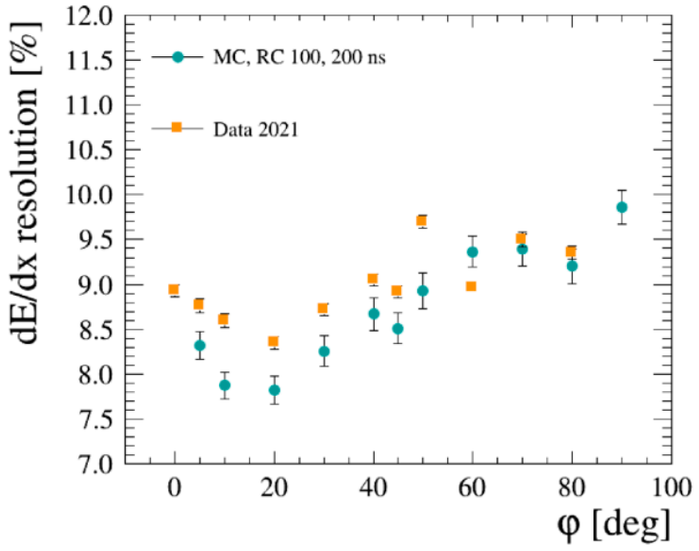
DESY 2021 test beam analysis paper draf: Discussion of the comments

22/11/2022

Comments: Gain

Line 177: Gain in MC is set to 1800 while mean gain in the data (ERAM- 01) is 1944. Did you put a correct for the gain in MC to take into account this difference? Later on in the paper, you show in figure 25 a data/MC comparison resolution. Will the correction affect the agreement?

No we didn't account for this but performances are independent on the absolute gain (this was studied for DESY2019 paper). What might have an impact are the non-uniformities in the data while the MC assume same gain for the whole module.



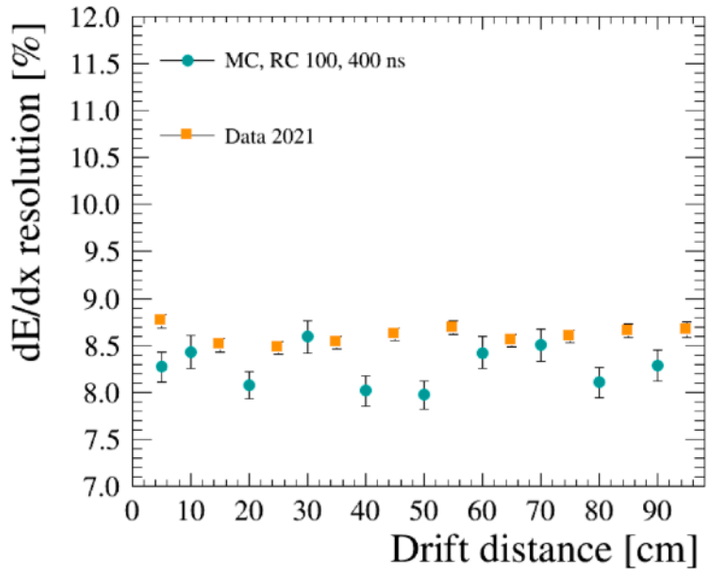
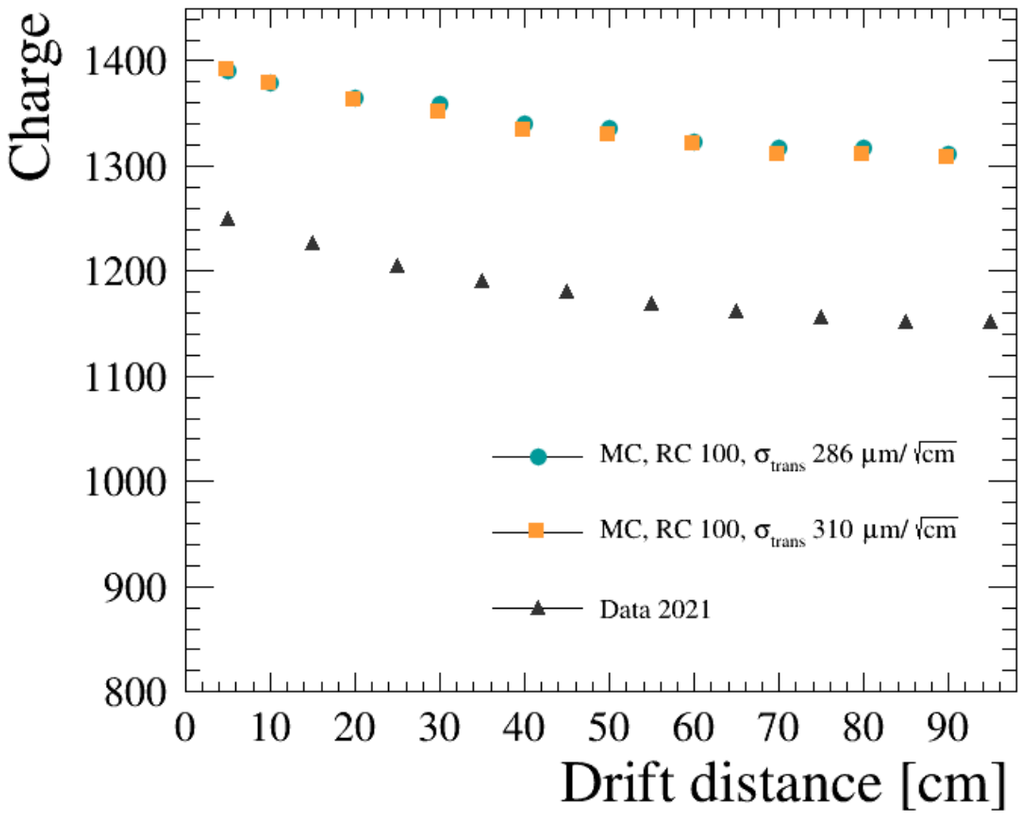


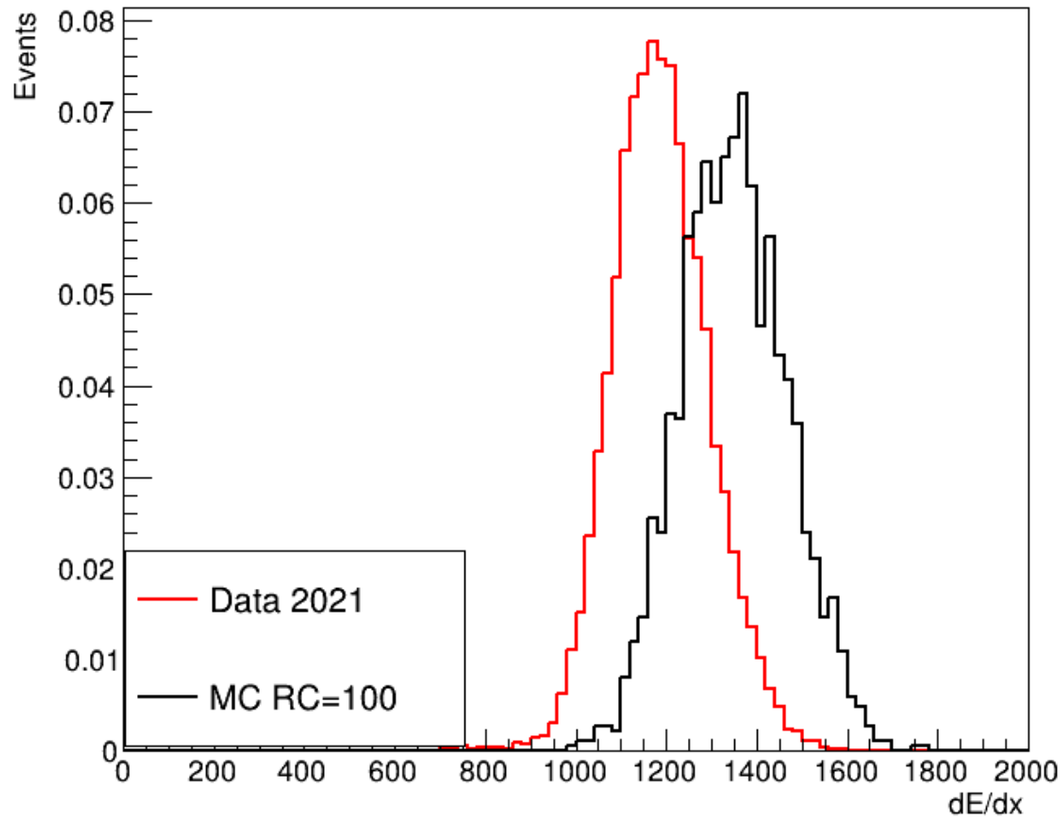
Fig. 25

Comments: Gain

Line 560: you show in figure 25 a data/MC comparison resolution dEdx resolution. Will the gain correction affect the agreement? I suggest that you show here the dEdx distribution in data and MC. dEdx resolution is not enough to explain the data/MC comparison.

dE/dx distribution indeed is different for Data and simulation.

