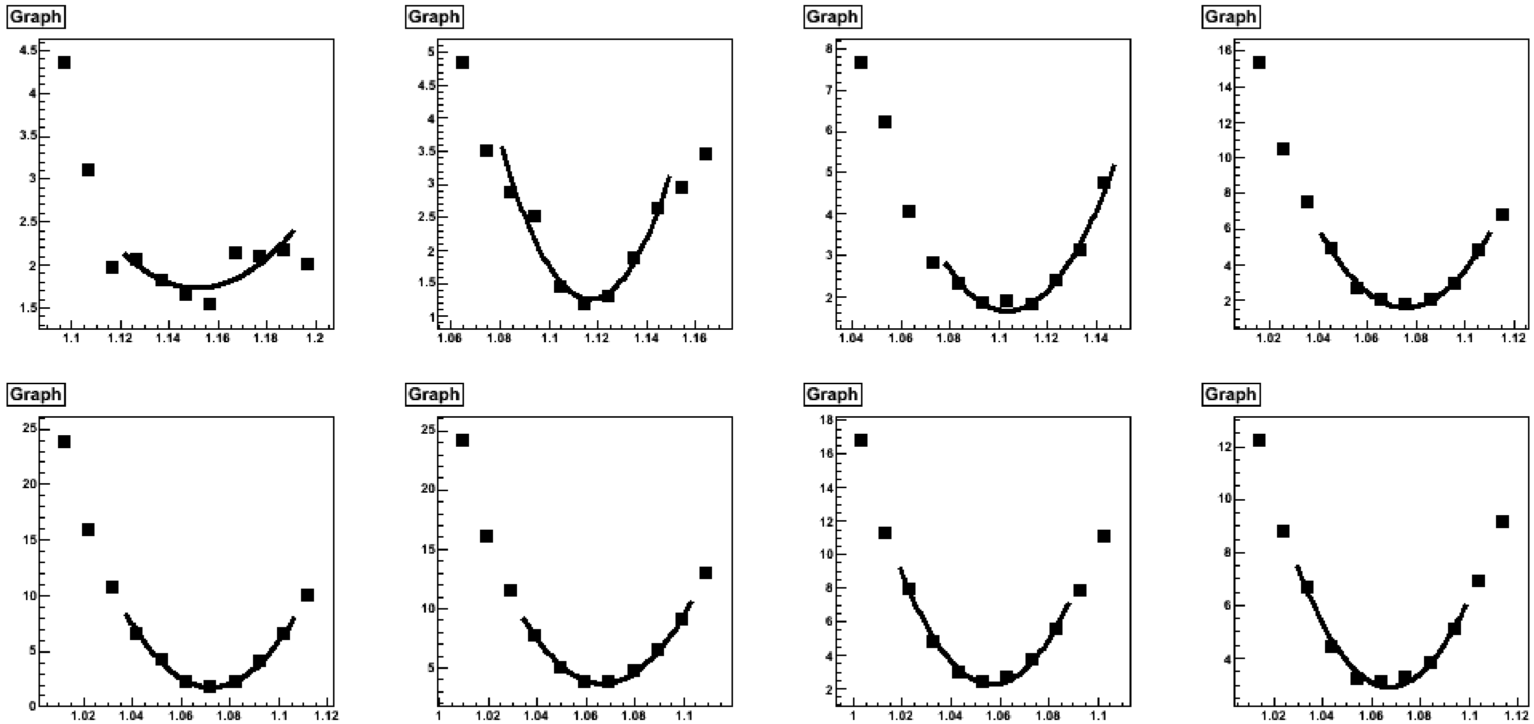
Data/MC energy comparison for electrons at 1, 2.5, 5 GeV (PS runs)

- I had never looked to 1 and 2.5 GeV electrons runs (center of tower 2):
  - 1 GeV : 700001259
  - 2.5 GeV : 700001202
- I was just curious to see if these runs tend to favour a global scaling factor of ~0.93 as the SPS runs seem to indicate
- And, as I should have expected, curiosity killed the cat…

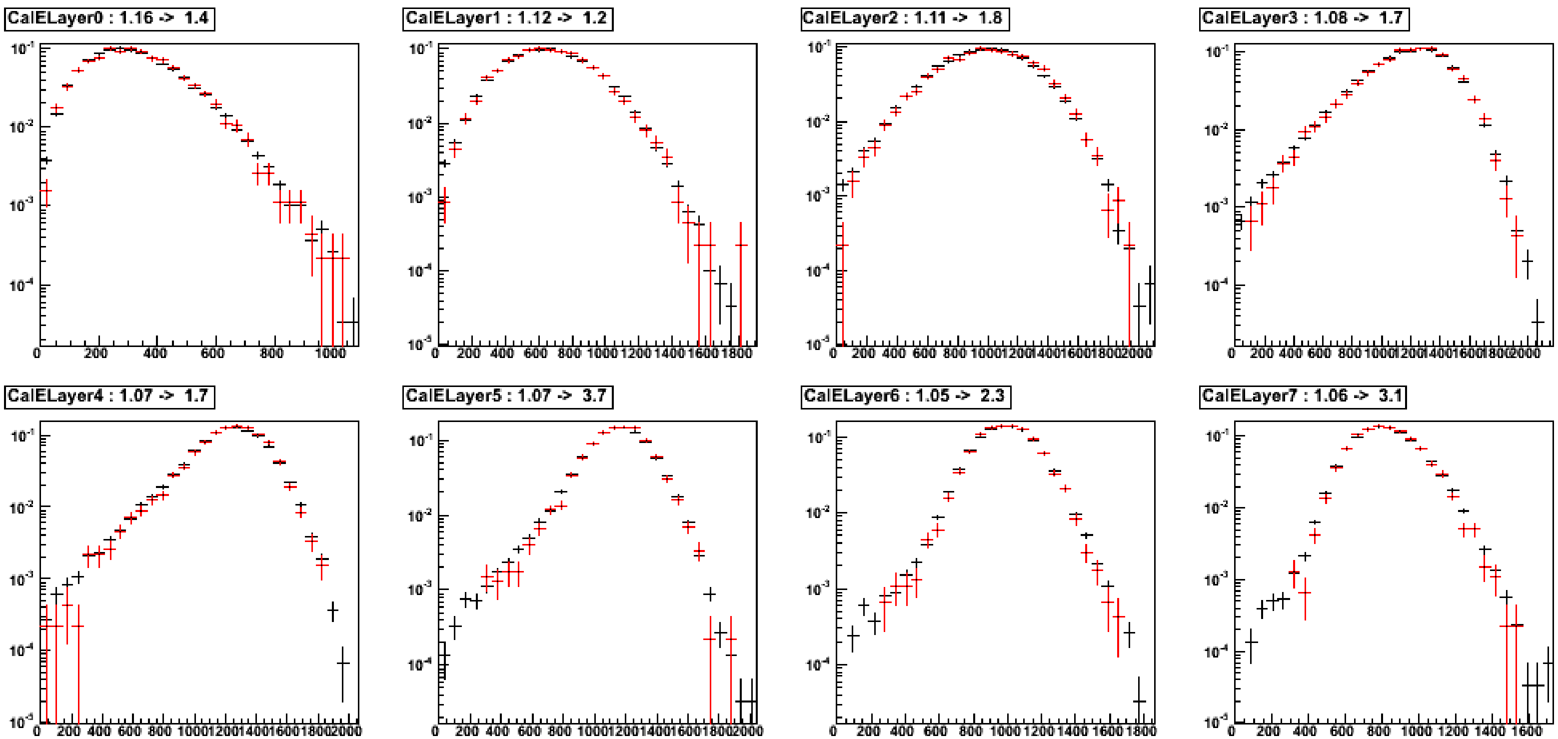
Reminder of how I proceed (10 GeV, 0deg)

- For each layer, I look for the scaling factor f that minimizes the difference between the distribution of CalELayer for data and the distribution of (f x CalELayer) for MC
- Here are the plots of the "chi2" as function of f for the 8 layers for the run 700002338 (10 GeV, 0 deg) that allow me to find the optimal f :



