and end-caps or various energies, and others are verbal communications which are not published, so they should not be taken as definitive, but indicate relative performances.

   We assume the $30 - 40\%/\sqrt{E}$ resolution quoted in the question refers to the ZEUS single hadron value. As is obvious from the table, this is not equivalent to the jet resolution; the ZEUS calorimeter gives $60\%/\sqrt{E}$ for jets, which is similar to ALEPH and twice the value which the CALICE calorimeter hopes to achieve. The intrinsic physics fluctuations in the composition of jets limits the ability to simply extrapolate performance from single hadron results. In fact, none of the other calorimeters listed above gets close to $30\%/\sqrt{E}$ for jet resolution.

Experiment	Resolution σ_E/E		
	Single e/γ	Single hadron	Jet
CDF [2]	$2\% \oplus 14\%/\sqrt{E}$	$3\% \oplus 75\%/\sqrt{E}$	$83\%/\sqrt{E}$
D0 [3, 4]	$0.3\% \oplus 16\%/\sqrt{E}$	$3\% \oplus 41\%/\sqrt{E}$	$3\% \oplus 69\%/\sqrt{E}$
ZEUS [5, 6, 7]	$19\%/\sqrt{E}$	$35\%/\sqrt{E}$	$60\%/\sqrt{E}$
H1 [8, 9]	$1\% \oplus 11\%/\sqrt{E}$	$2\% \oplus 50\%/\sqrt{E}$	$\sim 55\%/\sqrt{E}$
ATLAS [10]	$0.7\% \oplus 10\%/\sqrt{E}$	$3\% \oplus 45\%/\sqrt{E}$	$3\% \oplus 52\%/\sqrt{E}$
CMS [11, 12]	$0.5\% \oplus 4\%/\sqrt{E}$	$5\% \oplus 65\%/\sqrt{E}$	$5\% \oplus 110\%/\sqrt{E}$
TESLA [1]	$1\% \oplus 13\%/\sqrt{E}$	$3\% \oplus 35\%/\sqrt{E}$	$33\%/\sqrt{E}$

Table 1: Performances of various running and future calorimeters.

The comparison to the LEP experiments seemed relevant to us. They are both e^+e^- colliders so the overall event environments are similar, e.g., there are no beam remnants. Even allowing for worse backgrounds at the linear collider, they are still both much more similar than a $p\overline{p}$ machine. The 500 GeV stage of TESLA is only a factor 2.5 higher than LEP-II. For many of the physics processes of interest, such as $\nu\overline{\nu}W^+W^-$ where for a centre-of-mass energy of 500 GeV the average W energy is around 100 GeV, the resulting jet energies are quite close to the highest energy ones seen at LEP-II. This is also true for the 6-jet states mentioned in the proposal. Although the calorimeters, particularly the hadron calorimeters, of the LEP experiments were not of the standard of the more recent experiments mentioned, the environment was clean enough that energy flow algorithms could be used for jet energy reconstruction.

ZEUS in fact do use an energy flow algorithm [13] to obtain the jet value given in the table. This makes full use of tracking information. The algorithm operates on the result of central track finding and calorimeter clustering packages. The clustering package identifies “islands” by associating calorimeter cells to local energy maxima in the calorimeter. GEANT3 simulations indicate that these objects provide the best estimate of particle multiplicity in the calorimeter. The algorithm then “swims” tracks from the central tracker, through the intervening material (which includes the coil in the central region) and performs a matching to islands based on estimated impact position at the surface of the calorimeter. Once matching has been performed, there are several classes of energy-flow objects. Those with islands but no track are assumed to be neutrals and the island energy is used in the jet. Those with a track and no island are assumed to be particles stopped in the dead material in front of the calorimeter (or muons depositing very little energy in the calorimeter) and the track angle and momentum are used in the jet. For those objects with one (or more) tracks matched to an island, the estimated uncertainties on the island energy and the track momentum are compared, and the more accurate of the two measures is used in the jet. This algorithm is used [7] with an additional correction based on presampler information for particles showering early in the dead material. This probably represents the best obtainable performance for the ZEUS jet measurement.

It is interesting to note that CDF have compared [2] an energy flow-type algorithm with their standard calorimetric energy reconstruction for jets. The value in the table corresponds to their standard method. Using clean events with a well-reconstructed, high p_T photon opposite a single jet, they have obtained a significant improvement in the jet resolution and get $64\%/\sqrt{E}$. However, it seems it is not straightforward to apply this method generally, due to other jet activity in typical events, and this method has not been applied in any physics analysis as far as we are aware.