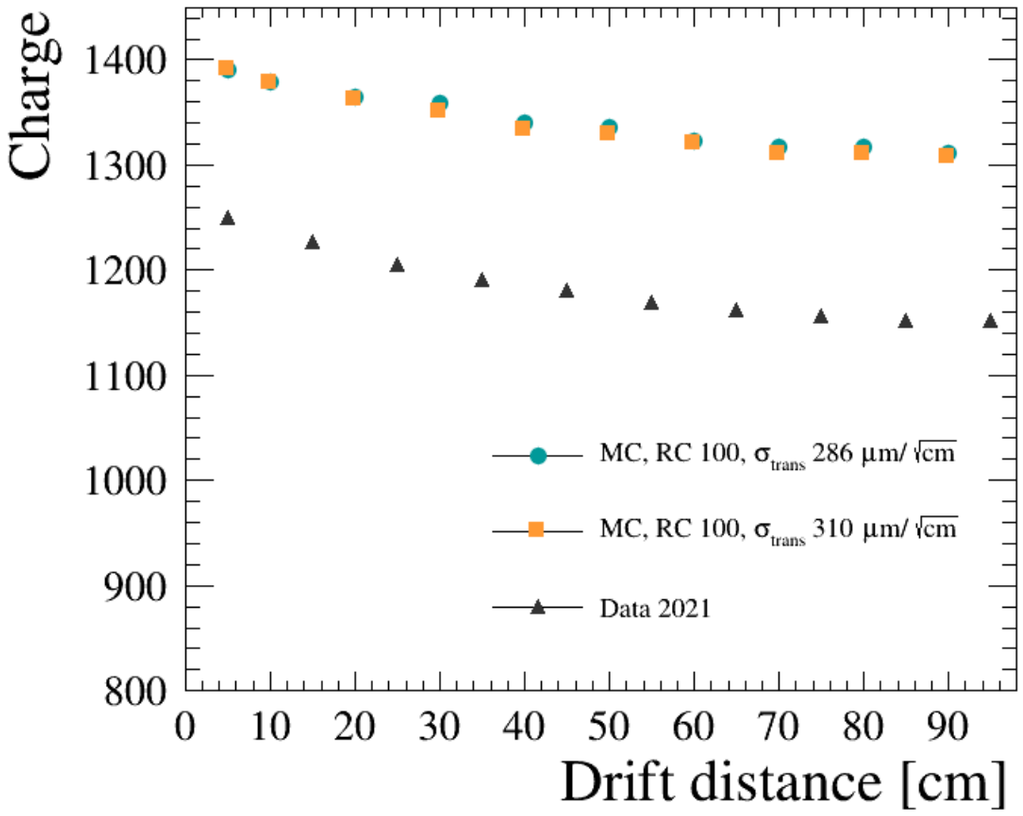


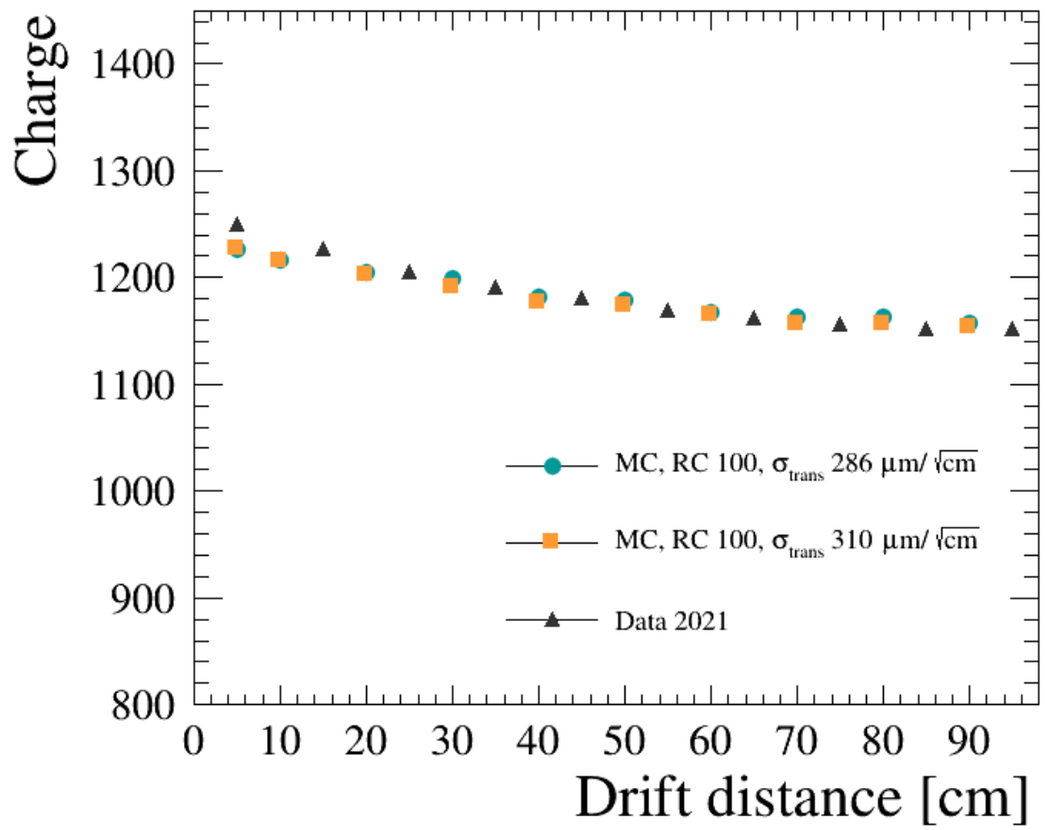


Comments: Gain

- The gain map from Shivam shows a gain of 1944. It is a gain in ADC and it is calculated as the mean of the sum of the waveforms in matrix 3x3 around each channel.
- In the simulation, we are using the gain of 1800, the absolute gain (electrons per keV) to be applied to each electron.
- The conversion factor: G = 1944 * 183/224 = 1588 —> explains why in the simulation the dE/dx is larger than in the data.

⟱ **By applying a conversion factor** ⟱

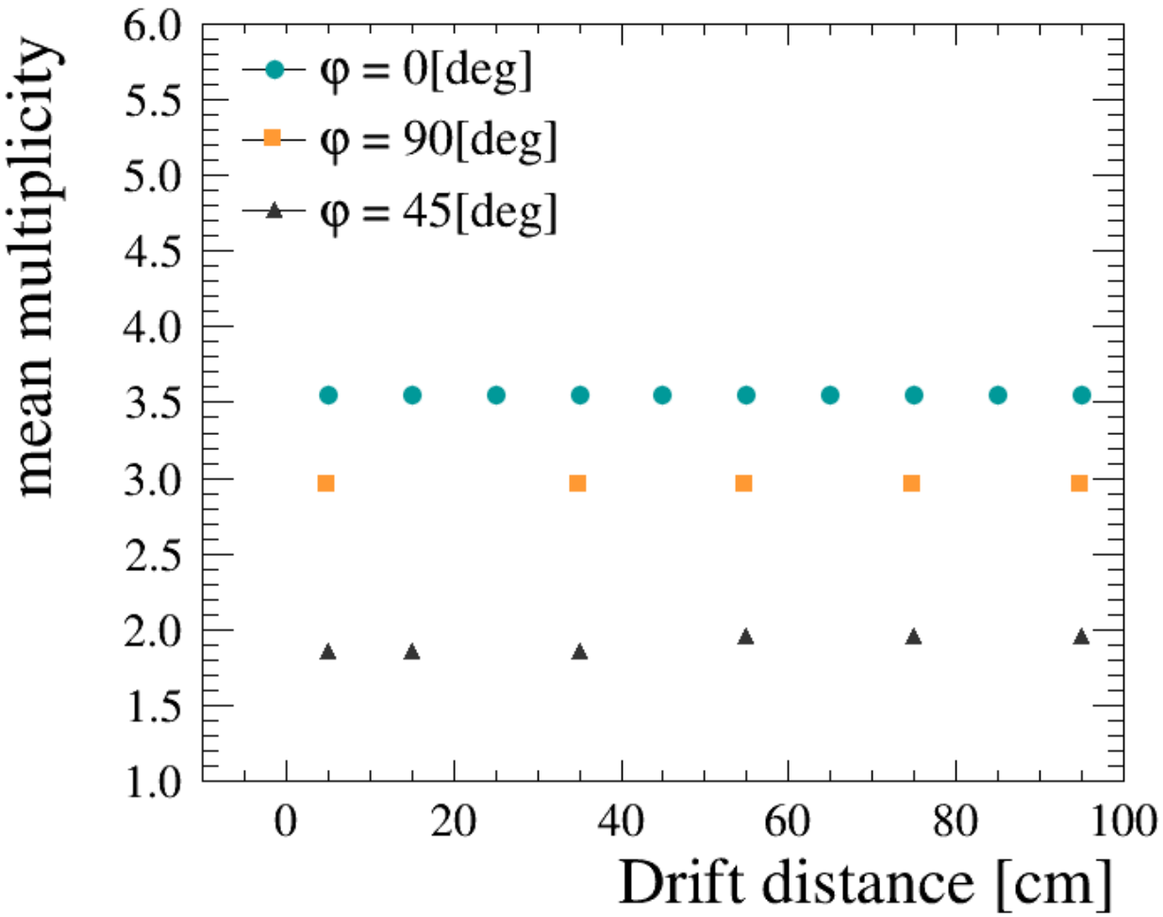




Comments: Mean multiplicity

L.306-307 There is a contradiction or a least a tension with L.440-442 where it is explained that for diagonal track, transverse diffusion is less significant

The two arguments are not the same. Here we are referring to the mean multiplicity that increases more significantly for diagonal tracks. The comment on L. 440 refers to the spatial diffusion for which the worst performances are dominated by the larger pad size in diagonal clusters and the effect of the diffusion is negligible.