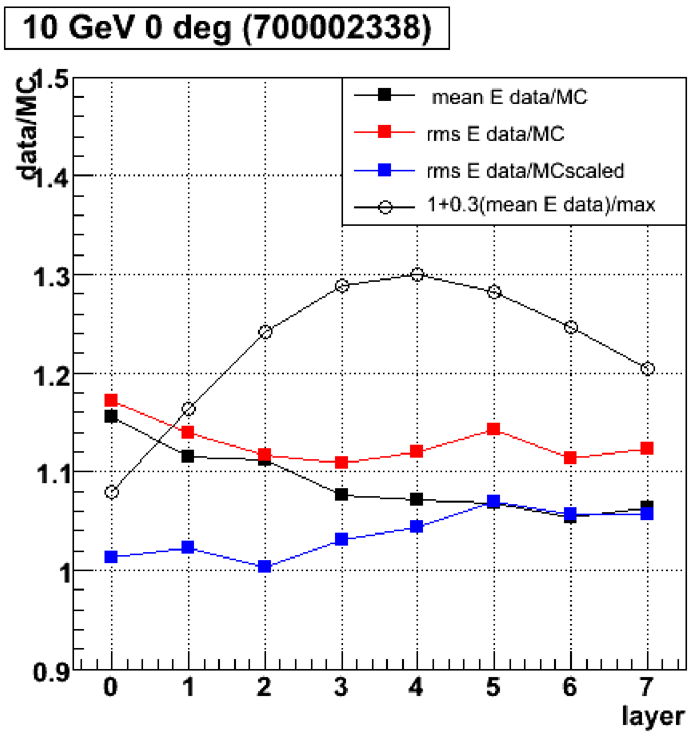
Reminder of how I proceed (10 GeV, 0deg)

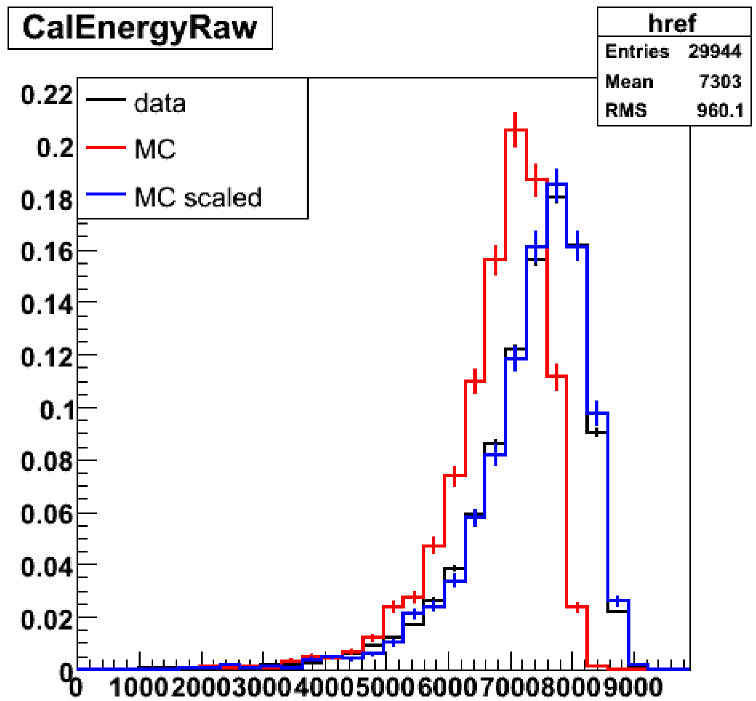
- Here are the plots of CalELayer for data and (f x CalELayer) for MC for the 8 layers for the run 700002338 (10 GeV, 0 deg). You can see the nice agreement I get.



Reminder of how I proceed (10 GeV, 0deg)

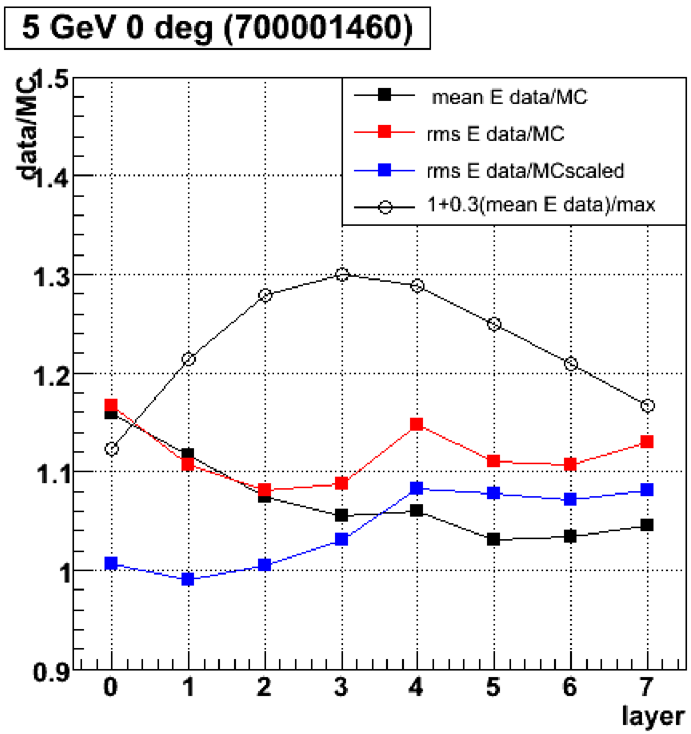
- Here is the summary plot for the run 700002338 (10 GeV, 0 deg). Right : the CalEnergyRaw distribution. Left :
  - Black : the optimal scaling factor as function of the layer : from 15% to 8%
  - Red : the ratio of the rms of the CalELayer distributions in data over the rms in MC
  - Blue : the same as the red one, except that the rms for MC is when applying the optimal scaling factor
- You can see that after rescaling, the agreement is good (and ~also for the rms)