The table indicates ATLAS should have the best jet resolution of the other calorimeters. Even here, the ATLAS TDR [10] gives a $W \to jj$ mass resolution as ~ 8 GeV at low E_T, which is substantially worse than the Z mass resolution of ~ 3 GeV for stationary Zs shown in Fig. 1 of our proposal. This precision is required for clean W and Z separation where the target is a resolution at the level of the W and Z widths.

It should also be remembered that electromagnetic resolution is important for CALICE too. One important physics process is $H \to \gamma\gamma$. For light Higgs, this may be the main channel observed at the LHC, so it would be vital to also measure this branching fraction at the LC to normalise the results. In addition, for $H \to b\bar{b}$, lepton tagging is very powerful and for the calorimeter means identifying electrons within hadronic jets. This demands laterally narrow showers with well-measured energies for efficient track-cluster matching. We note the ZEUS calorimeter has substantially worse resolution for electrons and photons than the TESLA TDR; electron tagging within jets is not a major physics issue at HERA.

It should be noted that hadron calorimeters are intrinsically less precise than electromagnetic calorimeters due to the many different possible hadronic processes which have different thresholds and also because the energy is deposited in larger “lumps”, so it is more granular and hence subject to larger statistical fluctuations. The ideal jet measurement measures effectively all charged particles ($\sim 60\%$ of the energy) in the tracking detectors, the photons and high energy ($>\sim 50$ GeV) electrons ($\sim 30\%$ of the energy) in the ECAL and then only the neutral hadrons (n and K_L^0, $\sim 10\%$ of the energy) in the HCAL, so as to minimise the intrinsically less accurate contribution of the latter to the total. This is the basis for energy flow algorithms and a finely segmented calorimeter with a small lateral shower spread, both for the ECAL and HCAL, which can separate out these three categories of particles is more important than intrinsic calorimeter energy resolution. This means that, although the resolution of the ECAL is not a limiting factor in the jet resolution, the ECAL has a dramatic effect on the pattern recognition for energy flow. It is not true to say the ECAL is irrelevant for jet resolution.

3. *Regarding the overall simulation results, what is gained by having* 30×10^6 *EM cells rather than 15 or* 7×10^6*, e.g. by decreasing the sampling frequency which would presumably affect only the EM resolution and/or e.g. by ganging together pads on opposite sides of an absorber layer?*

   These questions are obviously extremely relevant and they form exactly the type of result which will come out of this proposal. The most simple approaches, such as removing every other layer of silicon readout to give 20 layers, have already been studied. This shows a minor degradation for jets from $33\%/\sqrt{E}$ to $34\%/\sqrt{E}$ and a more significant change for the single photon resolution from $13\%/\sqrt{E}$ to $18\%/\sqrt{E}$. However, it is not obvious that keeping the same tungsten layer thicknesses under these conditions is optimal. It might be that significant gains can be made by varying the thickness differently for 20 layers compared to 40.

   As for ganging; this effects only the number of channels, not the area of silicon wafers needed. This means it would have little effect on the cost, as the readout electronics are only 8% of the total. It might simplify some of the operational issues of calibration, etc., of course. To reduce the channel count, a different approach might be to increase the pad size as a function of depth so as to match the total shower size. This might also match the tile HCAL granularity at the back of the ECAL.

   Again, the effect of all these ideas on the overall physics performance need to be studied and that is essentially the heart of our proposal.

4. *The CALICE calorimeter approach is a global one with little or no emphasis on EM calorimetry with respect to overall calorimetry. At this stage, a lot of effort has already gone into the EM calo conceptual design and simulations and there is only one option actively under study. In contrast, the hadronic calorimetry consists of two competing options, one with digital readout of small gas cells, the other with analogue readout of larger-size tiles. Why have the UK groups chosen to join the EM calo effort rather than the perhaps less populated hadronic calo effort?*