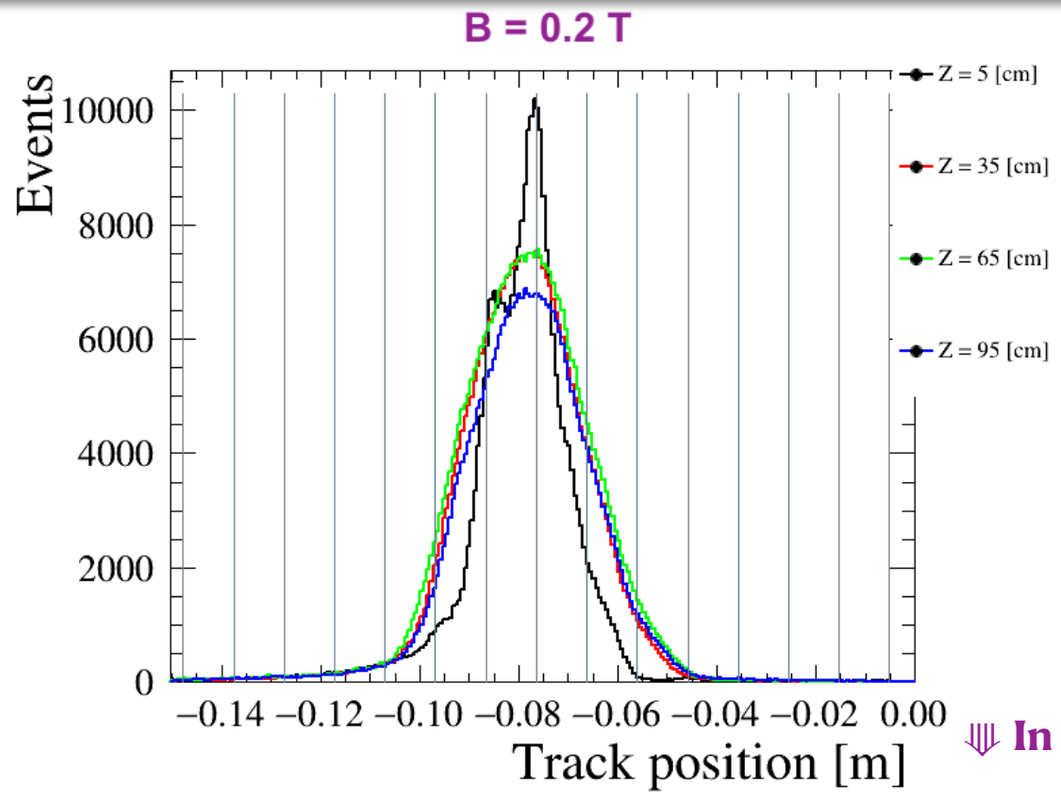


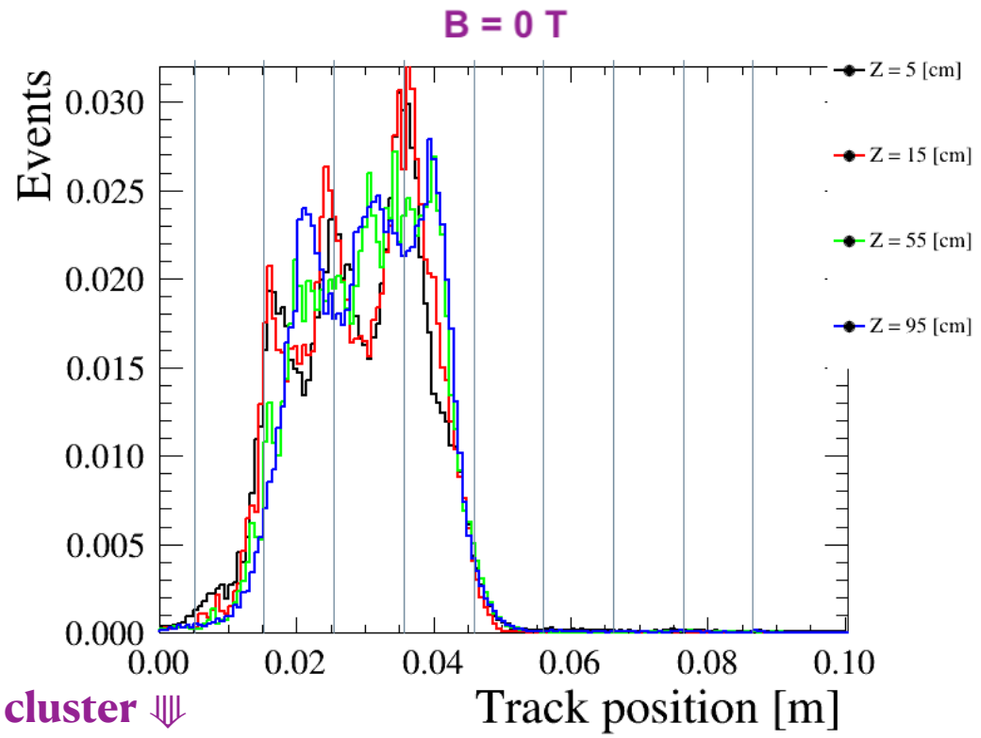
Comments: Reco track position (to discuss)

End of section 8 It is not mentioned that we do know that the reconstructedbeam profile is biased. A clarification is needed. This comment was done already for the 1st version of the note but, according to the responses to comments document, it has not been not understood why it was done at this place (end of section 9 for the 1st version of the note). The comment is done at the end of the section 8 here since this section covers spatial resolution and bias. This section is therefore the place where the fact ”that the reconstructed beam profile is biased” was expected to be mentioned.

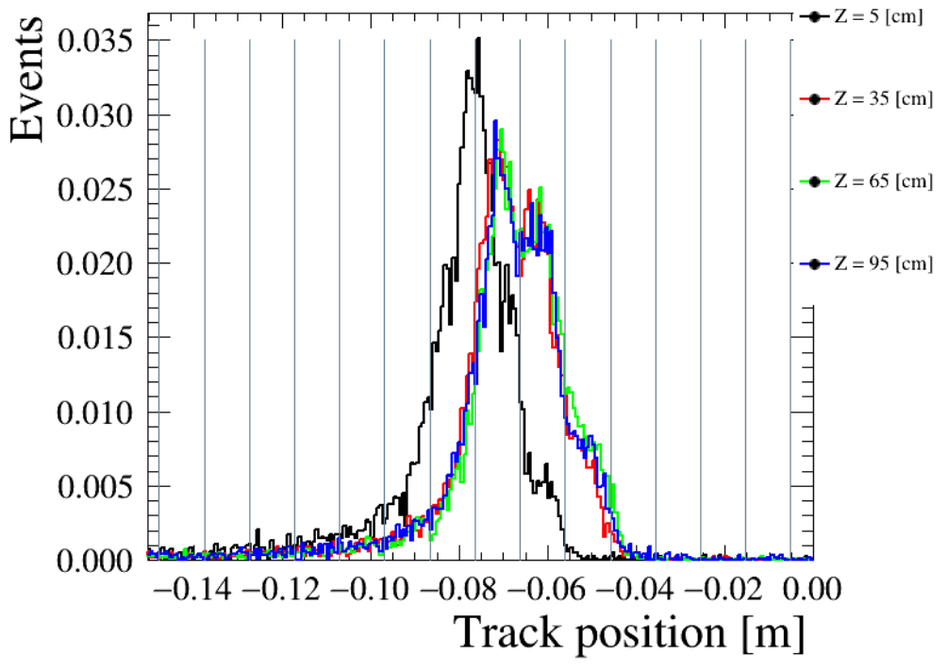
Are you referrimng to the distribution of the reconstructed position of the track?