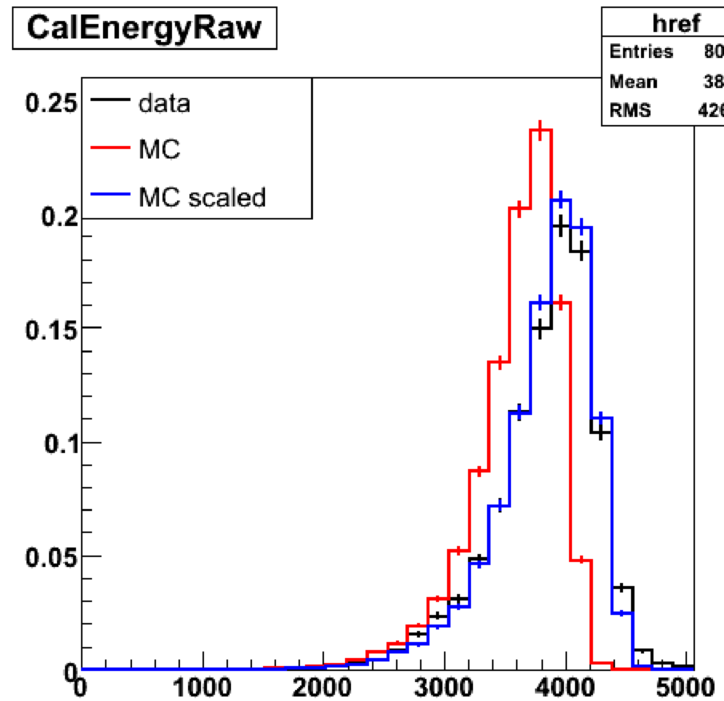




Results at 5 GeV, 0deg

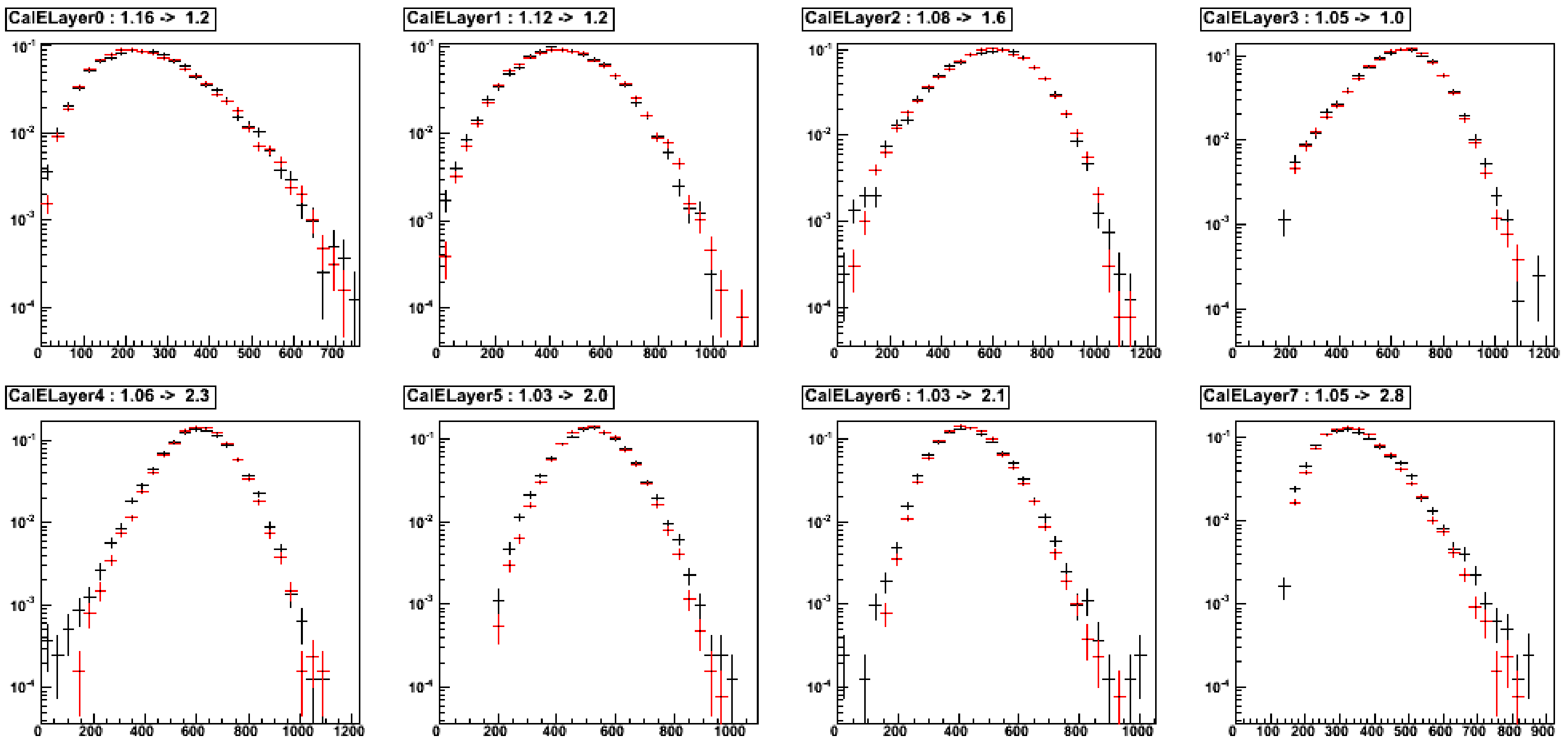
- Here is the summary plot for the run 700001460 (5 GeV, 0 deg), taken at PS.
- You can see that the results are very similar to what we have at 10 GeV (SPS). So there is no big change between PS and SPS !
- (the rms is larger for the last layers, but it is mainly due to an artefact due to cuts applied on non-scaled variables)





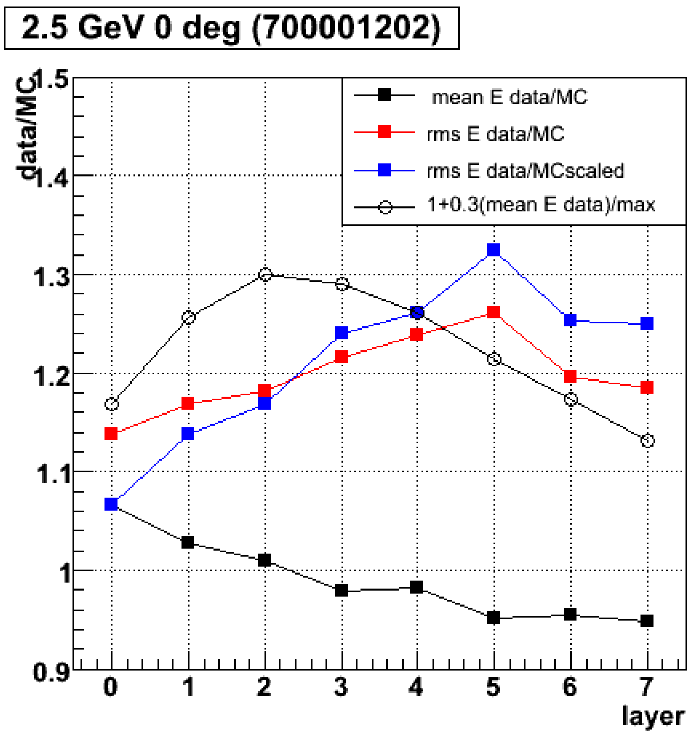
Results at 5 GeV, 0deg

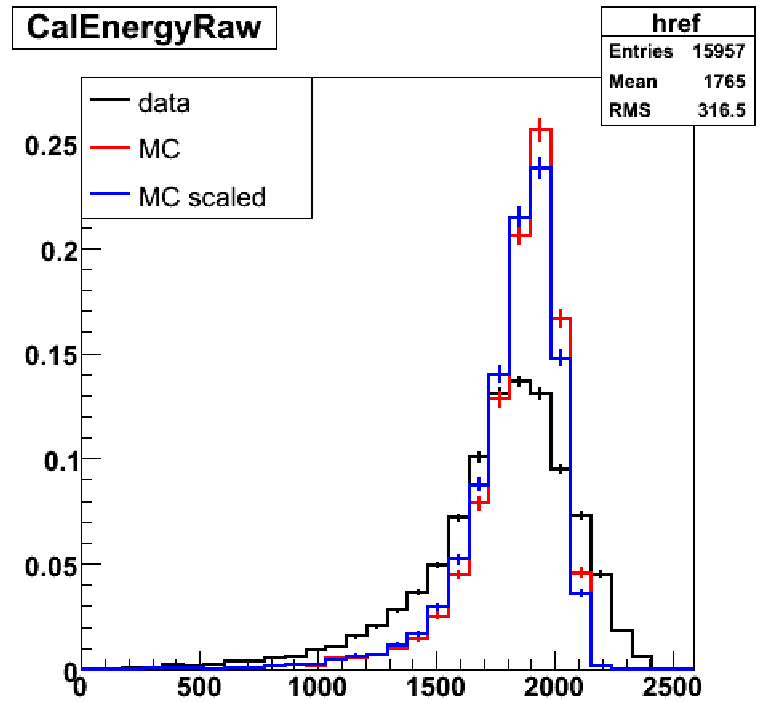
- Here are the plots of CalELayer for data and (f x CalELayer) for MC for the 8 layers for the run 700001460 (5 GeV, 0 deg). You can see the nice agreement between data and MC after rescaling.



