Thank you for the fast reading and detailed comments. They have been implemented as suggested in the note.

Comments to CAN-061 version from May 9 by Matthew Wing

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Questions on analysis and plots
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- Figure 2 and the discussion. The differences in the hit energy spectra are not large, but what effect do they have ? How does this feed into the systematics ? Maybe I missed it and it comes later, but then a forward reference would help.
Agreed the difference of the energy spectra is minimal.
A significant difference in the width of the distribution would I think mostly impact the number of hits and to a certain level the calo response and resolution. However, for this analysis, only the number of hits has a main impact which is minimal as the shape of the spectra are very similar.
A difference in the position of the peak of the spectra would have an impact on the energy scale of the detector. The precision at which the energy scale is determined as been assessed (3.6\% syst.) and it is included in the systematics when looking at the mean time versus the hit energy. I added a forward reference.

- Figure 3. I guess that we are not requesting to release this plot ? The disagreement around 20 MIPs should also be clear on the ratio, i.e. we should still be able to see the points. If it will not be shown in public, the it is not a high priority, but it would still be good to update.
This plot has been removed. I added a sentence to explain that the hit energy spectrum has been checked for electrons and that the data and simulation agree well in the range of interest.

- Figure 6, caption. I make s = 3920/(3613-88) = 1.44 ns/TDC, but you have 1.47 ns/TDC ?
Corrected.

- Figure 7. I would not show the mean and RMS for the results before correction. It is obvious with two peaks like this that the RMS will be very large. My initial thought was that the numbers applied to one of the Gaussians and not both together. So I think having the numbers is confusing with no real gain as it is so obvious, visually, that the correction has worked. Maybe I missed it, but why are there effectively two distributions before correction ?
Removed the mean and RMS before correction.
There are two distributions before the correction due to the fact that pedestals are dependent on the bunch-crossing parity which is not taken into account in the pedestal extraction. This was done in order to have enough statistics to determine the pedestal for each channel. However, this does not affect the final results as the offset in pedestals is corrected in a later stage. I reformulated in the text how the time of a hit is calculated and added a small paragraph on the pedestal extraction for each channel.

- Figure 9. The fit is okay, but a chi^2/ndf of 74/56 is not great. So do you know

what is missing ? Same for figure 10 where the fit is chi^2/ndf=1.86.
I believe that is because of the Chi2 method of calculation. If I am correct, generally in the Chi2 calculation there is the assumption that the errors are Gaussian. However for this study, the underlying distribution is not Gaussian. Also it may comes from the fact that we don't really know the exact fit function, it is just an assumption as well that this assumption is true for all chips. Same for the next figure, we assume that the fit function works for all chips but we know that different thresholds are used depending on the chip that might have a small effect there.

- Figure 11. What do the horizontal error bars represent ? Are they real errors, and if so what, or are they just some plotting feature ? Are they included in the fit ?
The horizontal errors are just the bin width by default in the Tprofile, it is just a feature of the plot. I removed them. Same for the figure 12.

- Figure 13. In the ratio, are the error bars on the MC due to statistics of the sample or the cross talk ? It mentions cross talk in the caption, but I wanted to check whether there is a statistical effect too.
In the legend, the data has "stat+syst" but in the caption it says just "statistical"; correct one or the other.
The errors bar in the MC are statistical and also from the cross-talk. However, the uncertainty from the cross-talk is very minimal. The error bars in the tails of the MC are mostly from statistical uncertainty.
Corrected the plot.

- l.377-81. Attributing a factor of 2 to the GEANT version seems like a poor advert for GEANT; is this really the case ?
Is a factor of 5 okay; don't we need a comment.
After discussions and cross-checks, QGSP_BERT_HP and QBBC are in relative good agreement with the data.

- Figure 14a. I assume the red points are under the blue points so essentially invisible ? Looking at the error bars, I assume these are correlated which means that all points all move up or down together ?
Removed Mokka from the note.
Yes indeed the error are correlated.

- Figure 16. Is the uncertainty shown correlated between points or is a shape change, within that band, allowed ? Same for Figure 17. This then relates to the discussion and the goodness of the description.
The systematic uncertainty was recalculated as it was under-estimated due to correlations which increased the error band. Now all physics list agree with data within the uncertainties.

- Figure 17. I don't think I have fully understood the procedure as how you get negative values for the mean time ? Is there a subtraction that happens ?
The time is defined as t_i - T_ref for the i-th channel. For each bin of radius/energy, a distribution of t_i - T_ref for all channels is done. The mean of this time distribution is then taken in the range [-50, 200] ns. This mean will be dependent of the shape of the distribution especially the tail on the left between -50 and 0 ns.

- l.435-9. That is a bit of an unsatisfactory end ? How will this difference affects the results ?
This is indeed a bit unfortunate. A better equipped calorimeter would give a higher power to select pion properly and reject multi-particle events.
This would enable to perform a better comparison between data and MC and a conclusion could be drawn from this study.
I decided to remove the correlation from the note because the results are inconclusive.

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Corrections to note/plots
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- l.49-51. This sentence does not make sense and I am not sure what to take from it. Maybe make it two sentences and it will be clearer.
Corrected. Two sentence are made. The point to take away from this is that the performance of this partially equipped prototype is not the performance expected of the AHCAL engineering prototype.

- l.58-9. Be more precise in the description. Which layers are ECAL, which layers are HCAL. In principle, someone should be able to read this and programme the exact configuration; at the moment I don't think you can.
Implemented in the note.

- p.8, Eq.5.1 and discussion. Normally, I think that a slop is defined as (y2-y1)/(x2-x1) but you have (x2-x1)/(y2-y1). I don't think that this affects any of the analysis, and so I think you should just avoid the word "slope" as it did confuse me.
Has been implemented. The slope is defined as (y2-y1)/(x2-x1) and the inverse of the slope is used for converting the TDC to ns.

- l.228-9. I am not sure what this sentence means, particularly the word "rapport".
Corrected.

- Figure 8. The stat. box says -165.3, but the caption -165.2 ns. It's only a small difference, but should be the same.
Corrected.

- Figure 14b. The legend overlaps with some of the points and so needs to be moved.
In the ratio, we need to see the points around 10-13 triggered channels.
Improved the visibility of the plot.

- l.394. I think this should be "In this range, and at higher MIP values, all …"
Corrected.

- Figure 19. The caption should be clear that (a) is data. Also state what the red box is.
Removed according to Roman's comments.

- l.44-5. I am intrigued by the statement that you can remove gamma-gamma events

by timing cuts. Is there a reference for this or is there more explanation somewhere ? This its general question probably not necessarily relevant to the release of the work as “Work in progress”.
The sentence has been reformulated and a reference is given.