   The main reason for choosing the ECAL was that this is where our interests lay. This is at least in part because there is significant relevant experience in EM calorimeters, electronics, silicon detectors and software amongst the people involved in this proposal. P.D.Dauncey has worked on the readout electronics and trigger for the BaBar EM calorimeter as well as the silicon vertex detectors for Mark-II/SLC and Delphi. R.J.Barlow has also worked on the BaBar EM calorimeter, including leading the test beam for this detector. I.P.Duerdoth and R.J.Thompson are working on the ATLAS silicon wafer development, particularly on the practical implementation of the whole system. D.R.Ward has worked on the OPAL EM calorimeter and lead the OPAL GEANT3-based simulation for many years. J.M.Butterworth, together with members of the UCL electronics group, designed the ZEUS MVD clock and control electronics and the ATLAS SCT timing interface module. D.R.Ward, N.K.Watson and M.A.Thomson have significant experience of energy flow algorithms from OPAL W physics analyses and J.M.Butterworth has lead several analyses on ZEUS which depend heavily on jet reconstruction. In contrast, there is very little HCAL expertise within the UK groups with no experience of scintillating tiles, although the Manchester group do have significant expertise in RPCs for the digital HCAL option.

   We would regard the global approach of CALICE as a major benefit, not a drawback. This proposal commits us to the ECAL for the next few years only. At present, this is where our interests lie, but it is hard to predict the future. If after this time, we did indeed decide that working on the HCAL would be a better option, this would be a lot easier from within CALICE than outside it. Indeed, the readout electronics would probably be the most closely related area. Our proposal actually includes tile HCAL electronics readout and the data acquisition of course covers both systems too.

   We do not agree with the implication of this question that the Si-W ECAL issues are all solved. Both questions 3 and 5 raise several issues for which more work is needed. There are many other items, some listed in the proposal and open presentation, which will take significant effort to answer. Solutions to these will need to be balanced against cost constraints which will tend to reduce the performance. As an example, one very major issue is the location of the front-end electronics. The TESLA TDR assumes no amplification will be needed at the silicon diodes and so cables up to 1.6m long carry unamplified signals to electronics at the corners of the barrel structure. These signals need to have a dynamic range of 15 bits and a resolution of 10 bits. While the capacitance of a single cable is sufficiently low that this does not induce too much extra noise, the cable density at the end of the structure is very high and system issues such as cross-talk could cause significant problems. An alternative would be to include preamplifiers on top of the diodes. UK studies have shown that the passive heat flow out from these would be marginal and that cooling pipes are likely to be needed between the tungsten sheets. These will increase the gap depth and the impact of this on the energy resolution needs careful study. This must be balanced against an overall increase in the radial thickness of the ECAL from increasing the gaps as this significantly increases the radius of the magnet coil and hence its cost.

   We therefore believe there is still a large amount of work to be done to optimise and verify the concept of a Si-W ECAL. In addition, in the area of the ECAL readout, which is potentially a strong area of interest of the UK groups longer term, the TESLA TDR had very little detail; this is still a wide open field.

While the Si-W ECAL is the only option currently being studied within CALICE, there are other options being considered outside this collaboration, even within Europe (see question 6). However, within the groups which arose from the TESLA TDR, the Si-W ECAL is now clearly the baseline design. This is because it is viewed as being clearly superior for the physics and will be the design used if it can be brought within a realistic cost. We would turn the question around and say this could be a strength of our proposal; we have tried to "back a winner" and not be left in a few years with a rejected technology.

As stated in the proposal, the members of CALICE are roughly divided equally between the three parts of the program, namely the ECAL, tile HCAL and digital HCAL, so that the HCAL is not obviously underpopulated. Almost all the effort on the ECAL is from French groups, while the HCAL studies have significant Russian and American membership; it is clearly easier (and cheaper) to collaborate with geographically close-by groups. Also, the ECAL people involved were known to us from CERN and SLAC and we had confidence that they would be capable of delivering on this project.

5. *The Panel felt it was essential that the UK should play a leading role in the simulation effort. What is the current status of the simulation effort and what are the UK plans to contribute to this in a visible way over the next months? The TDR simulation had many idealised features: it was not based on a full reconstruction of fully simulated GEANT events, but rather on an idealised reconstruction, only the barrel calorimetry had been simulated and one expects jets to become more collimated in the end-caps, the two hadronic calorimeter solutions were using totally different tools, the choice of G4 at the present moment for performing these hadronic calorimeter simulations is extremely questionable, etc.*

   This question overlaps closely with question 11, so we answer both here.

   It should be remembered that we are *not* proposing to build a detector; we are proposing a study to design and optimise a detector. By definition, if all the answers were already known, then this proposal would be redundant. The points on the lack of realism in the TDR simulation are all valid and again reinforce our statement that there is a lot of work still to be done on the ECAL before we could propose a realistic design. The lack of detail used for these studies reflected the short timescale and low level of effort available at the time of the TDR.