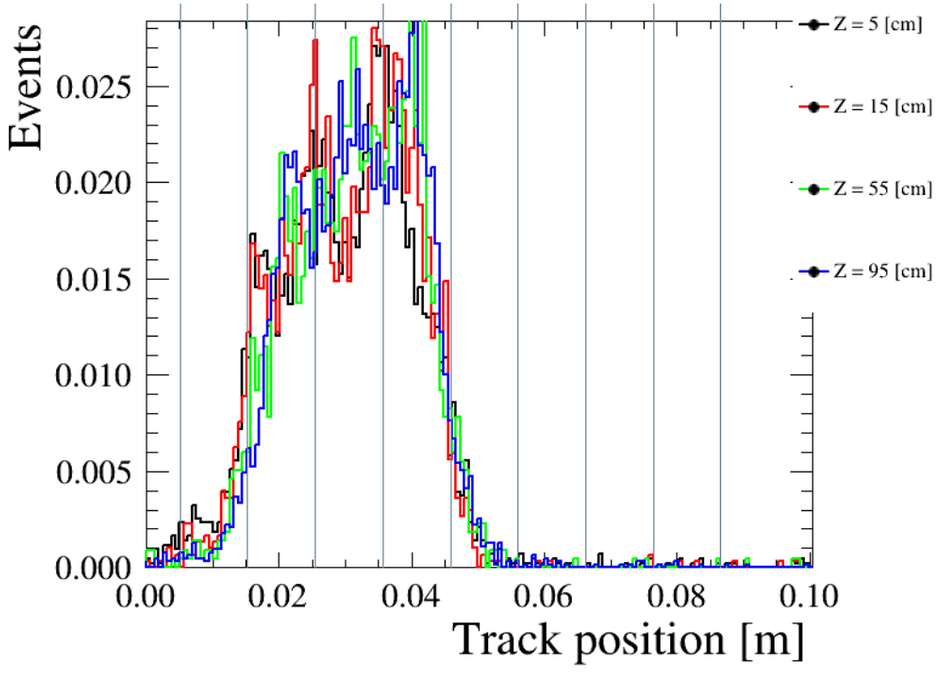
Comments: Reco track position



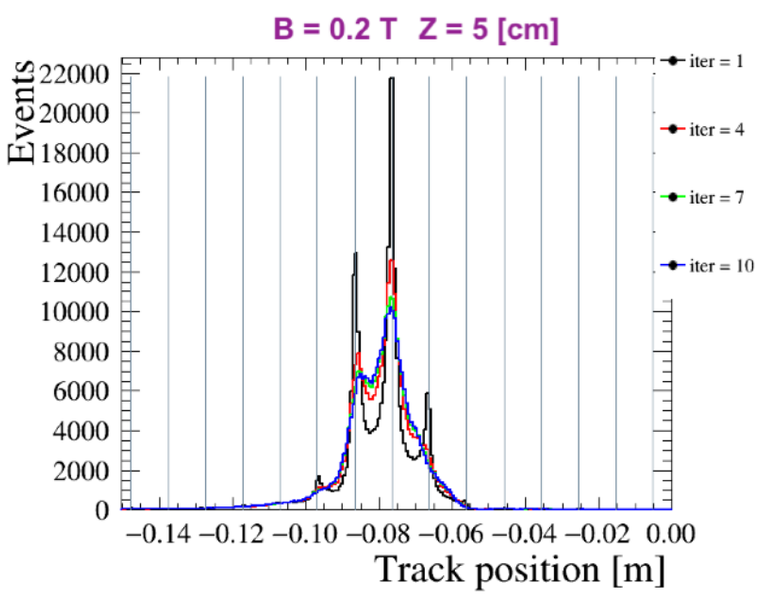


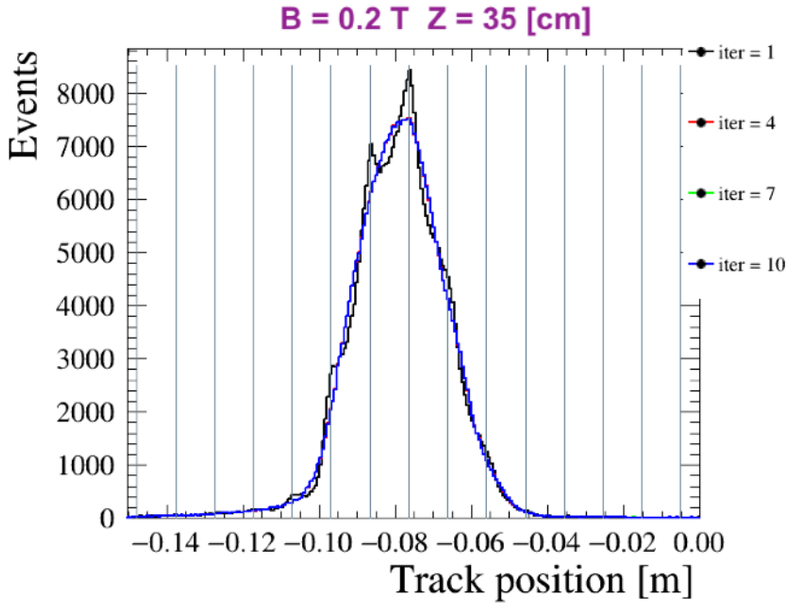
⟱ In the 1st cluster ⟱

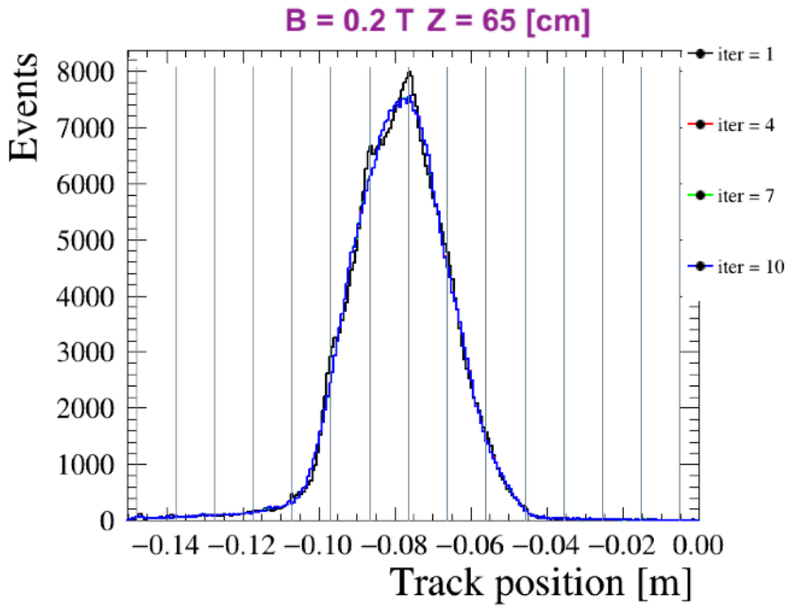


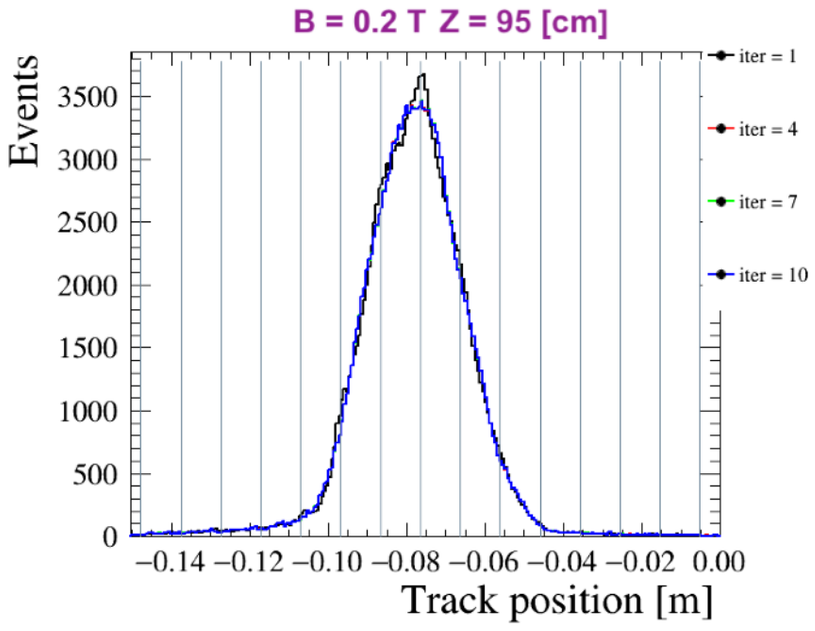


Comments: Reco track position







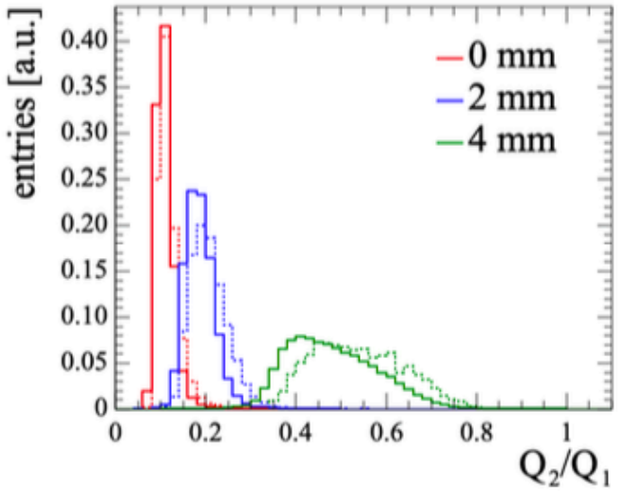


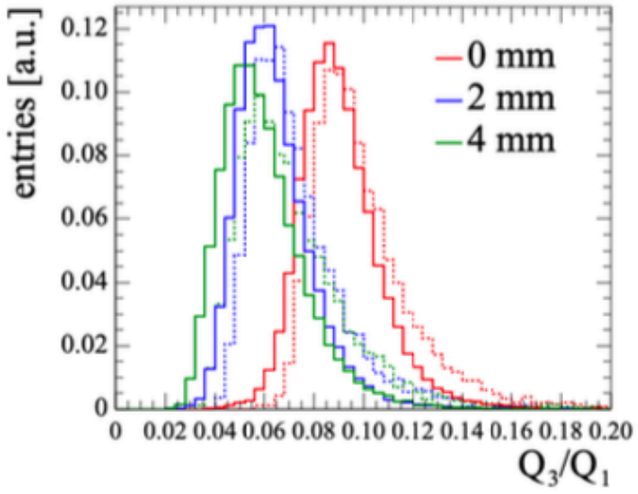
- Z=5 cm different iterations continue to change the position.
- For all other Z the position is the same after few iterations!
- Suggestion: to do even more iterations for Z = 5 cm sample to see if it will converge at some point

