BT_minutes_9_may_07

BT notes 9 may 2007

Present: Luca Latronico, Eduardo do Couto e Silva, Elliott Bloom, Carmelo Sgro, Johan Bregeon, David Paneque, Philippe Bruel, MarioNicola Mazziotta, Monica Brigida, Tomi Ylinen, Yvonne Edmonds, Benoit Lott, Alex Moiseev, Hiro Tajima, Ping Wang, Leon S Rochester

these are notes taken during the meeting, please edit and correct them if necessary, Luca

News

CU handed-off to SLAC, follow SLAC operations here LL: many thanks to Carmelo, Luca and SLAC team

G4 Standalone simulation: some TKR results - Carmelo (pdf)

slide2, based on joahn and franz code, tested several configs slide3, all example plots show no significant difference slides4-6: difference plot for all configurations, all compatible with no effect; also plotted energy shower for penelope, we know already there is tendency to prefer lower energies slide7: questions for more studies

sasha: earlier results showed that honeycomb affect cal energy profile LL: difference produced by penelope vs g4, not by geometry - our conclusion was that penelope is not reliable at this energy range sasha: still interesting, why not continue johan: running on a stdalone cal module geometry, where you expect g4 to be right, there is a big difference, so we concluded penelope is not reliable cs: i will look at cal differences anyway

TKR Hit deficit evaluation after data reprocess - Nicola (pdf)

MNM: analized new data and compared with current MC: remember that new CALrecon has effect on selection cuts for TKR hits. slide3: summary at 0 degree shows perfect matching of new and old data and same difference with MC EB: is your definition of TKR is hit strips?

MNM: yes - all hits above threshold. same at 10 deg, small difference at 20 and 30 degrees - can investigate but not worth it

More on Data-MC comparisons: Beam profiles and TkrTotalHits in new MC - David Paneque (pdf)

DP: let's start from TKR hits to connect to nicola. most plots there for reference.

page 49: difference between TkrTotalHits in merit and svac, it means cluster in merit and hits in svac. nicola showed differences in svac tuple, i.e. nb of hits. here i concentrate on difference in clusters and we have more clusters.

slide 50: cluster differences data-MC for sps e runs at different energies, disagreement raises with energy. MC has limited stat here

page 52: conclusion of TKR hit comparison, difference with cluster

LSR: now i am confused. data and MC at 20GeV agree well, and this is hit. is this not contradictory to nicola's finding?

DP: no, this is nb of clusters

LSR: second question/comment, if things change with energy and agree at 20GeV probably means that Low Eenergy behaviour cannot be appreciably affecting this because nb of particles in shower is stable from 20GeV. still true that

clusters are more wide, but this has to do with shower production

DP: probably true, shower must have more secondary

EB: which variable? TkrTotalHits from merit?

DP: correct, that is what I use, i.e. nb of clusters

MNM: what I showed at the coll meeting the deficit in hit has an average at 20%, 10% at cluster level, so leon's comment on cluster width is correct. David you should use some cut in the cal to reject e sample contamination like pion and might flase your comparison

DP: agree, can find details of cuts somewhere in slides

LSR: all M data seem to have a cutoff at 170 clusters, from stat you would expect more, so there is something special in MC at 170 clusters or there is a tail in data that is not due to e clusters

MNM: slide 51 show data at 60 degree which might have backsplash contamination

LSR: but we do not see it in MC and you expect to MNM: it seems we have more hits away from TKR

EB: other discrepancy in MC having more hits at low values wrt to data, is this new?

DP: normalization effect, if we have less events on one side we will have more on other side

EB: shape is different, how do we know abs nb of events is different

LL: mac-disease again, discuss this offline with elliott and come with some update, let' restart from your second topic

DP: slide 1: make MC beam profile closer to data. will show modified params and comparison

slide 2: played with params after new g4generator made available by leon

slide3: details of table rotation

slide4: JO file I modified beam width, set cerenkov pressure to 0 (the case for most runs); problem is that what we measure is not what we tune in JO, so i have to do it iteratively

side 6: status summary of search parameters, good at high energy, acceptable at 50GeV, cannot get it at low

energy, can only get larger beam profile in MC

slide7: you can compute 3 quantities, CUresloution+BeamDivergence, CUresolution, Beam divergence

slide8: summary plot show the 3 quantities become different at low energy

slide9: show comparison also for PS data

BL: reason is that we probably overestimated amount of material, so dominant effect is MS and we overestimate it,

so whatever we do the MS effect is there

DP: did we not study this and found out that we have a good estimate?

BL: true, but amount is small so will not affect shower profile but will affect your beam profile

slide10: we have this small effect but it is acceptable

MNM: did you take into account trigger condition?

DP: yes, see for example right plot in page 10 showing a square profile due to trigger scintillator

page12: simple cuts shown

slide13: show change after my improvement (X0 and Y0)

slide14: show improvement on incoming angle

following slides show comparison for many runs, including some CAL variables

slide 53 conclusions: modifications successfully, passed them to Francesco for std production with many events,

plan is to move to CT variable comparison (actually variables used to compute such vars)

MNM: slide 50, did you use last TKR digit algorithm?

DP: yes, using GR25

MNM: tkr digit is v2r6?

LSR: you only get it from GRv10r6, and we are back at v9, so we do not have that yet

EB: page22, calTkrEdge, it looks like it has problems still

DP: page21 actually shows beam profile comparison, i think this mismatch is responsible for differences in page22,

this is one run where I could not fix difference (also page 28-30): i need to check these runs. any other MC run this variable is properly described. we should mention that this variable is different for CU (pencil beam) and LAT; to compare this variable for CU and LAT you must do this trick, put many different CU runs together to mimick LAT phase-space, then you can compare it in the way Ping showed.

LL: we will include this in next MC simulation. Unfortunately Francesco has been feeling bad in the last 10 days, so he cannot attend the meeting, hopefully he will recover soon

Update on Bari TKR-digit - Monica (pdf)

now implemented level1 digi, can be called in JO using shown prescription

we tested our code with 6gev p and 5gev electrons

slide3: we verified charge sharing in simulation with a p beam

slide4: also tested when p beam cross a lateral strip between wafers (i.e. strip 384); only the neighboring strip on the same wafer show charge sharing, as expected

LL: question to both leon and Bari - leon you included the Bari charge sharing map in the simple digi alg (v2r6) so if we produce MC runs with the 2 algs we will get the same results?

LSR: i did not do that, my main objective was to cure heavy ions, and the fractions that I introduced are based on measurements taken at Pisa with charge injection. interestingly these numbers are very close to Nicola's analysis, and this is surprising as Gary and Robert commented as the charge sharing should be different between saturated signal (as for heavy ions) and MIP signals (as for Nicola). I could use an average value from the Bari maps if needed, but since this is similar to the numbers I introduced the net effect should be the same

MNM: 2 different charge sharing, one is capacitive coupling in the detector and the other is xtalk in the electronics

LSR: correct, but the numbers are the same

LL: let's resolve this offline

Quick look at reprocessed run 700002024 (0deg, 100GeV)

- data/MC energy ratio in each layer is unchanged : from 1.2 to 1.0
 CalTransRMS : data (mean=19.9, rms=1), MC (mean=18.3, rms=1.15) PB: no significant change with new calrecon, in particular CalTransRMS is still different and worrying because it is very important for background rejection, I want to look into more details
 - BL: what was the difference before reprocess for CalTransRMS PB: dont remember, need to look back at old data