   The UK simulation effect is mainly concentrated in Cambridge (D.R.Ward) and Birmingham (N.K.Watson) at present. These two people will lead the UK effort; D.R.Ward has great deal of experience of the OPAL simulation, in particular the OPAL calorimeter. Cambridge have a 0.5 FTE PPARC post-doc Fellow (C.Ainsley) for this work. Birmingham have requested a fraction of a new RA post in the rolling grant which is covered by the contingency request in the proposal. Manchester will also contribute here through R.J.Barlow and have a rolling grant RA (N.M.Malden at 0.1 FTE).

   Given the relatively small number of people involved in the simulation at present, coordination is not considered a problem. D.R.Ward and N.K.Watson have worked together in OPAL for more than ten years in various major software projects from offline event display to public W^+W^- analysis software.

   The current status of the simulation in the UK is that both the Cambridge and Birmingham groups already have the GEANT4-based detector simulation of the CALICE detector and test beam prototype installed and running. D.R.Ward presented first studies at a meeting with non-UK CALICE colleagues meeting in February [14] to assist design decisions for the readout and data acquisition.

The UK groups have discussed with other software developers within the CALICE collaboration and an initial (not exhaustive) list of activities to be started during the coming months includes:

- Comparison of physics content of GEANT4, relative to GEANT3 and other packages. This is an area where it is clear more work is required [15]. It should be noted that the GEANT4-based application developed has the ability to write out a GEANT3 compatible geometry description, which will assist us in these comparisons.
- Investigate the stability and robustness of earlier conclusions about the benefit of energy flow measurements to changes in the choice of, e.g., hadronic interaction models, or of a particular set of model parameters. This is full detector study and is not restricted to the prototype. It is expected that this would lead to improvements in the algorithms currently being developed.
- Use the above to identify particular areas of the modelling which are less well defined, and hence critical issues to be investigated using test beam data when they become available.
- Use of the simulation to understand how the test beam prototype can be used most effectively to test the energy flow concept, given that there is no tracking chamber or magnetic field in the test beam.
- Use of the model of the whole detector to construct a compilation of physics-related results which will be useful for the electronics design team. These include the rate per beam crossing in the end-cap, the occupancies in the barrel and end-cap, the rates per train and per wafer for data volume calculations and the impact from very low energy pairs due to machine background. These are all related to the full detector design and are not restricted to prototype.

There are clearly distinct groupings of tasks in the above list, e.g., some relate to whole detector studies and physics processes, some are more oriented towards the future test beam data, some relate more specifically to comparative studies of GEANT3 and 4. The number of such groups is comparable to the number of active people, and tasks could be divided accordingly. There is a shared interest with MINOS at Cambridge in the GEANT4 studies so they will most likely take on the first three tasks above. Birmingham will then concentrate on the last two.

Concerning the jets being more collimated in the end-caps; most of the physics processes of interest are s-channel and hence use all the available energy and have effectively no boost relative to the laboratory frame. These give approximately isotropic distributions for jets in the detector and no correlation between the angle of the jets and their energies. For these, the end-caps will see jets of the same energies as the barrel. However, there are some important t-channel processes, such as $\nu\bar{\nu}W^+W^-$, which do produce forward-peaked jets. However, even in these cases, the average energy of the jets in the end-caps is only around a factor of three or four higher than those in the barrel, as shown in Fig. 1, so the effect is not severe.

As for GEANT4; it is already widely used and its use is expanding, at least to some extent for technical software (C++) reasons. Pragmatically, it would be difficult to start using only GEANT3 at this point as so much effort is going into the newer code; the only viable approach for the future seems to be GEANT4 but it clearly must not be

