BT Meeting Minutes 24 october

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Unedited notes taken during the meeting, correction and additions welcome LL

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news

Data reprocess

somehow good and bad news together. Good: the new BTRelease is in the pipeline. we had to face different problems, like python versions to talk to cal db, it took some days to start. Bad: we forgot to change the calibration flag in the JO, which are different for the new BTRelease, so the reprocessed runs that we had produced need to be redone from scratch. i sent an email to the beamlist but it did not work.

NM: I would like to add the Full-Brem run at 30 deg to the golden list, since we always use it
JB: just send the run numbers

Simulation status

can check the link, my slides give a summary
2: we now have a large electron scan with new BTRelease. residual problems on beam spots (shown by nicola last week), which I am fixing.
run 2082 was generated in several different configurations listed here. Good samples to look at, I will show something.
I also did several runs to get a cerenkov pressure scan. IN the last runs only we had the right pressure corresponding to the real data runs, will show something done by Carmelo, and Philippe will do as well
3: FB runs: we have 3 so far, that is why nicola asks for more. Run 1445 simulated in different configurations
4: tagged photons. i do not remember if we talked about this last time, but the important change is that we now have a realistic tagger spectrum which we obtained by changing the current in the MC. hadron runs are missing, I will move to them next
5: some comparisons. Alignment effect on TkR variables for 20GeV e at 0 degree - Leon fix to alignment is effective and there are no extra hits added from the fixed alingment procedure
6: bari digitization effects on tkr variables for run 2082. there is some effect on ToT, but it requires more investigations to decide if we really need that digi algorithm or not
7: cal and tkr vars between two MC, the std GLAST physics MC (black) and the LE physics MC (red); it seems we now have solved the handling issue of LE physics, and see no major differences, only less than 1% in the energy scale. There is some effect on the TKR hits and clusters, we need some investigations here to decide

DP: can you quantify how much time you need to run LE physics wrt GLAST physics? a factor 2-3 or 10?
JB: I think michael kuss quantified that in less than a factor 2
DP: you ran the std MC with std range cuts?
JB: yes
DP: do we plan to run with LE physics always?
JB: we need to identify some runs to check and decide
BG: how does LE physics get rid of HE evts in the tails?
JB: I did not say that, I just said that there is a minor shift of about 1%, whereas before we had a shift toward higher energies but that was a bug which we fix
BG: does this package change interactions of MIPs as well?
JB: no, it is implemented only for electrons

SPS cerenkov pressure scan results

Johan (for Carmelo)

Now that we have a new MC with a corrected cerenkov pressure, we wanted to reproduce several runs with different pressure, and made them.
2: scanned from 0 to 10 bar for run 2082 (non-realistic). interesting region is up to 2 bar where the pressure can be. there is clearly a strong effect in the TKR hits and clusters, almost a linear correlation up to 10 bars
3: cal variables: impact is much less important wrt to tracker variables. the scan 0-2 bar only changed about 100MeV, and data are still very far
4: energy profile for these runs, you see that in the 0-2 bar range the change is minimal; when you go to greater pressure, you start to see the shift of the whole shower and see the shower starting earlier - again data are way off.

BG: what is equivalent X0 fro 10 Bar?
JB: do not know
PB: 1 bar 1.1X0 (see PDG)
BG: so 10 bar would be 1X0 which scales with the plot
PB: right, clearly not possible
DP: slide 2 is essentially saying that different pressure cannot be the reason for the difference data-MC, as the 'right' pressure would be different for hits and clusters
PB: do not agree, TkrTotalHirs from SVAC is nb of hits, while Tkr1CoreHC from merit - you need more variables to say that
JB: the point in checking Tkr1CoreHC is that Bill extensively use this. 1bar is the real value from the logbook