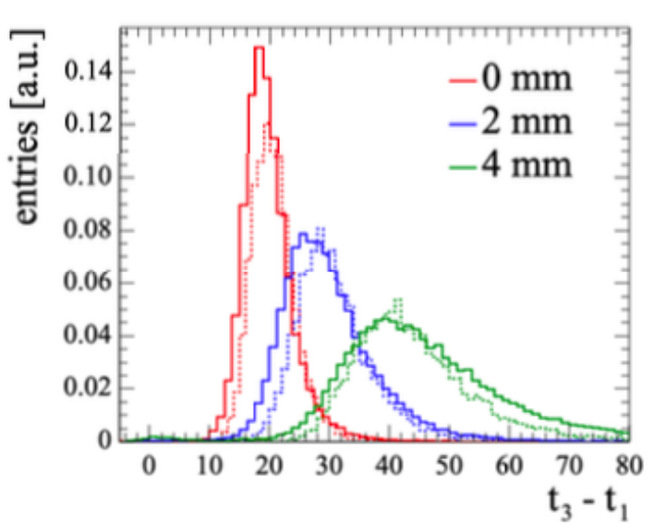
Comments: Data vs MC comparison

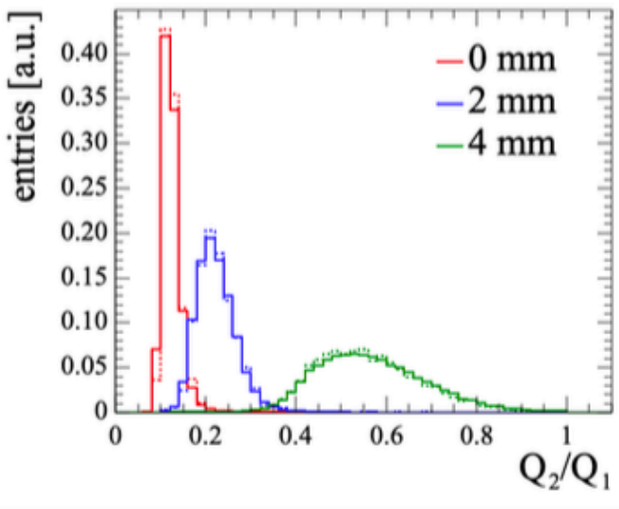
1. Figures 7 and 9 They are far too small given how much they are busy.
   In print they are almost unreadable.
   Zooming on the plot left bottom on figure 9, one sees that the statement of the L350 ”show in general a better agreement between data and simulation as shown in Fig. 9” is arguable (red distributions for instance)

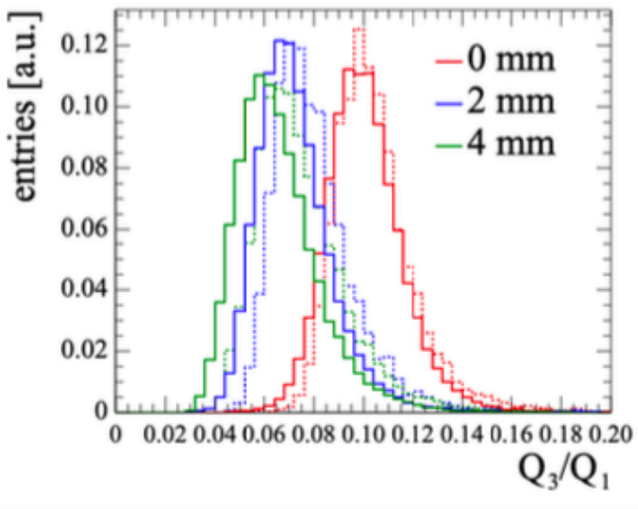
I don’t see an easy way to fix this.. the only option I see is to redo the plots for only 3 samples instead of 6. I still think that Fig. 9 shows better agreement than Fig. 7.

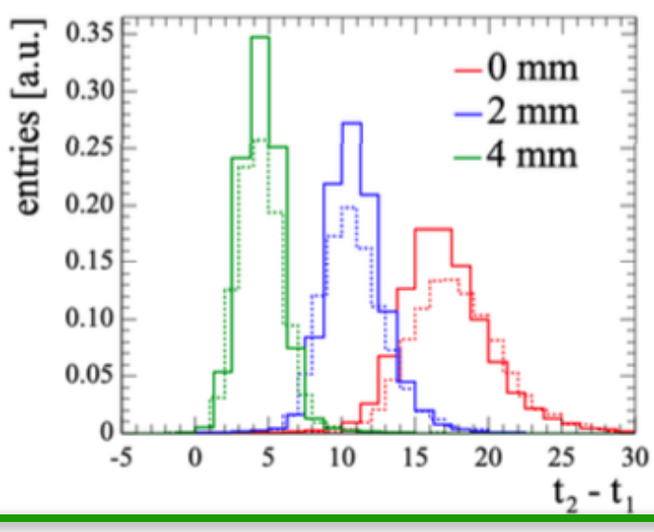








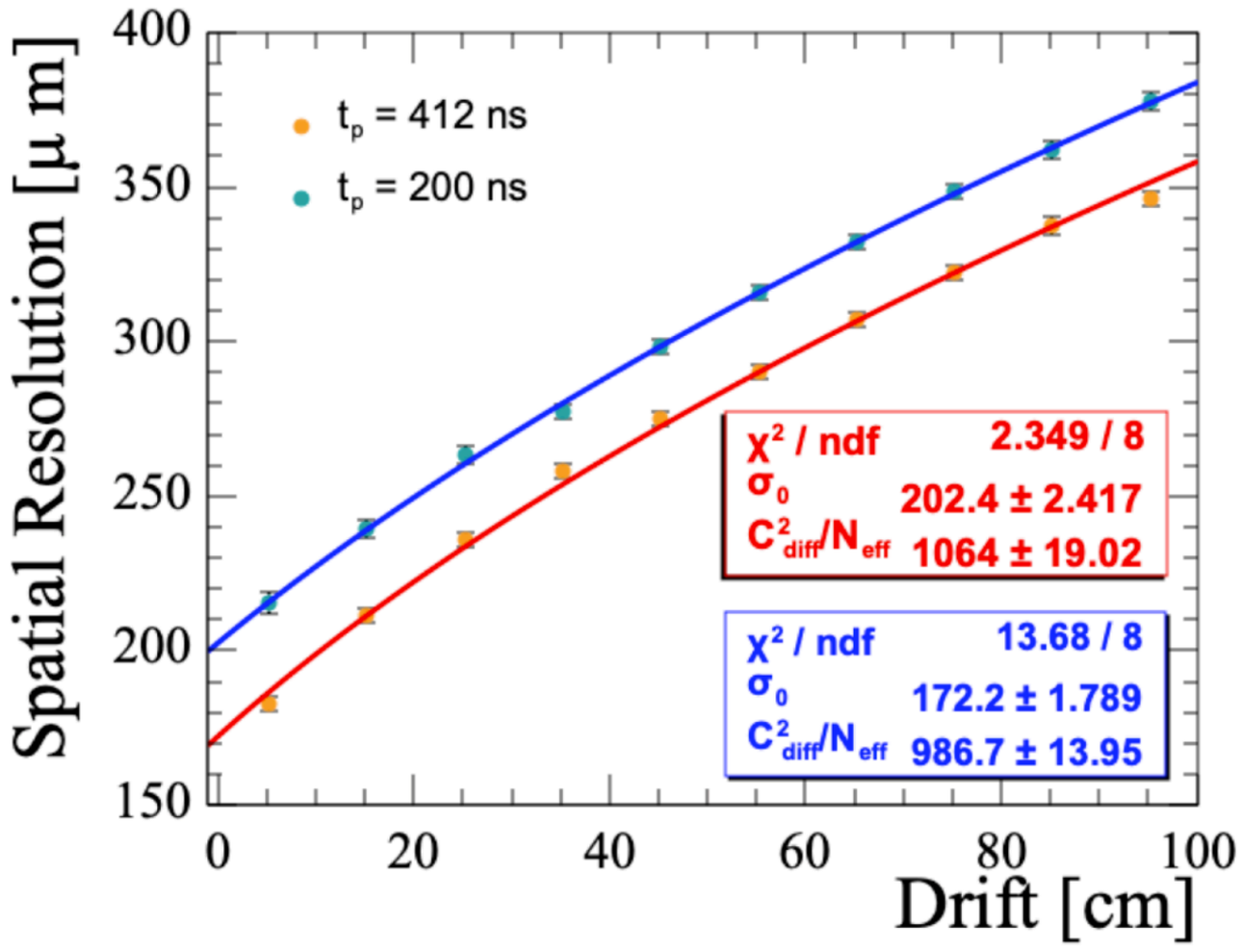




Comments: Spatial resolution (to discuss)

Line 405 : A larger peaking time results in a higher amplitude in the neighbor pads. Please add here and higher pad multiplicity this is what is helping in having a better resolution at higher peaking time