Results at 2.5 GeV, 0deg

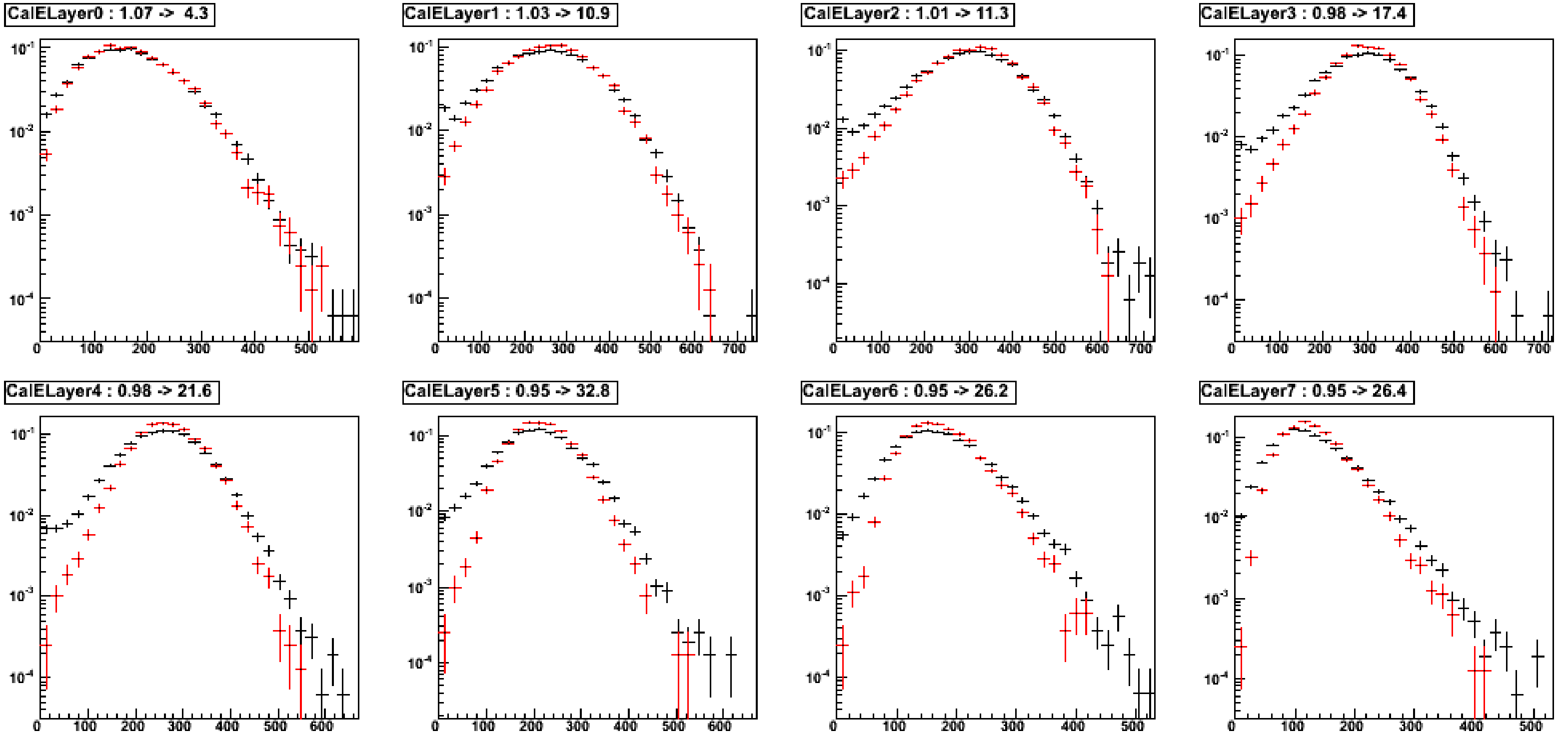
- Here is the summary plot for the run 700001202 (2.5 GeV, 0 deg), taken at PS.
- You can see that the results are NOT similar to what we have at 5 and 10 GeV :
  - The scaling factors go from 8% to -5%
  - The rms after rescaling do not reproduce the data





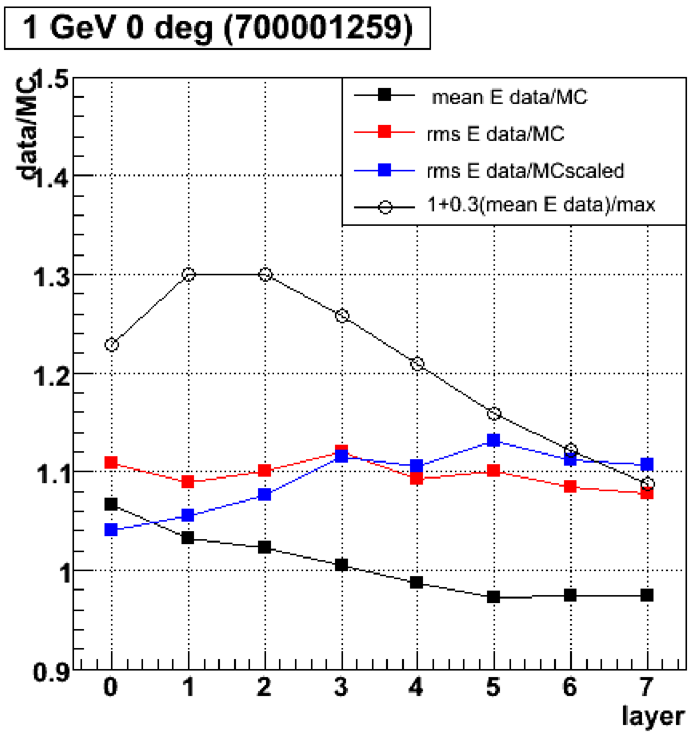
Results at 2.5 GeV, 0deg

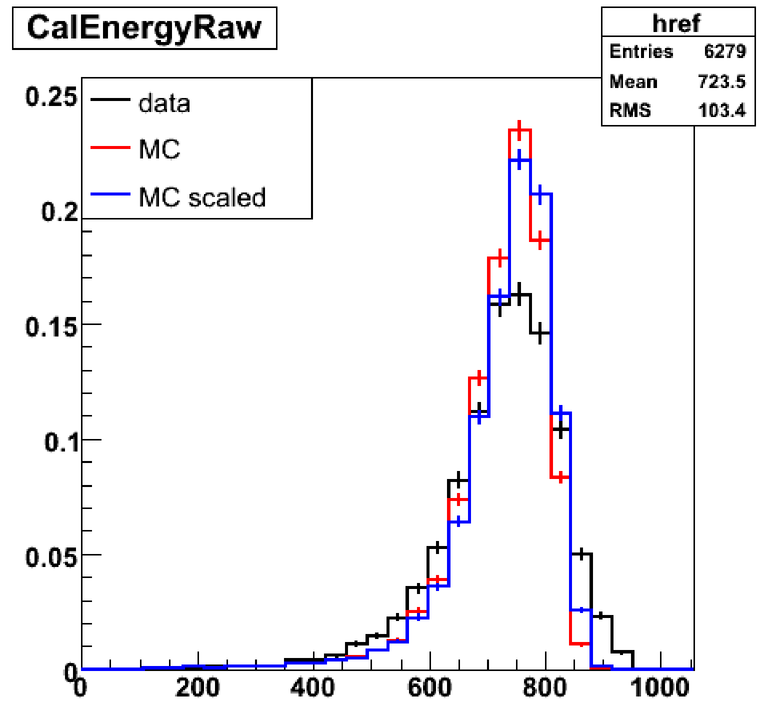
- Here are the plots of CalELayer for data and (f x CalELayer) for MC for the 8 layers for the run 700001202 (2.5 GeV, 0 deg). You can clearly see that the rms are not well reproduced.