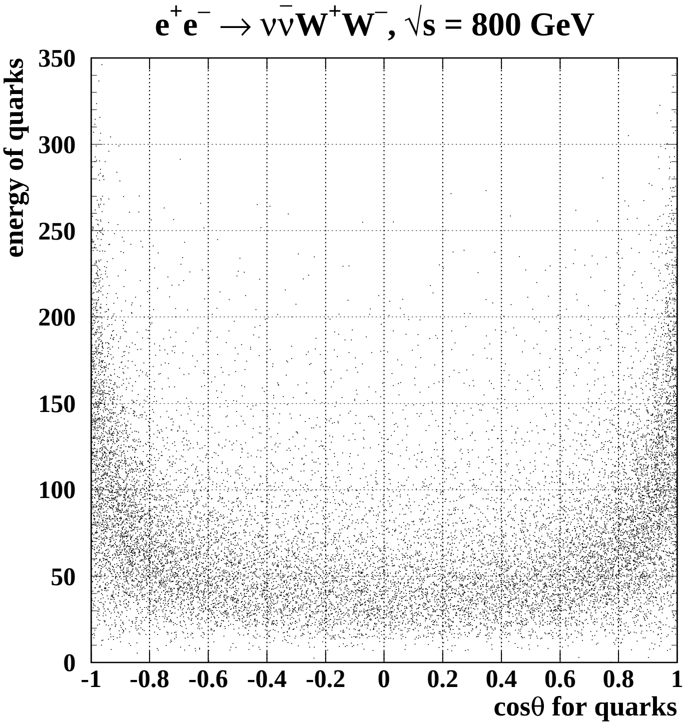

Figure 1: Energy of quarks produced from W decays through $e^+e^- \rightarrow \nu\bar{\nu}W^+W^-$, as a function of $\cos\theta$, where θ is the polar angle.

used uncritically. It will require tuning, particularly for hadronic interactions, and we will be able to contribute to this effort. Manchester have both CALICE and GEANT4 collaborators in the HEP group, so we will be able to feed back our studies into GEANT4 and vice versa.

2 Hardware

6. *Are there competing technologies to Si-W, for example compensating calorimeters? What is the scope of other LC calorimeter studies throughout the world ? Where would the UK stand if Si-W was not the final chosen option?*

   A recent summary [16] of linear collider R&D worldwide has been produced and many details can be found there. In addition, the recent CALOR02 conference [17] has presentations from various groups. All current studies known to us are looking at sampling ECALs with either tungsten or lead as the converter. The main differences are in the materials used in the detecting layers and whether the main drive is for high granularity or compensation. All groups involved are also doing simulation studies of energy flow algorithms.

   Within Europe, besides CALICE, there is an Italian group of around ten people working on a tile scintillator ECAL with either lead or tungsten plates [18]. The tiles would be around 5×5 cm^2 and be read out via wavelength shifting fibres. However, because the granularity of such a device has been found to be inadequate for energy flow jet reconstruction, they propose to add three "shower max" layers of silicon diode pads, with a pad size of 1×1 cm^2, at depths of 2, 6 and 12 radiation lengths to help in the pattern recognition. The silicon diodes are therefore very similar to those used in the CALICE ECAL design and the scintillating tiles are like those of the CALICE HCAL. (For historical reasons, the Italian groups have not joined CALICE, but there is communication between the two efforts.) This collaboration was responsible for the Shashlik design described in the TESLA TDR [1], although this option has now been dropped.

Within the US, there are ECAL groups at Oregon and SLAC working on a Si-W ECAL [19] with similar aims to CALICE, although their schedule is somewhat behind that of CALICE. There are good communications between the groups and a member of that collaboration, R.Frey, has "observer" status on the CALICE Steering Board.

In Japan, there are studies for the JLC of lead-scintillator calorimeters, again with one or more "shower max" silicon diode layers included [20]. There has been some work here on tuning the thickness of the lead plates to optimise the resolution and/or achieve "hardware compensation" by balancing the e/π response [21]. Hence, the aim here is for a combined ECAL and HCAL and so is a somewhat different approach to the European and US efforts. These groups have already done some beam tests and have further runs planned for this and next year.

Although mainly European, CALICE is the only calorimeter collaboration which has participants from all three geographical regions (European, US and Asian). It is also collider independent to a large extent. Hence, whichever collider is chosen and wherever it is built, it is likely members of CALICE will be involved in the detector collaboration. If Si-W is not the chosen solution for the ECAL, then we would need to change our contribution and in all likelihood would join in the effort in the alternative technology. However, we would then be in the position of having contributed to the technology choice already, being known (and, we hope, being well regarded) within the linear collider calorimeter community and having gained significant experience of the issues involved. Such a change would not by any means be unprecedented. However, as stated in question 4, we believe Si-W is the best guess for the most appropriate technology at this time so we hope we have minimised the chance of this happening.