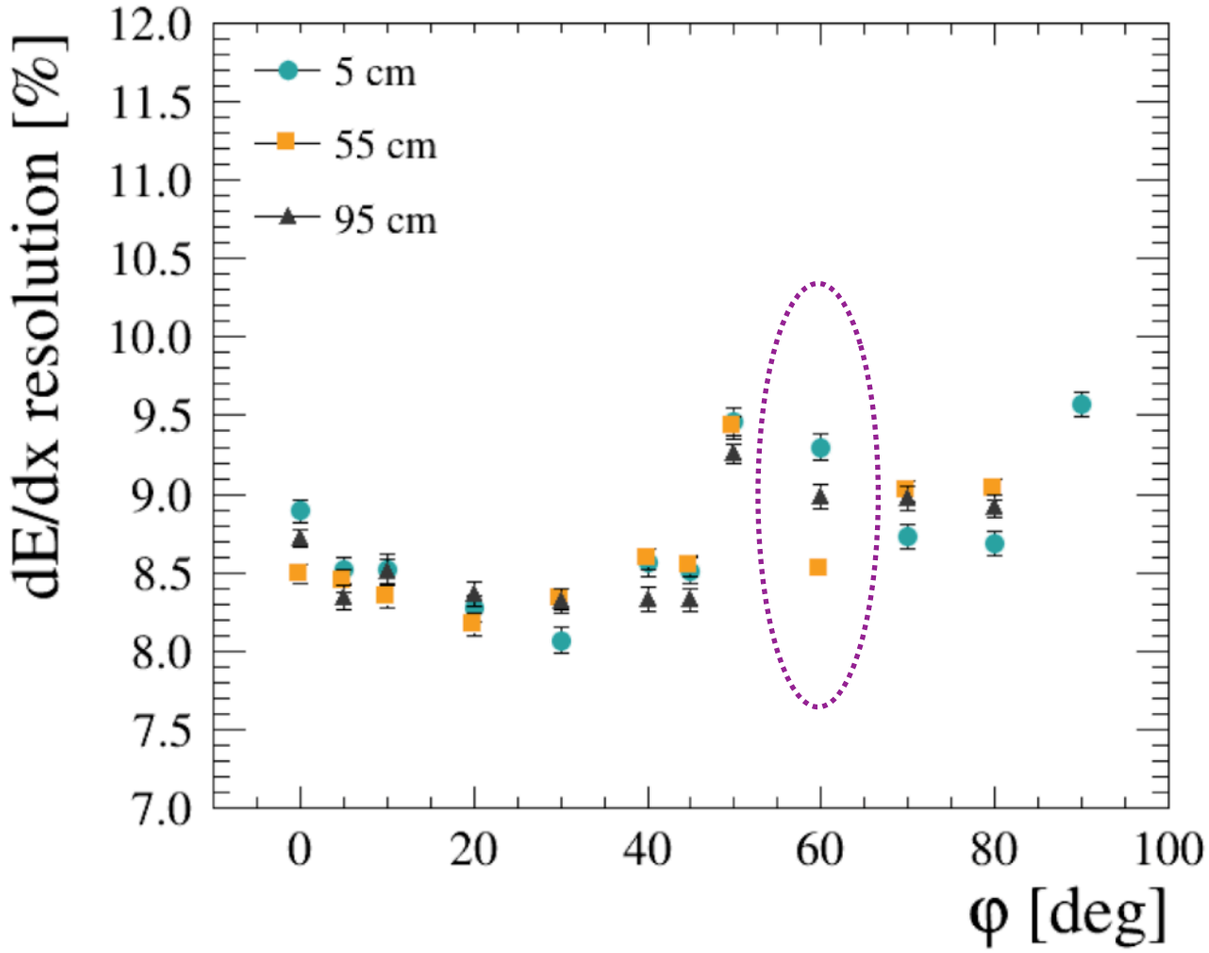
We can add higher pad multiplicity but it is not the only the reason for which we have better resolution



Comments: dE/dx resolution

Line 515: ”Furthermore it is independent of drift distance and of the electronics peaking time. We do see a difference up to 10% for the angle 60 for different drift distance. Do you have an explanation for this point? Otherwise, Please remove the sentence, or mention that 10% difference is observed for that point. The referee will spot it anyway.

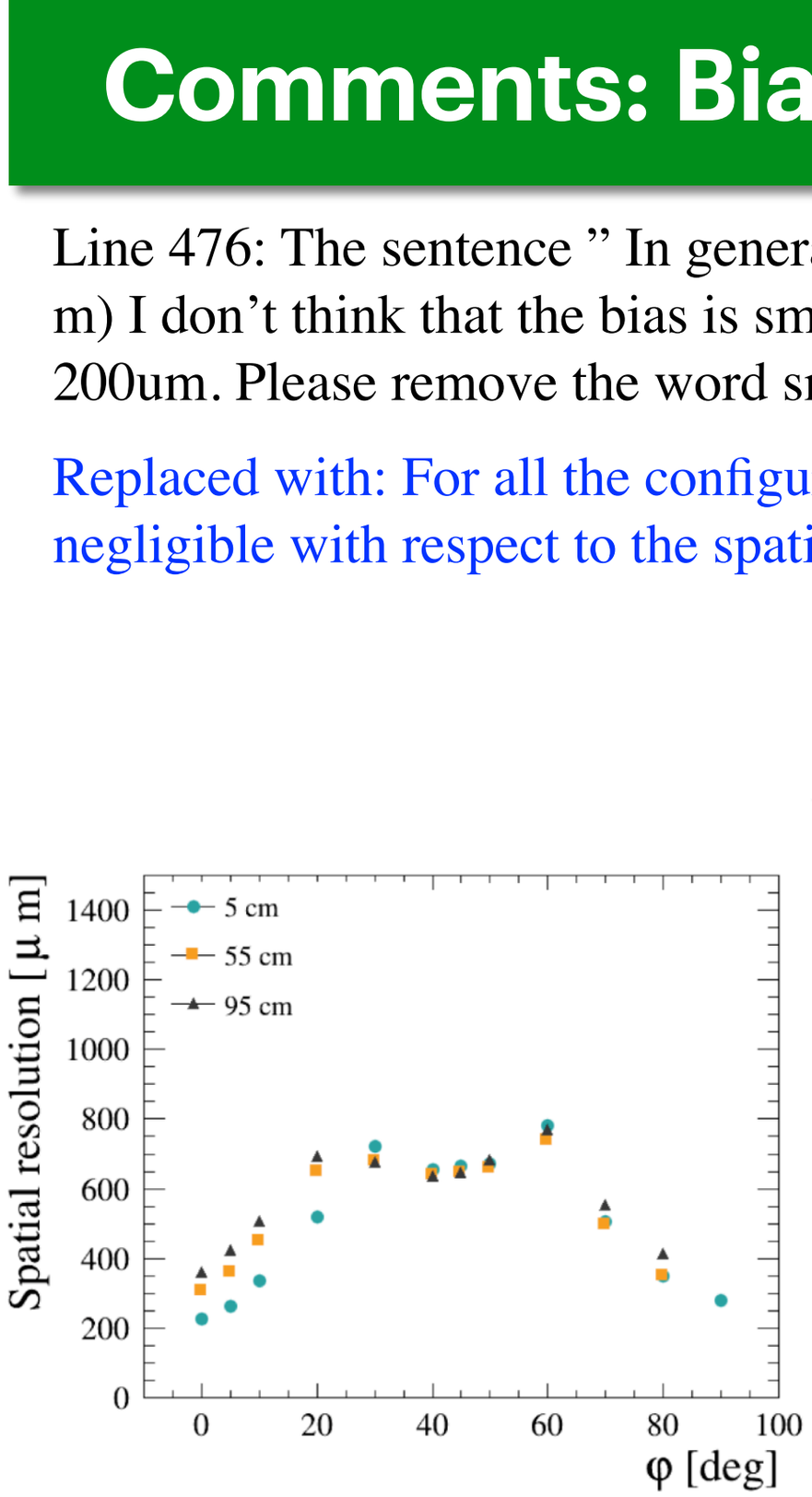
Not sure the difference is significative, but we can add a comments in the caption of the plot

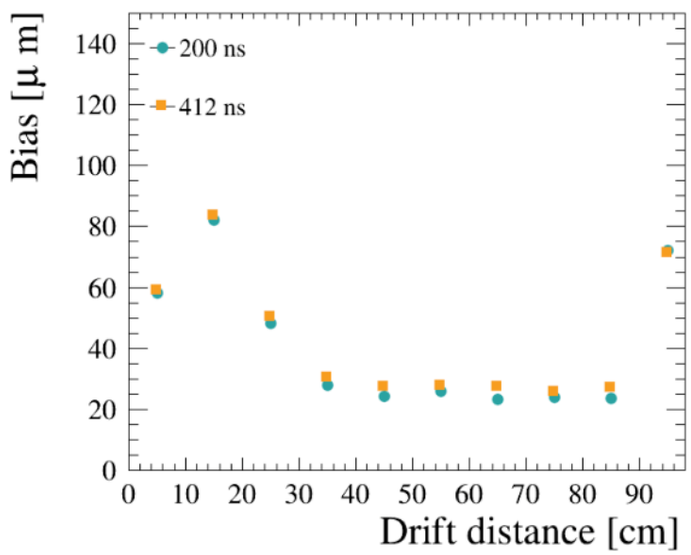


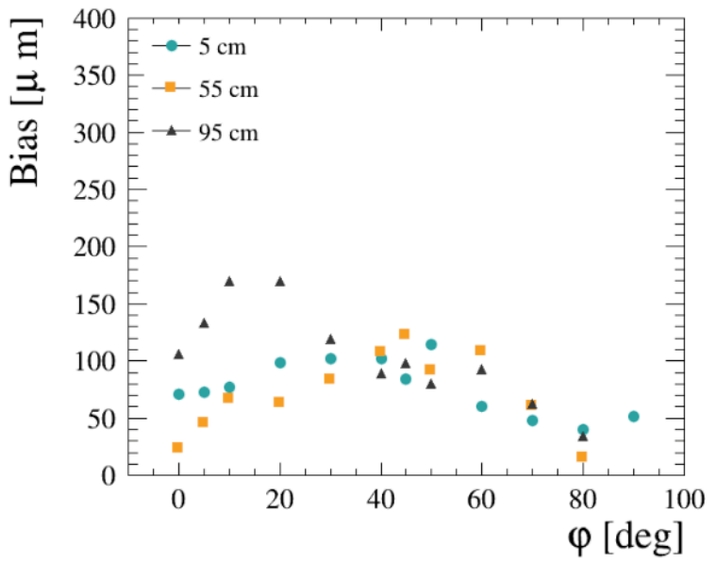
Comments: Bias

Line 476: The sentence ” In general the biases are small for all the angles of track inclination (¡200 m) I don’t think that the bias is small. If we have a spatial resolution of 400-600um and the bias is 200um. Please remove the word small

Replaced with: For all the configurations the biases are smaller than < 200 µm and their size is negligible with respect to the spatial resolution for highly inclined tracks.