Results at 1 GeV, 0deg

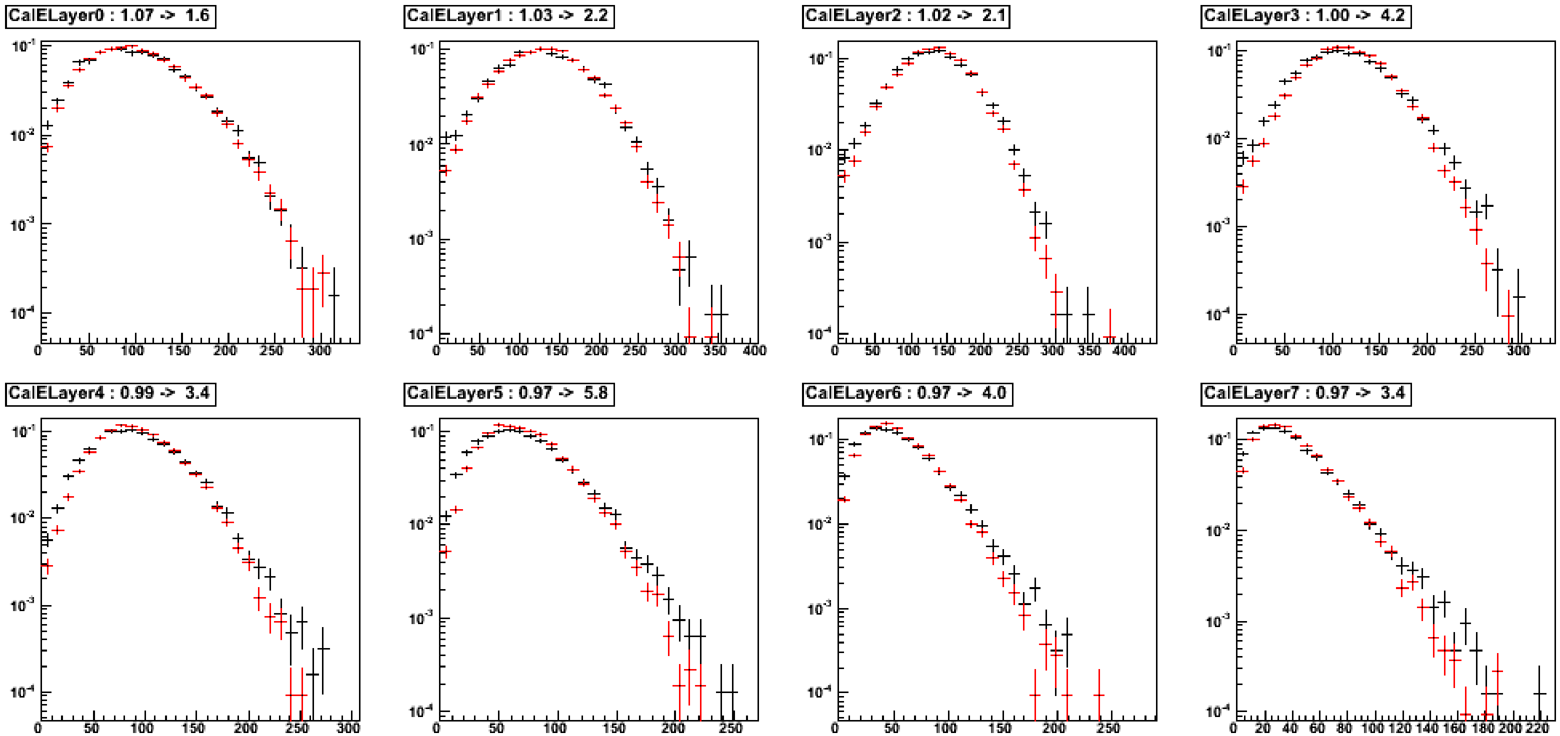
- Here is the summary plot for the run 700001259 (1 GeV, 0 deg), taken at PS.
- You can see that the results are similar to the results at 2.5 GeV and NOT similar to what we have at 5 and 10 GeV :
  - The scaling factors go from 8% to -2%
  - The rms after rescaling do not reproduce the data (it's less bat than at 2.5 GeV)





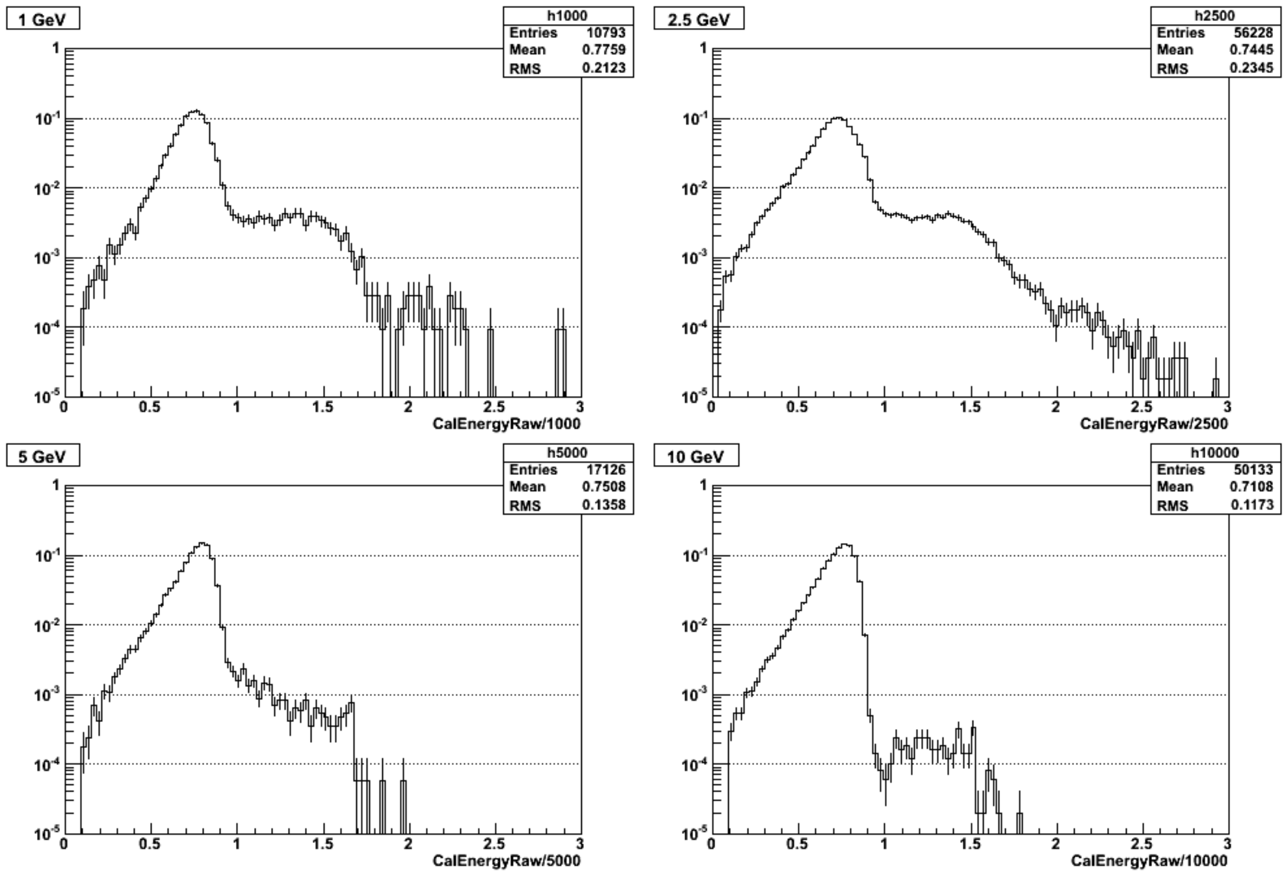
Results at 1 GeV, 0deg

- Here are the plots of CalELayer for data and (f x CalELayer) for MC for the 8 layers for the run 700001259 (1 GeV, 0 deg).