7. *Is there an issue of radiation damage to the silicon? Will the possibility of running cheaper lower-grade silicon at cryogenic temperatures be considered?*

   Radiation damage is thought not to be an issue, but accurate estimates have not been done for the whole detector. The levels of radiation also differ somewhat between the different linear collider machines. Some calculations exist for the vertex detector region, as that detector is thought to be the most sensitive; the values for the ECAL should be significantly smaller (and certainly will not be larger). At the inner layer of the vertex detector, radius 1.5 cm, the flux for neutrons is estimated to be around 10^9 cm^{-2} per year, and is certainly less than 10^{11} cm^{-2} per year, which is well below the level at the LHC. Electromagnetic effects are expected to be highest at the innermost radii of the end-caps due to machine background and low-angle Bhabha scattering. The vertex detector inner layer expects around 1000 $e^{\pm}$ tracks cm^{-2} per second, or 10^{10} cm^{-2} per year, which is four orders of magnitude lower than the known limits of silicon detectors, thanks to the developments from the LHC silicon groups.

   Given this, then the argument for using cryogenic detectors would be based on cost, not performance. As stated in the PPRP open session, we were not aware of this possibility as it had not been presented at any meeting when a UK member was present. However, it *has* been discussed within the CALICE collaboration and was not thought particularly advantageous. The cost of the raw materials for a processed silicon wafer are around 10% of the total, so that even if the cost of the lower-grade silicon was negligible, a maximum saving of around 9 Meuros would be possible. However, this would be offset by the cost of the cryogenic structure and significantly more complicated mechanics and electronics connections. It is likely the structure would also require more space radially, increasing the HCAL and hence coil size (a major cost driver). An accurate costing has not been done but it seems likely that actual savings would be quite small, if any. In addition, the performance may be degraded because of larger dead regions due to the cryostat and operationally, the detector would require higher bias voltages and be much harder to access and maintain.

Of course, there is nothing to prevent this option being raised again in the future if some of these issues are known to have changed.

8. *What is the status of elementary R&D on one layer of Si-W: connections, FE electronics, mechanics, signal/noise, etc? There was little experience with Si calorimetry in CALICE* ~ 1 *year ago: has this changed?*

   As stated in the open presentation, the CALICE ECAL work is divided into two parts; a "physics prototype" (the beam test detector) and a "technical prototype". The latter is a single mechanical layer of the TDR structure with a few wafers. Despite interest in several of these issues, the UK does not propose to participate in this latter effort, to some extent because this seems premature given the uncertainties of the TESLA TDR design. It would also be difficult due the funding situation in the UK at present. Progress in this effort can be seen in the presentations at the last EFCA/DESY Workshop [22].

9. *Regarding the prototype electronics design: will the on-board FPGAs provide intelligent processing of the data, or are they just passive?*

   The proposal foresees only passive data processing in the readout board FPGA. This is a place where we chose simplicity (less FPGA design) and robustness (no problems with incorrect thresholds, etc.) to be more confident of completing on schedule. The benefits of keeping data from all channels are that pedestal and noise studies are much simpler and thresholds can be readjusted at any time offline. The downside is the much larger data volume (by an order of magnitude), which leads to more disk space and slower data acquisition and analysis. The total disk volume requested is costed at £8k so that major savings would not be possible here, particularly when compared with the engineering effort to implement the extra FPGA firmware design for intelligent processing. The bandwidth available for data acquisition with all data read out is easily sufficient (100 Hz needed, up to 1 kHz available), so there would be little gain here. Finally, we foresee the first stage of data analysis as being data reduction to a smaller dataset, removing any speed implications for subsequent analysis.

3 Costs and Manpower

10. *How will the UK provide the strong academic leadership necessary to provide effective coordination of a relatively large number of people spending small fractions of their time on the project? This is a major concern of the Panel.*

    P.D.Dauncey will act as spokesperson for the UK collaboration and will coordinate the effort overall. He will also lead the electronics part of the project, as outlined in question 12. Following the previous PPRP meeting, he has negotiated with the IC group to increase the fraction of his time on this project to 0.3 FTE, with the rest of his time being 0.4 FTE on BaBar and 0.3 FTE on teaching. The Grants Committee has been informed of this change. In addition, he will apply for a PPARC Fellowship at the next round which, if successful, would remove his teaching commitment and allow him to spend 0.5 FTE on each of BaBar and CALICE.

Other potential increases in effort may come from R.J.Barlow, who is also applying for a PPARC Fellowship, and D.R.Ward, who may obtain a sabbatical for the next academic year. In each case, if successful, this would allow them to increase the fraction of their time on CALICE substantially. As detailed in questions 5 and 11, D.R.Ward will be one of the people leading the simulation effort (which is where R.J.Barlow will also contribute) and this would be a major boost to this part of the project. Finally, Manchester will be hiring a new lecturer and CALICE is one of the areas which is being pushed for this post.