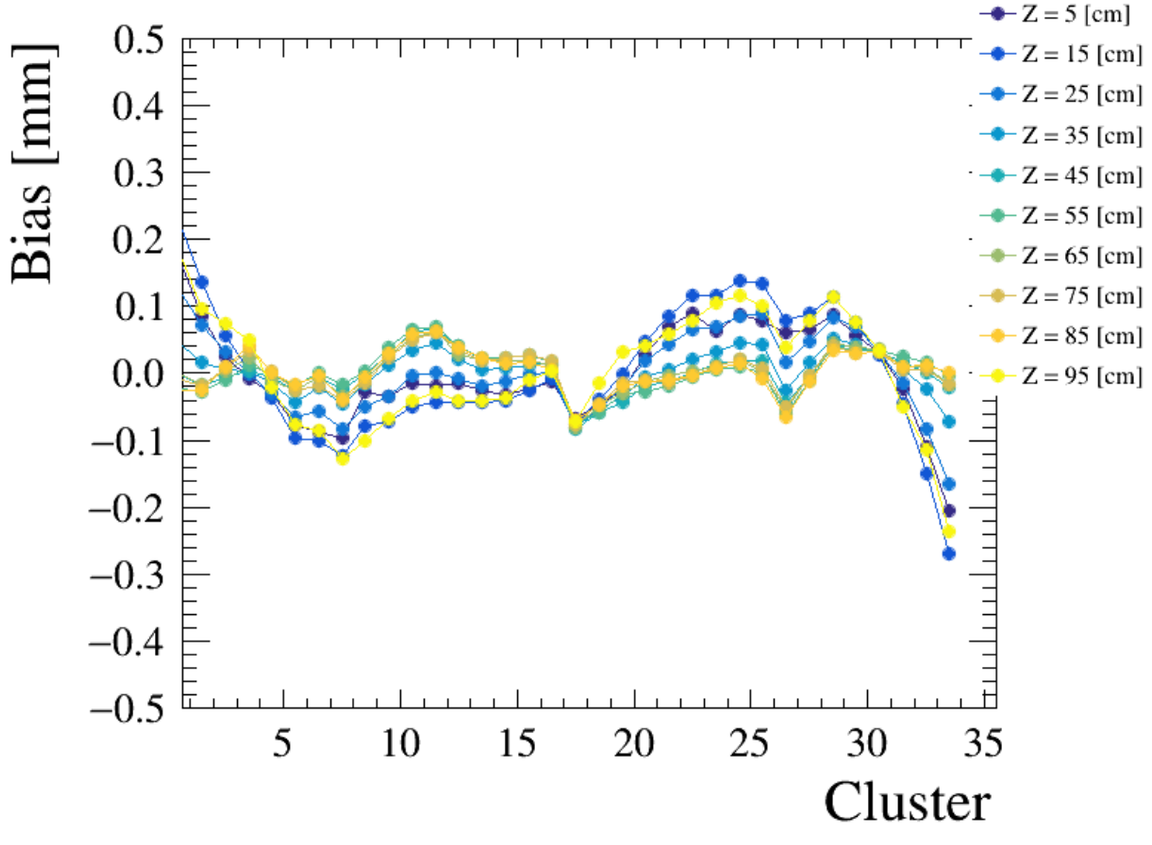


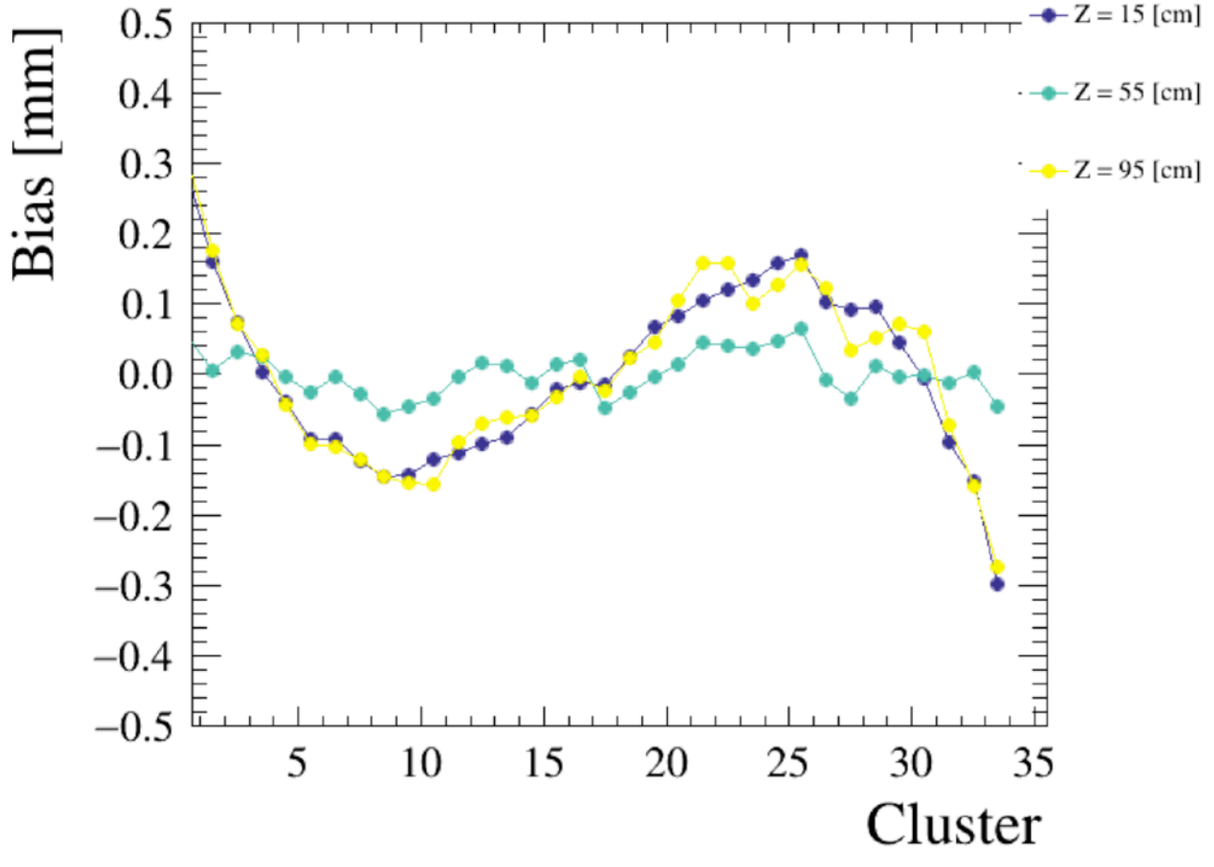


Comments: Bias vs column for diff drift distances

Figure 17: I do not understand why the bias is not show for all the drift distances as it is the case for the spatial resolution on the left as in the plots I have sent in my previous comments (attached again here) to illustrate the shape of the bias In the plot I have suggested you last time, we can show all of them and the plot will not look crowded. Please add the other drift distance in the plot.