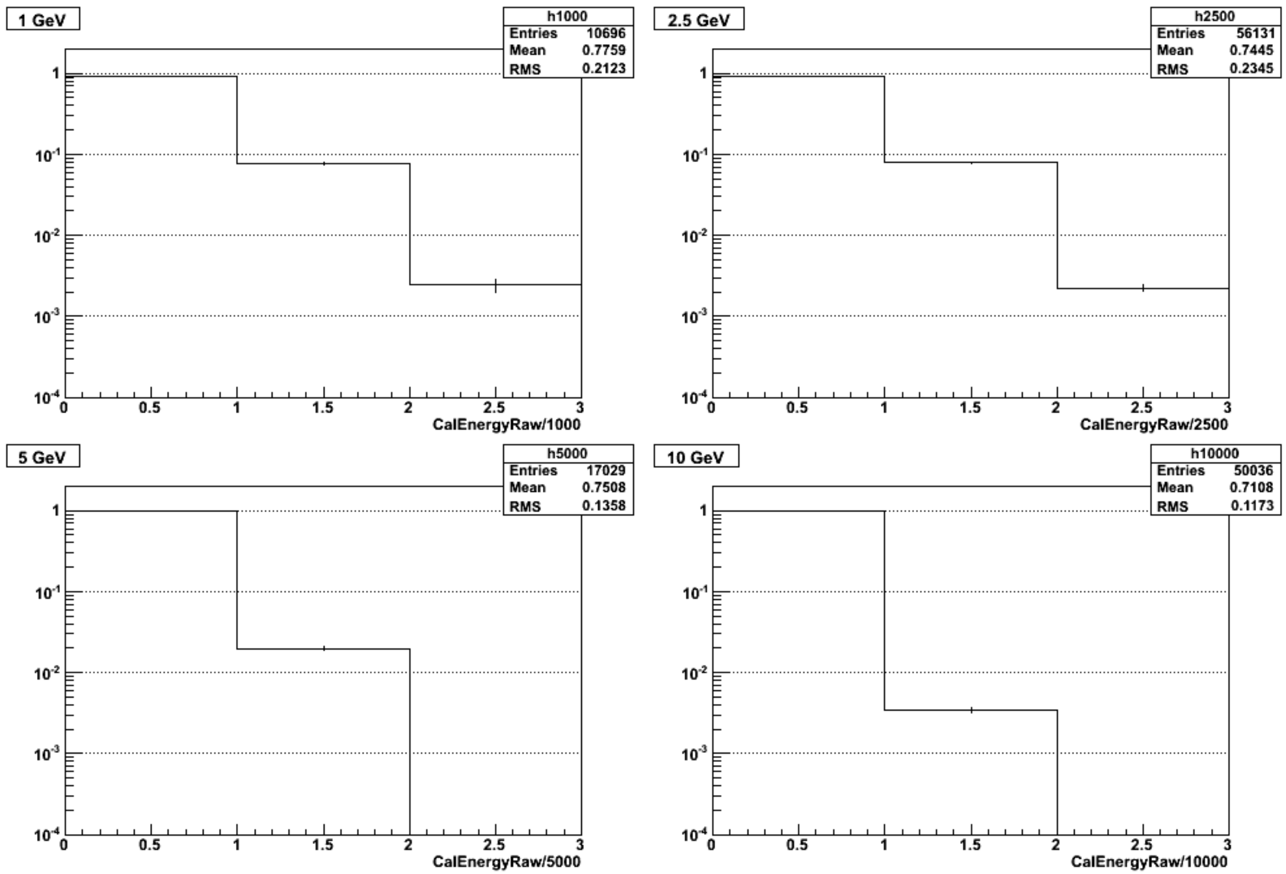
Why 1 and 2.5 GeV results are different than for E>=5 GeV

- All the results in the slides before have been obtained rejecting multi-electrons events (CalEnergyRaw<E_Beam)
- Let's look at the CalEnergyRaw distribution without cuts :



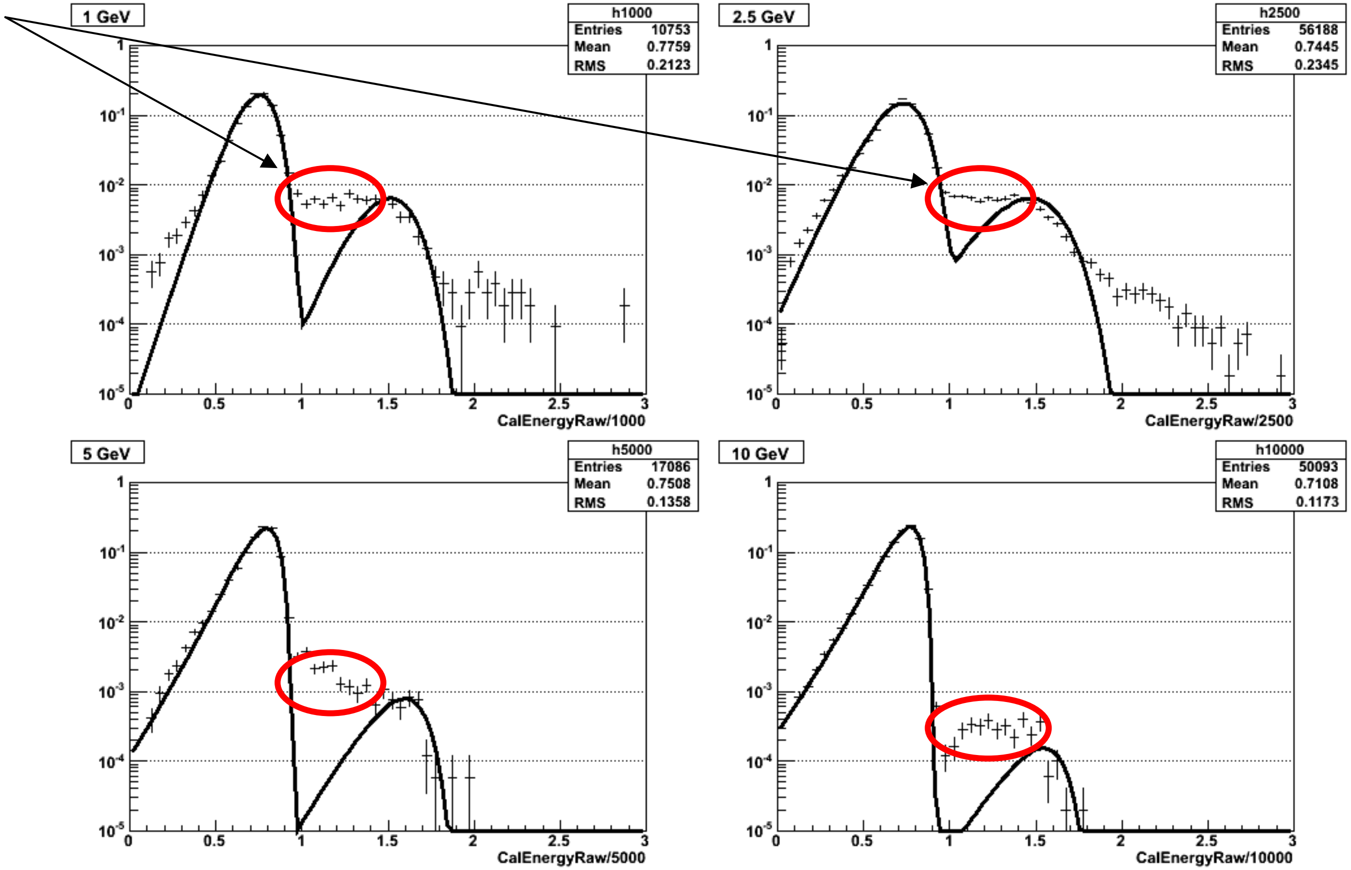
Multi-electrons event rate

- Same histograms as in precedent slide, but with nbin=3 to estimate the multi-electrons event fraction : the 1 and 2.5 GeV runs are the ones with larger 2-electrons fraction (~9%).