Within the effort listed in the proposal, there are some other points to note. J.M.Butterworth is nominally down for 0.1 FTE for CALICE although he is spending another 0.1 FTE on supervision and management of the UCL HEP Electronics Group. As UCL will mainly contribute to the electronics (see question 12), then his contribution is likely to be significantly more than the 0.1 FTE might imply. N.K.Watson is only listed for the first two years, as this is the duration of his current post, although he would certainly intend to remain on CALICE in whichever position he takes after this time. He will be the other leader of the simulation effort and again, this would give more continuity than the effort listed would indicate.

Note that, although the physics potential of a linear collider is very important, the current proposal is for a relatively small-scale project with no direct physics output within its three-year period. It would be unrealistic to expect a large number of people to put a major fraction of their research time into this project at such an early stage.

11. *How is the simulation work shared throughout the UK groups ? Who does what? Who is leading the simulation effort, and how is it being co-ordinated across groups? What is the UK role in simulation within the wider collaboration - do the UK have an established role? Who in the UK will set up the simulation work based on GEANT4? Will GEANT3 and FLUKA also be considered?*

    See answer to question 5.

12. *In more detail, how is the electronics work broken down amongst institutes, including TD. Who does what? Justify why extra TD effort is necessary. Who is leading the electronics effort, and how is it being co-ordinated across groups?*

    We propose that the electronics work will be divided between IC, UCL, Manchester and RAL TD. The exact allocation of effort depends on several factors which are not known yet; namely the exact specification of the trigger board and the level of engineering effort and experience of any RAL TD people. The following is how we would like to see the project work.

    IC and Manchester will do the readout board, which is estimated to be 18 months effort in total. Manchester (D.Mercer, with some help from S.Kolya) will do the master FPGA firmware design, which includes the board control and VME interface. IC (D.R.Price and O.Zorba) will do the slave FPGA design, which does the cable signal control and ADC readout. They will also take on the overall board design. However, the total effort of 12 months available from these three engineers, i.e., 0.5 FTE for the two years, is less than the estimate needed. This shortfall of 6 months is part of the requested RAL TD effort. In addition, none of the University groups involved have experience of board layout for multi-layer boards, as will be required here. IC would like to gain this experience but the short timescale for this project probably means it would not be feasible to use this project as a test-bed. Hence we assume the board layout (and subsequent fabrication) should be done through RAL TD. We estimate 1 month effort for this for each of the prototype and production versions of the readout board.

UCL (M.Postranecky and M.Warren) will do the trigger and test boards, estimated to need 6 months each. These two boards are required a little later than the readout board and the test board will lean heavily on the readout board design. This matches well with other UCL commitments during 2002. Again, the total effort of 6 months available is less than the estimated requirement and we propose to make up the shortfall of 6 months with RAL TD effort. As before, layout and fabrication would also be done through RAL TD, taking around 1 month for each of the two boards.

The absence of a significant amount of the requested RAL TD effort would require us to scale back on the scope of the project. The test board might need to be completely scrapped, which then raises the issue of how to test the readout boards. The trigger board might be simplified but only by forcing much of its functionality out into (non-UK) parts of the project which are not expecting (and have not budgetted for) this extra complication. In this scenario, UCL would work on the readout board with IC and Manchester, making coordination somewhat more complicated.

With zero TD allocation, the layout would become the critical issue. This would either kill the project or incur significant delays while IC came up to speed.

P.D.Dauncey will lead the electronics effort although J.M.Butterworth will oversee the trigger and test board part of the project. Academic input at Manchester will come from I.P.Duerdoth. Note, if we were awarded effort from RAL TD and an experienced system-level engineer was available, then they would play a major role in coordinating the engineering development of all parts of the project. However, if the effort is at a lower level, then they would work under the supervision of the University staff.

The coordination of the three UK groups and RAL TD engineers would be done through regular meetings, including some phone conferences. This has worked well so far; UCL has proved to be a convenient location and RAL is likely to become another. Note, the two parts of the electronics project (i.e. the readout board and the trigger and test boards) will have well-specified interfaces so that parallel development should be straightforward.

13. *The travel costs are felt to be high, in particular what are the "beam-time expenses"?*

    We proposed travel funds at the level that we think would allow us to be most effective. Obviously travel is a soft target and we would be able to survive with less funds, although we would be less able to contribute efficiently and there is a higher risk of incurring delays through reduced collaboration with the other members of CALICE.

