BT_5sep_notes

BT Meeting, 6 september 2007, Notes

Unedited notes taken during the meeting, comments and additions are welcome LL

Participants: Luca Latronico (LL), Leon S Rochester (LSR), Benoit Lott (BL), Takkaki Tanaka (TT), Markus Ackermann (MA), Philippe Bruel (PB), Hiro Tajima (HT), Michael Kuss (MK), Jan Conrad (JC), Tomi Ylinen (TY), Elliott Bloom (EB), Berrie Giebels (BG), Eduardo do Couto e Silva (EDC), Gary Godfrey (GG), Ping Wang (PW), Bill Atwood (BA), MarioNicola Mazziotta (MNM), Piergiorgio Fusco (PF)

News

- Updated BT deliverables list
  LL: presented this to the IFC on monday that well received it and was impressed by the team work. It is a useful guide for us too in order to have future spelled out
- BTRelease status - Michael Kuss
  Old sims from Francesco harder to compare as not run on boer. New BTRElease running a factor 3 slower, 5 times more memory, but we can live with that on the pipeline. We will keep on looking for the memory leak, but in the meantime the plan is to start generating full statistics runs.
  LL: next run to generate is the special run with modified TKR geometry with a vacuum layer between the SSD and the tray facesheet to test secondary delta rays propagation

CAL Calibration issues - Elliott

This is work done by Ping and myself, she is trying to calculate de/dx code and comparing to geant.
3: Weaver-Westphal (WW) is probably best code for de/dx available, talked to Weaver few times as he is in LBL. here are some results from his code
4: fred piron et al wrote a note some time ago comparing G4 with other MC and PDG and Bethe-Block (BB). he found that g4 values did not agree very well with calculations. there are in fact some differences in what is calculated: the mean energy deposit is calculated, in the case of geant, while what is calculated in WW, PDG and BB is the energy loss, expected to be higher. nevertheless there seem to be issues from geant.
5: very good agreement between ground data and MC for LAT for CAL_MIP_Ratio
6: the factor used for calculating CAL_MIP_Ratio seem to be there to match data and MC; in fact the 15% difference is wrt WW, which is energy loss (higher) and not deposition (lower)
7: ping figured this out (cuts and geometry) with help from leon and tracy, now we have another person able to do this
LSR: 20.2 is probably mm, not cm (typo)
EB: yes, got it from an email, you provided that so
LSR: number is right, if you got it from the code, units might not be
PB (from message board): it is not a typo : center of tower 10 : 374.5/2+27.84/2 = 201.17mm
8: energy deposition and energy loss plots for a 1GeV muon
9: same plot as 4 with direct CAL-only gleam simulation for energy loss added. again some issues with calculated values
10: conclusions

LL: it seems to me that the only issue is with different versions of g4 (g4v6 vs g3 and g4v5 as in slide 9 and 4) as the difference wrt calculated values is explained by the difference between energy loss and energy deposition. Did you also say that error bars are smaller than they should?
EB: error bars are correct, but all data points look different, and the most different is the last one

BL: the note was not written by piron, but by thierry reposour, you might want to aknowledge the right person. the purpose was more focused on comparison on predictions for carbon. the CsI slab thickness that was used by thierry was very thin (1mm i recall). the contribution of deposition is fairly low, as many e actually escape the slab, go out and do not release energy therein. so there is good reason for a higher energy deposition that you find for a much thicker (1.85cm) slab as you have used here

LL: which version are we using in BTRelease or GlastRelease?
LSR: v8
LL, PB: yes
EB: we are using g4v6r29p5
MK: well numbers can be different, v6r29p5 is our numbering convention related to our g4 external library version, nothing to do with actual g4 version
EB: how do we find corresponding version?
LSR: probably picking g4v8

HT:very basic question - when you say mean de/dx do you mean peak or average value from the distribution?
EB: slide 8, both calculated, we compare the same quantities in every case
HT: when you calculate mean energy loss, do you include all energy band, i.e. not just what is in the histogram which may be cut?
EB: all you see is on the plot and there are no other data points