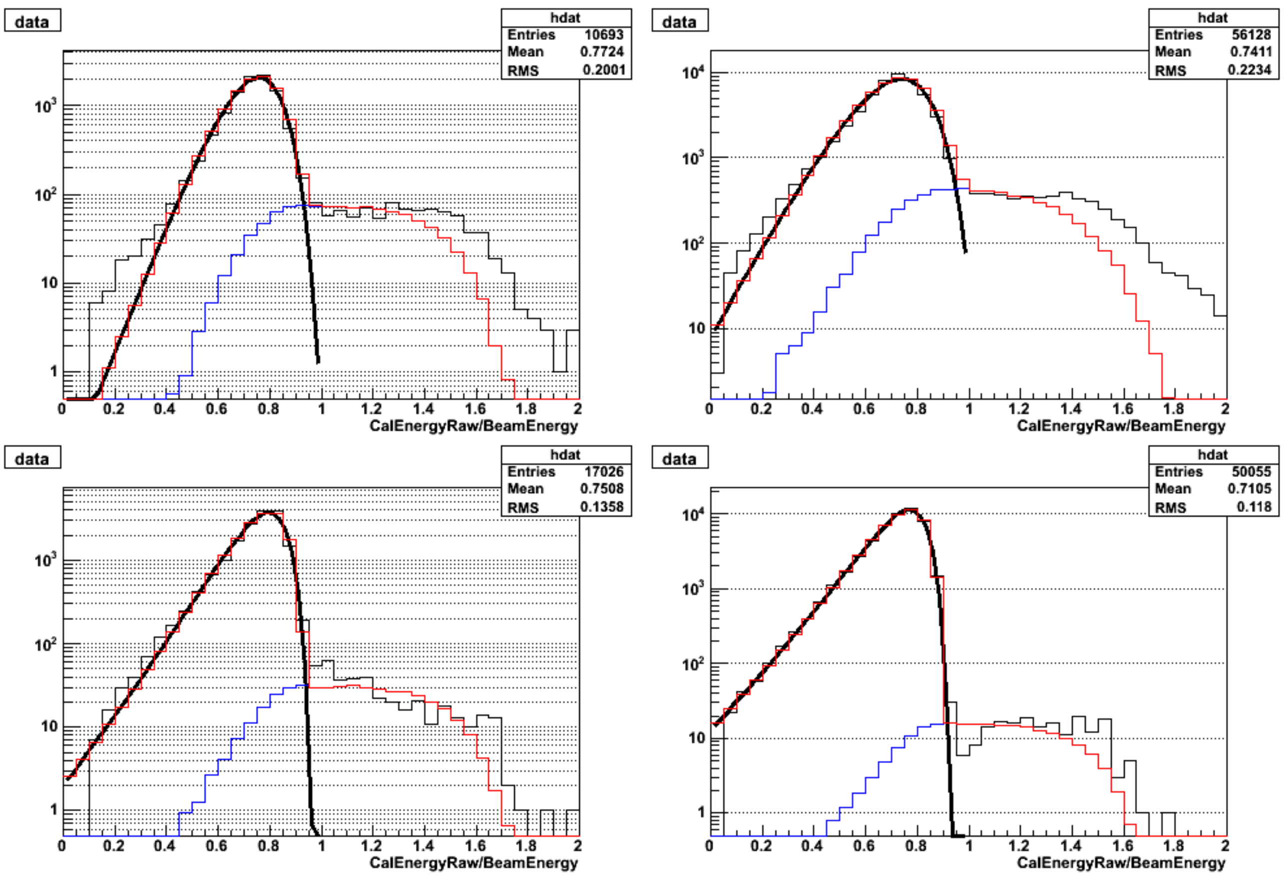
Multi-electrons fit

- Fit the histograms with fln + [fln+fln], where fln is a lognormal (for the 1 electron peak) and [fln+fln] is the sum of two variables distributed as fln (for the 2-electrons peak) : it doesn't work. The reason : the 2 electrons are not in time -> one of them is badly measured -> it fills the region between the two peaks.



Multi-electrons fit

- Let's try : fln + [fln+fln*Rndm()], where Rndm() simulates the fact that there is one electron which is badly measured (the measured energy is the expected deposited energy times a random number between 0 and 1)



Black : data

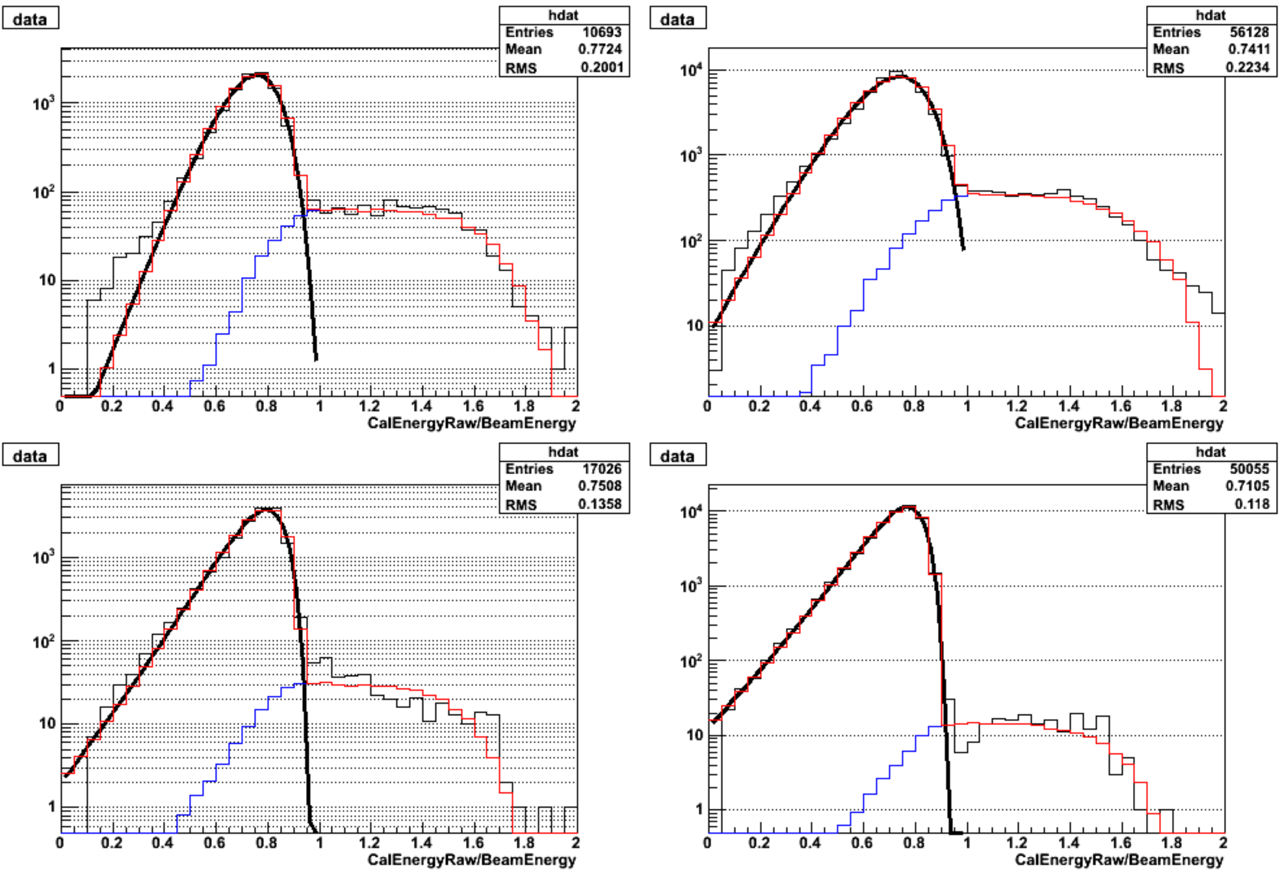
Blue : [fln+fln*Rndm()]

Red : fln + blue = total

It works better, but the second peak is still not well fitted.

Multi-electrons fit

- Let's try : fln + [fln2+fln2*Rndm()], where fln2 is like fln except for the peak position which is scaled by a factor f.