    There is clearly some uncertainty in the travel estimate as we have not yet had enough experience within CALICE to know accurately the level required. We do know that we have been severely limited in our ability to contribute so far because of lack of travel funds. We have used £2k for the first three months of this calendar year, with N.K.Watson also using funds from his PPARC Fellowship and Birmingham using a pilot grant for travel in addition. This has restricted us to one attendee from the UK for some CALICE meetings. To contribute effectively in the future, we will need to increase this level of travel substantially. We have been trying to solve complicated issues, such as problems in the simulation program and interface specifications for the electronics, by email rather than face-to-face.

    The estimate of FY02/03 £42k per year is based on the following. UK travel is costed at £10k per year. Of this, electronics coordination meetings each cost around £200, and fortnightly meetings would therefore total around £5k. The simulation effort would take a smaller amount, around £3k, but of a similar magnitude. The rest would be for general UK linear collider meetings, which can involve a large fraction of the CALICE members attending. Overseas travel is mainly to CALICE collaboration meetings. There are ECAL, HCAL and combined meetings and, because of the general DAQ responsibilities, people from the UK would need to attend all of these. It is likely there will be one of these approximately every two months and in future, it seems they will tend to be spread over more than one day. With five or six attendees for the ECAL and general meetings, and one or two for the HCAL meetings, then this would be around 24 trips per year, each costed at around £1k, totalling £24k. Finally, CALICE meetings will be held during the ECFA/DESY and International Linear Collider meetings. There will be two or three of these per year and will be quite expensive, around £2k, as they last a week and are often outside Europe. We assume around two people from the UK will attend each of these, totalling around £8k per year.

The proposal includes "up to £10k" for beam line set-up. This is effectively what the UK operations common fund contribution would be in the worst case. This is based on the option of using the IHEP/Protvino beam lines. Configuring the beam lines for CALICE would cost the collaboration approximately 60 keuros. There would be some further minor costs associated with operations. If the UK joins CALICE, then we would be around 16% of the collaboration and so would be liable for at least 10 keuros. The £10k covered these costs. Note if DESY or Fermilab were used for the beam tests, such costs would be substantially reduced, which is why this is flagged as "up to".

14. *The Panel was not convinced about the award of RAs (1.6 FTE's), and was confused about how the RA's were to be shared. How would the RAs be distributed across institutes, and why are they essential to the work? A workplan with deliverables would need to be provided. Are there RAs in post already, and are being applied for as assurance that they might be lost in the next RG round? Or are they really new posts?*

    Given the importance of the simulation studies, and later the beam test data analysis, each UK group wants to be able to contribute in at least one of these areas and the proposed RAs are mainly to do this work. As stated above, Birmingham, Cambridge and Manchester will do simulation studies in the short term. Of these, Birmingham is lacking an RA and so is bidding for a completely new post which will be split 0.2 FTE on CALICE and 0.8 FTE on BaBar. It is hoped this post would start as soon as practicable, i.e. from around October 2002. (This is why the bid has only 0.1 FTE in FY02/03, but 0.2 FTE in the later years.)

    IC and UCL will not have sufficient effort available to do both simulation studies and the electronics project in the short term. The only RA in these groups is D.Bowerman at 0.2 FTE from October 2002 and he will be providing day-to-day effort for electronics testing and software. Hence both these groups have bid for completely new rolling grant RA's to start in the medium term, aiming for data analysis. The IC bid is for a new post starting in FY03/04, with 0.3 FTE on CALICE and the remaining 0.7 FTE on LHCb. The UCL bid is for a new post starting in FY04/05, a full-time linear collider RA split equally between CALICE and luminosity/beam monitor studies.

    The Birmingham RA would work on the last two points of the list in question 5. The longer term needs for data analysis, which is where the IC and UCL RA's would participate, are more difficult to define and depend on what are discovered to be the critical issues for validating the simulation, as stated in point three on the list. These will only be know after some of the simulation work is completed.

    If these bids were unsuccessful in the rolling grant and were then covered by PPRP funding, then a different arrangement would be needed, as the other parts of these RAs would still be unfunded. Specifically, existing posts would be used to provide the fractions of CALICE effort. The PPRP contingency funding requested in the proposal would be used to extend the duration of these positions and/or allow overlap when replacing the people in the posts, so as to compensate for the loss of effort in their present experiments.

If these posts are not awarded either through the rolling grant or the PPRP, then the impact will be on the simulation studies and exploitation of the beam test data, not on the electronics. This would reduce the impact which we could have in these areas. We would still be able to fulfill our commitments to the hardware side of the project.

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