BA: wrt calculation of CalMipRatio, since the code is ultimately from me, the energy factor was simply determined to make the peak with muon runs to make 1. nothing fancy there. calcsirln is derived by tracing tracks to the cal and making equivalent rl path
EB: thanks, very useful

GSI analysis - Eduardo

This is preliminary work, I got some feedback from Eric Grove, but I could not include it as i was away
2: motivation for this study: getting ready for leo and have limited tests for timing for cal triggers
gsi data are natural place to look at. hope this analysis will evolve into LEO analysis. hope to extend to other ion species. The main question is whether cal-le and cal-he behave as expected with C ions
3: so far analised 1 run only.
4: basic distributions, no cuts, will make simple selections. will look at most populated trigger types (22 and 30)
5: middle plot is maxene in cal, bottom plot is the question, i.e. arrival time difference between cal-lo and cal-hi. this is expected to be negavitve, cal le cannot arrive before cal he
PB: blue is 22 (cal he on), red is 30 (cal he off), so do not understand middle plot
EDC: that is the max energy in a crystal for cal-le and cal-he triggered events
BL: it seems that blue and red are swapped
EDC: could be, will check offline (later confirms that these are swapped)
6: time arrival difference for different gem cuts
7: first column is all evts, middle column is evts with arrival time difference smaller than 10, right column is time difference for positive and high time arrival difference; middle row is condition arrival time for cal-lo, bottom row for cal-hi.
8: same color code, second and bottom row are scatter plots of calmaxene vs condition arrival time
EB: what do you know about a priori timing between those guys?
EDC: inferring they are not different, but we do not know, will comment later
8: to get time just multiply a tick by 50 nsec
9: the shoulder at 5-10 ticks is from direct energy deposition in the diode, usually 5-10 ticks earlier than peak deposit. these are expected and were measured by martin during IT, but could not do it with cal-hi as we had too few events.
10: test this idea by looking at signal in nearby cal modeuls
11: twr 1 and twr3 cal-he triggers requiring either cal-le, cal-he, both. please note once again the difference in x and y axis. the point is that there is abundance of triggers in adjacent towers, so it should not be diode deposition
12: same as 11 but for cal-le
13: event display from evts with cal-he arriving before cal-he
15: another possible explanation - could it be the jitter (slide 9)? a jitter would be between 2 and 4 ticks, but it extends up to 10-12 ticks, so must be something else

eric argues that cal-lo should never be allowed to open the trg window ...

LSR: could it be a skewing effect?
EDC: will check that, will talk offline

LL: i recommend you look for clean evts, maybe cutting on tkr variables, as the instantenous rate at GSI was much higher wrt average, in particular at the beginning of the spill where the machine was delivering beam from other users and collimators were closing into our line

BL: i am not sure C events are the best for this study, as the average non-interacting energy deposit for C ions is below cal-hi threshold. did you consider looking into cern events? we should have more handles there, and we have external trigger too, so potentially you could estimate timing wrt the external trigger
EDC: right, looking at cern data too, this just came earlier

BA: slide 8 middle plot is definitely strange as you sais: the energies are all>1GeV in a xtal, nothing to do with cal-lo events
EDC: must go back and check, I might have swapped labels for cal-le and cal-he

BL: is there a schematics of the trigger timing?
EDC: yes, I will provide that

LE simulations udpate

LL: francesco and carmelo are investigating the LE simulations presented last week, indeed there is a problem in the energy release for TKR and ACD, where recent LE simulations deliver twice the energy wrt standard glast physics. Will update as soon as we know more on this.

AOB

BA: what is the status of understanding tkr multiplicities? there is a variable that we use very much in background rejection it is called tkr1corehc and it very important that we have a good MC for that
LL: we will reevaluate TKR hit discrepancy, including that of tkr1corehc, once we generate simulations with the new BTRelease, and we are about to start that. we will perform also a dedicated background simulation run with extra hits in the simulation using the alignment bug in the simulation discovered and fixed by leon that seemed to provide more hits. we are also preparing a run with a modified tkr geometry (vacuum layer between SSD and tray facesheet) to test secondary delta rays propagation with a more realistic geometry (glue dots instead of an average density layer of glue)