Philippe

2: first looked at CAL energies. we tried long ago to add extra material along beamline, but since the ratio was never above 1 we concluded it was not the right way. Recently the issue was retriggered by Calice people at the lab here that told Berrie they needed 0.1X0 for the H6 line simulation to agree with the data they took in the same period of our run.
BG: they not only told me that, but they also had to set the g4 cut at 1% of active material thickness
DP: you mean distance cut
BG: yes, distance cut had to be 1% of whatever active material the beam crossed
MN: what was the G4 version they use?
BG: i can ask
MN: so extra material should be very far away, since H6 is the same line split very far away
PB: they did not try to explain the origin of this, they just added and it works now. there is a relation between H4 and H6

3-4: layer deposit for layers 0 and 7, difference due to position wrt to shower max
5: tried to minimize discrepancy over all layers using 2 parameters, an overall scale and the pressure
6: for 50 GeV: in each line i plot an angle, each column is a layer. you can see the max of the shower when the dependence with pressure is null. the red point is the pressure coming out of the fit
7: all configurations - best fit would require 2.25 bar and 0.93 scaling factor
8: residuals after applying the scale factor and the pressure. you can see that a solution that is good for 20GeV (left plot) does not work for 100GeV (3rd plot). For 0 deg data tend to be above MC, for 30 deg it is the contrary, so this is incompatible with extra X0 along beam line and extra X0 in CU. I tried with extra X0 in the CU and the fit results in a negative x0
9: residuals for layer vs configuration - rules out a single scale factor
10: tried an energy dependent scale factor, but it does not help.

second link is a plot for tkrtotalhits, the avg value for all configurations (i.e.pressures): the red horizontal line is data, the red vertical is the best fit result and it is 1bar for all configurations and there data and MC macth quite well

JB: i guess an extra x0 very far away from the CU would help, w/o changing so much the tkr hits or clusters - can you verify with the calice people?
NM: cerenkov on h4 are two, so your 2.25 pressure would be about 1 in each cerenkov? btw, 1bar should give pions above thresholdaccording to H4 documentation, so the setting is in principle wrong
JB: you remember we had hard times putting the cerenkov in operation, i recheked the logbook several times, we used 1bar below 50 gev and 0.1 above
NM: were they in the trigger?
JB: should check. 20GeV run is actually quite clean, no hadron contamination

LL: i think we can summarize and say that we have a fair comparison for Tkr cluster variables, and this is due to the new MC and to the modified cerenkov pressure set at 1bar in the new sim. This is in line with the pressure scan test performed and the analysis that both carmelo and philippe showed, and it matched with the logbook record. I think we should quantify the systematic error we make in using the average of the distribution to plot the trend, maybe by taking different data runs in similar confiruations (i mean similar beam spot positions wrt to the silicon wafers) and check the fluctuation of the average. This is similar to using fiducial cuts as Philippe made using the same cuts he uses for the CAL analysis. For the CAL variables we still are far from MC, and philippe demonstrated there is no single scale factor that accomodates the discrepancy for all configurations. We should investigate on extra material along the beam line with a variable slab at variavle distance wrt CU to check. Will modify the sim to do that.
We will double check the cerenkov pressure with benoit

Tkr Cluster comparison

conclusions: we have a deficit in the MC hits, a surplus in MC clusters
PB: which pressure did you use at 280GeV?
JB: above 200GeV we are sure we emptied the cerenkov, so the pressure i applied in the sim was 0.1. also, i do not know what empty the cerenkov means? 0.1? 0.01?
PB: so runs from 10-100GeV were simulated with 1bar, above 200 with 0 bars
JB: yes, the pressure is summarized in the table on confluence
NM: so the drop is due to the cerenkoiv pressure change?

BT-like evts from orbit data

NM: just a reminder that we have an ongoing analysis, trying to select some evts by using data on orbit from similar CU confiugurations. we analyzed all the orbits, at some point we could show this analysis at the ISOC meeting and the CA group
LL: yes, good to pursue this, makes sense

AOB

NM: please preregister to the CA workshop