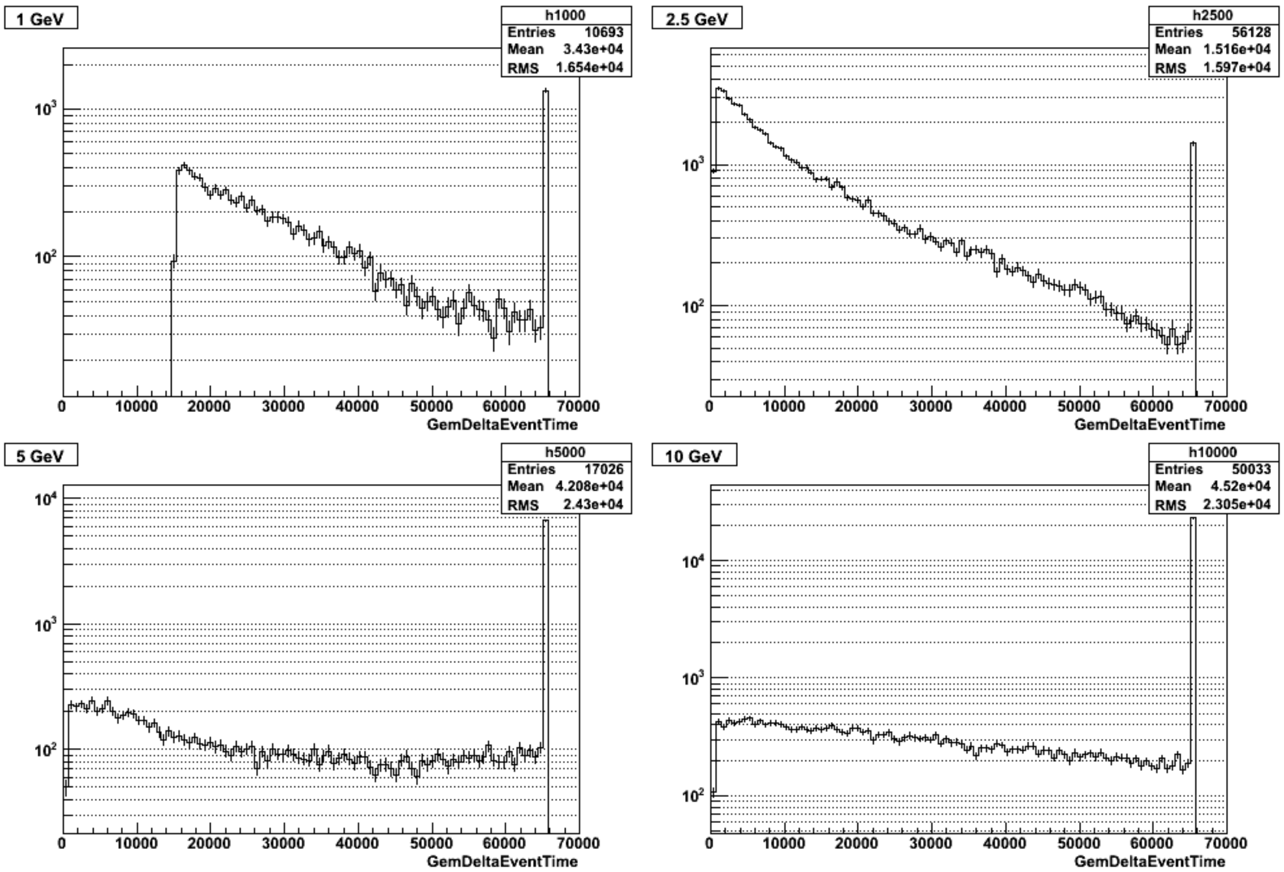
Black : data
Blue : [fln2+fln2*Rndm()]
Red : fln + blue = total

It works better. But what does it mean ? Factor f =
1.12 at 1 GeV
1.15 at 2.5GeV
1.02 at 5GeV
1.07 at 10 GeV

The 2-electrons pollution in
The 1-electron sample is
~2% at 1 GeV
~2.5% at 2.5GeV
0.9% at 5GeV
0.1% at 10 GeV

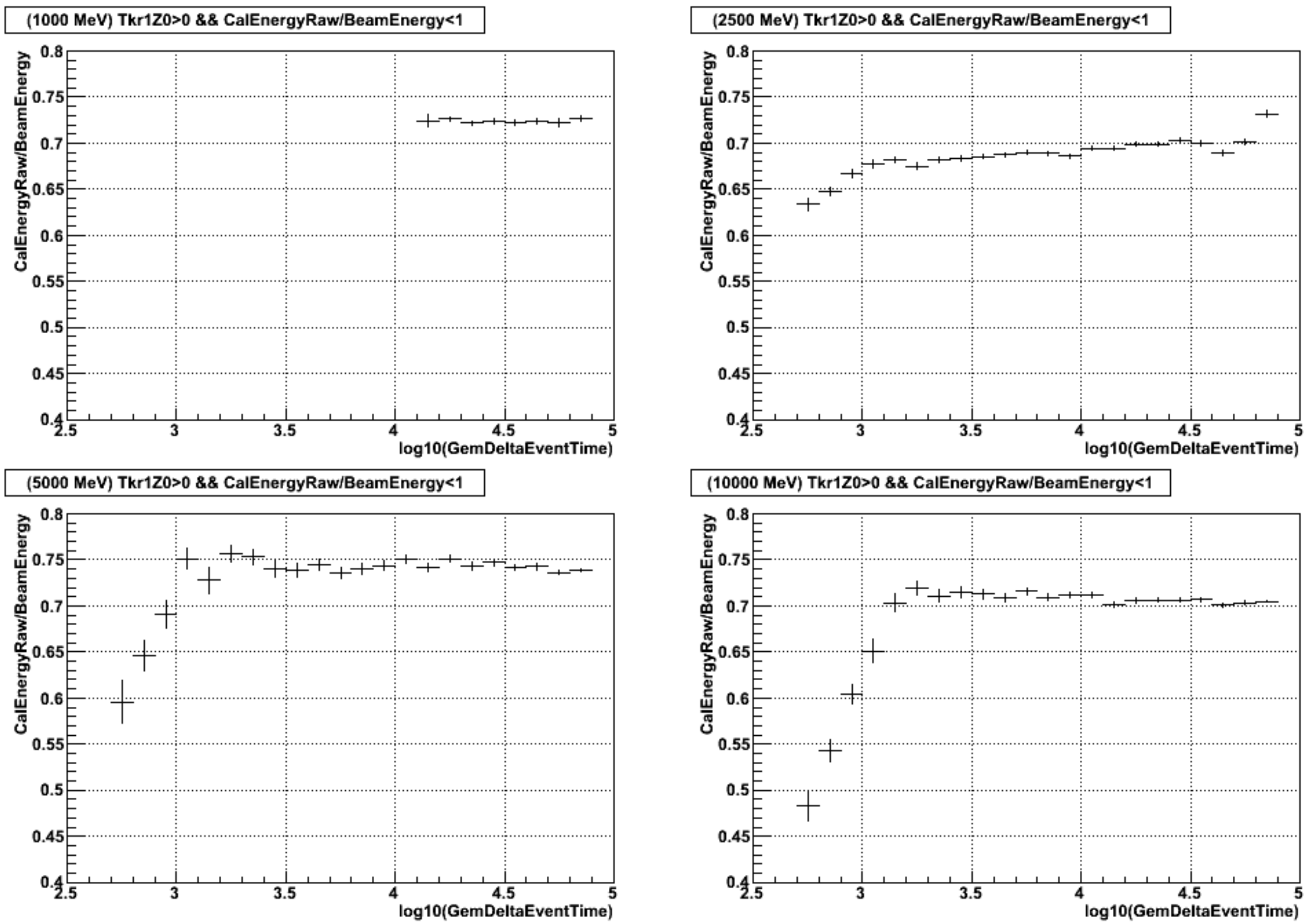
Event rate

- The GemDeltaEventTime histograms show that the true particle rate was higher at 1 and 2.5 GeV than at 5 and 10 GeV.



Event rate and energy

- Look at the average CalEnergyRaw as function of log10(GemDeltaEventTime). We can see that at very low GemDeltaEventTime, the deposited energy is underestimated. Nothing striking at large values of GemDeltaEventTime (except at 2.5 GeV).



Conclusions

- The 2 runs at 1 and 2.5 GeV exhibit a difference with the other electron runs : they don't agree with a global scaling factor of 0.93 (though tagged gammas agree)
- Why ? No answer yet, but the high rate could be a cause :
  - 2-electrons pollution : it means that the real 1-electron peak would be a bit lower…
  - Pedestal shift (?) : it would mean that the real 1-electron peak would be higher
- We have to understand this different behaviour
  - Last minute idea : the runs at 1 and 2.5 GeV have been taken with the Si chambers in the beam line, but not the 5 GeV run. Is it the case in the MC ? More X0 at 1 and 2.5 GeV would mean less deposited energy.
- Is the 2-electrons pollution a problem for the tracker variables ?