BT_notes_4july07

Meeting 48: July 4, 2007

Participants: Luca Latronico (LL), Philippe Bruel (PB), Berrie Giebels (BG), Leon S Rochester (LSR), Benoit Lott (BL), David Paneque (DP), Johan Bregeon (JB), Nicola Mazziotta (MNM), Claudia Monte, Tyrel Jonhson (TJ), Tomi Ylinen, Taka Tanaka, Jan Conrad, Monica Brigida

News

JB: working on synch BT with GR, currently experiencing some sug fault to fix with Michael, also working with Monica in Bari to check Bari digit implementation

CAL energy checks

PB: 1st plot is the one sent beforethrough the BT mailing list. The idea is to check the stability of CAL response with calibration runs. Looked at raw total energy and energy in layers, using as a reference 2024 (on-axis 100GeV e), and looking at energy and dividing by reference energy.
There are 4 scans, red arrows are twr2 scans, blue are twr3 scans, first 2 are Y scans, second 2 are X scan.
Applied my std cuts, in particular cuts that remove electrons too close to xtal boundaries. Cut is 0.4 for first plots, i.e. I keep only e that are within +-40% of 1 log. Really surprised by results, in particular in layer0 and 1, where a big difference can be seen for X and Y scans. First and last points of the scan are expected to be lower since you are actually shooting close to the xtal boundary, the rest of the scan should be stable. Johan told me we might see a problem with the beam position here. I tried making a harder cut, and there is an effect if you look at the other plots you see that. I am quite surprised by the sensitivity to this interlog cut, maybe not so obvious to conclude now.
The last plot with 0.25 cut still has instabilities, e.g. layer6. I need to check that the interlog cut is enough.

BL: is this the mean energy? the change is about 15% between the plots with 0.4 cut and 0.25 cut, which would mean that the energy is lost in cracks or something, surprising

PB: at high energies, when you are close to the interlog boundary, there is a strong effect, I am sending some slides

JB: in the first layers the shower has not developed yet, so we are bound to see a larger effect

PB: looking at the slides I just check sent, slide2, you see the strong effect, interlog cut at 0.4 means between 0.1 and 0.9 on slide2, cut at 0.3 means between 0.2 and 0.8...

LSR: *lost the comment*

PB: what is odd is when you look at the plots with 0.4 cut, it is higher for layer0 for this scan

Most probable energy - Nicola

MNM: today I would like to show some plots on the most probable energy. I already showed the CAL profile fitting on the 10 jan VRVS.
This shows the ratio of the various energy variables wrt to beam energy, it shows a 10% excess in the SPS runs, same for all incoming angles, could you comment on that?

PB: this is due to the energy raw excess; since all algorithms are based on the CalEnergyRaw, an excess there reflects into an excess in the corrected variables. I warn people that the only algorithm that is always defined in the whole energy range is the one giving EvtEnergyCorr, the other variables are not always defined in the full energy range

MNM: does this not mean that we have an overestimation of the data energy? what does it have to do with MC?
PB: yes, but it is due to the difference in CalEnergyRaw, the algorithm corrects for the missing energy, based on MC

LL: cannot really answer your question Nicola, we do not know if we have an excess in the data or an underestimation in the MC, but we have evidence that the energy deposit is what is expected from the longitudinal shower profile and the material upstream the CU

PB: not that obvious, the fact that we do not have a continuos cal but a cal with gaps says it is not obvious. all the work done with shower profile fitting was done with a continuous cal. all we know is that the algorithms are optimized on MC, so if we have to change the MC we have to redo the algorithm optimization, so I hope we can change the data (with calibrations or so)

discussion on error bars in Nicola's plots

Summary plots

LL: compiled a list of plots and updates that we should collect to summarize our results for us, the collaboration and the G4 developers to request their collaboration and help. Please review the list, add suggestions and start attaching the plots

LSR: might be useful for muons from LAT CR tests. For cluster size, especially for gammas and e, would be useful for separate 1st track from remaining tracks, as so called remaining tracks are kind of random collections of hits, so more meaningful for the 1st tracks
MNM: can we use svac or should we use recon files?
LSR: can use root files
MNM: done this work using the recon files, so need lots of space to store recon files
LSR: have similar problem, finally broken down and now submit jobs to SLAC farm, impossible to do this on my laptop
MNM: could you do this work and check if we can use just the SVAC tuple?
LSR: not sure I can do it in a short time frame
MNM: which SVAC version should we use? we know of a problem in the last data sample
JB: looked at total nb of hits, w/o cuts it is the same, there were many changes in the CalRecon, and this changes the way we identify photons
LL:yes, but if the changes in binning the events energy are so big (btw it is a systematic effect if you look at the plot) then we should see it much better in a CAL related plots, some energy only plot, like correlation of the CU energy and tagger energy for tagged gamma runs
LSR: plot of the energy distribution might be enough? we should see different shape in those distributions
LL: we will see if the differences disappear with the new BTRelease, and also check some energy plots

PB: I have almost all results for the correction factors for the CAL, will show that soon. there is no clear trend so not sure what to conclude. For the profile fit I am not sure we can gain something given all was done with solid models

BL: best would be to directly compare egse4 and geant to know if shower profile is correct. David will do something on this.

LL: anything on MIP discrepancies for CAL too? Berrie had something
BG: we have it for pions too
BL: this effect is a feature, been around for years, the width of the energy distribution is larger than MC
BG: at least at cern we had a well defined MIP beam,
BL: we saw the effect at GSI too, it is already published in our paper. the effect is in the width of the peak, the actual position is fixed by construction in the calibration procedure

LSR: one thing on my list was some kind of backsplash in the TKR, not a 1st order effect, but maybe a 2nd order effect on reconstruction efficiency showing in some special events with saturated buffers or so
TJ: happy to work in that if you give me details
LSR: will do

Comparison with Mars15

DP: compared g4 and mars15 energy deposit for 30 rad length solid cal, 100GeV incoming electron; big differences in the total energy and energy peak. in general g4 gives good resuls, mars15 seems bad. I contacted the mars15 author and told him, he revised things and found a problem in LE electrons, so I will try with EGS (help from Hiro on installation)

PB: I wish to remind that we have to prepare a summar talk for the collaboration meeting