Looks overcrowded, hard to see the bias absence in the middle of the chamber

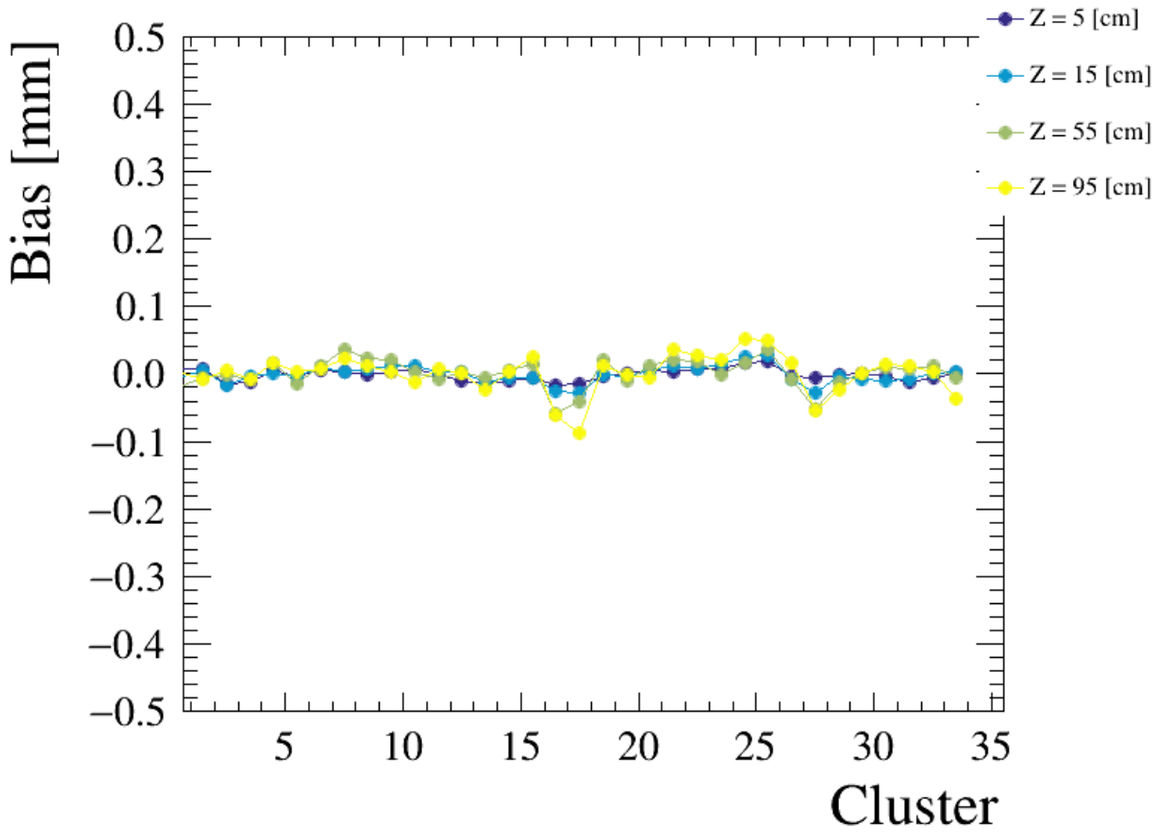




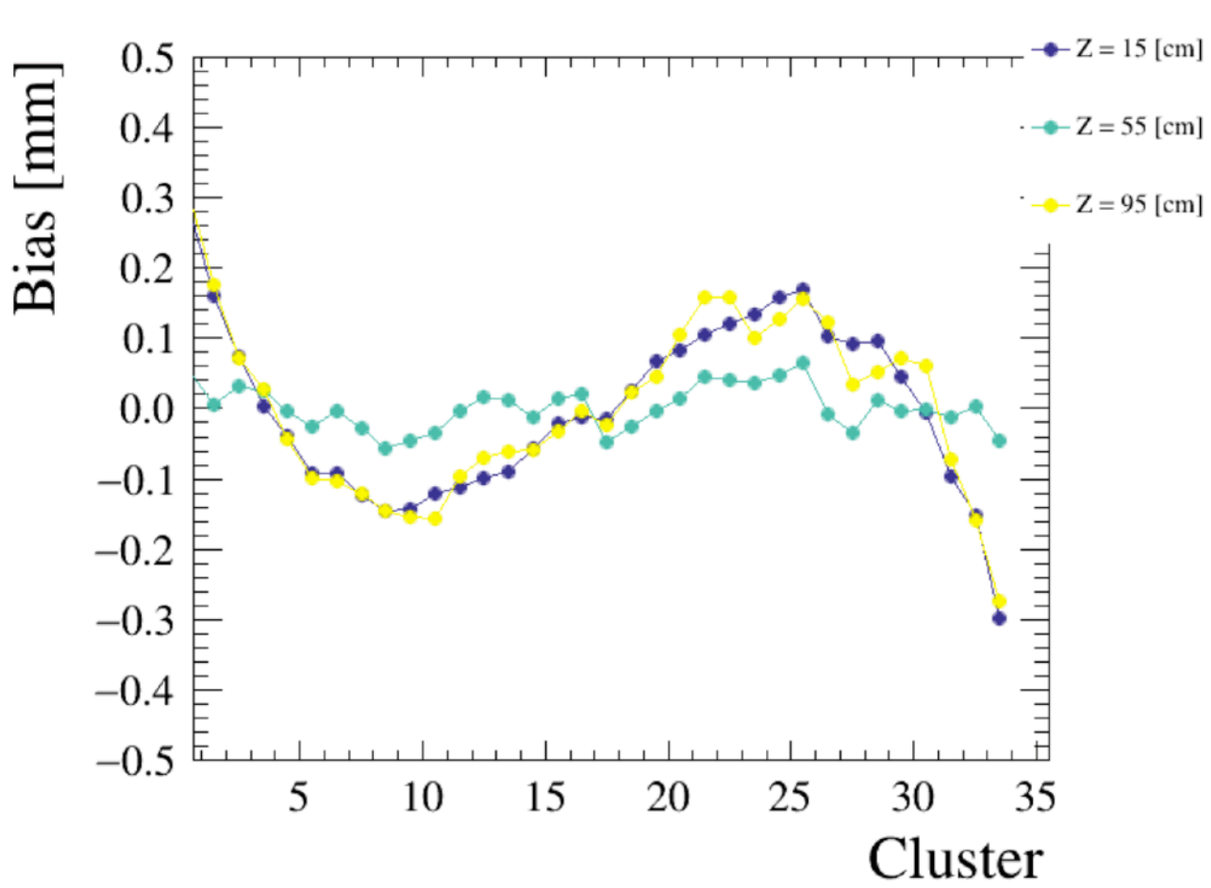
Comments: Bias with/-out mag. field

In this section you explain that the bias is different without B. Please add the bias plot with B=0, we have shown this plot in our analyses meeting to illustrate the argument.‘You can find attached an illustration Later in ExB section you show only the sum of absolute value.

B = 0 T



B = 0.2 T



Other comments: text and plots

L.267-268 Everywhere is said that the size of the cage field is said to be 1 m. Something should be done then to explain why the field is not 267 V/cm = (26.7kV)/ (1 m)

It seems that the real drift distance is 97.25 [cm] which corresponds to 274.55 V/cm

Figures 15 In print 15b is unreadable

Remove the 2D colz histogram

Figure 11 Errors are needed —> Will be redone (to use ROOT FitStat option?)

L.471-472 "The observed dependencies of the biases are not observed in the simulation" Is this true? Is there a simulation with the DESY 21 setup magnetic field? If this is a simulation with the ND280 field this should be said. —> In the GEANT4 simulation we assume a perfect B-field

General comment:We need a little schema showing the different dimensions x, y z of the ERAM —> Will be added

Figure 3: Please change the MC plot and show the waveform without saturation. This is an easy plot to obtain from simulation —> Will be added

Figure 6: I suggest that you normalize the mean distribution multiplicity to 1. Because in each plot we are comparing runs with different stat —> Will be added

Figure 8: Please correct the legend RC =55, 100 ns /mm2 with the correct unit. —> Will be added

Comments: ExB effect (to discuss)

L.563 EXB effect
It is interesting but does miss the point of the bias issue.
What is explained is the inclination of the track which is fine but the explanation of the biases along the track is totally missed since the rotation of a straight line gives a straight line and does not generate any bias. This section in its present content state should be somewhat shorten but above all dissociated from the bias issue. Yet it looks that the authors of this analysis have the tools to address the bias issue properly. Pierre Granger has completely addressed and solved the issue already a while ago. He has shown, simulating the charge drift in the field of the the DESY 21 setup magnet, that not only the image track on the anode gets inclined but also, much more interestingly, that this image gets a sinusoidal shape. If this image is fitted by a straight line or even a parabola, the biases will have a sinusoidal shape as in data (think to the residuals of a sinusoidal curve over a period fitted by a straight line). Therefore I guess that the authors should complete their analysis looking at the biases fitting their image track with a straight line. I suggest that this supplementary material should be presented in meeting first, before to go in any paper.

To Be Done: Let's discuss tomorrow about this. We can have a sentence on the fact that the shape of the track is sinusoidal, but, in my opinion, the studies done by Pierre do not explain why the bias depend on the drift distance, being smaller for tracks in the center of the chamber.

The plan is to have a look at the bias vs clusters in the simulation for different drift distances in the presence of ExB effect

Comments: ExB effect (figures)

L.Figure 31 and text L.605-610 It is no clear what this figure adds to figures 29 and 30, which covers already the dependence to field and Drift Distances. Contrary to these figures, the figure 31 does not even compare to the simu- lation discussed in this section.

Fig. 28 shows the B-field dependence for one drift distance, Fig. 30 shows the drift distance dependence for B=0.2 while Fig. 31 shows B-field and drift distance dependencies. But I do not have a strong opinion if this figure is needed or not.

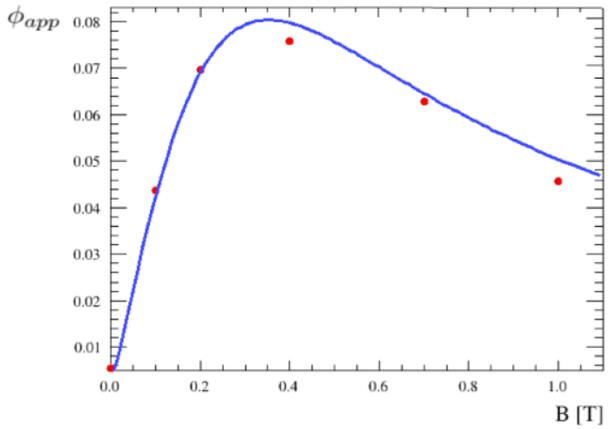


Fig. 28

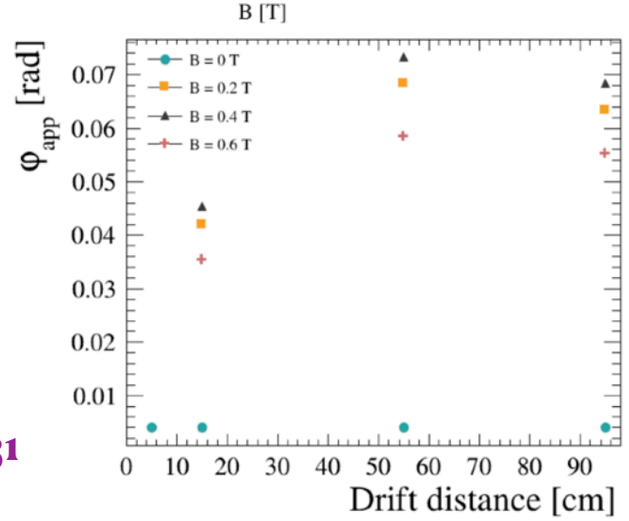
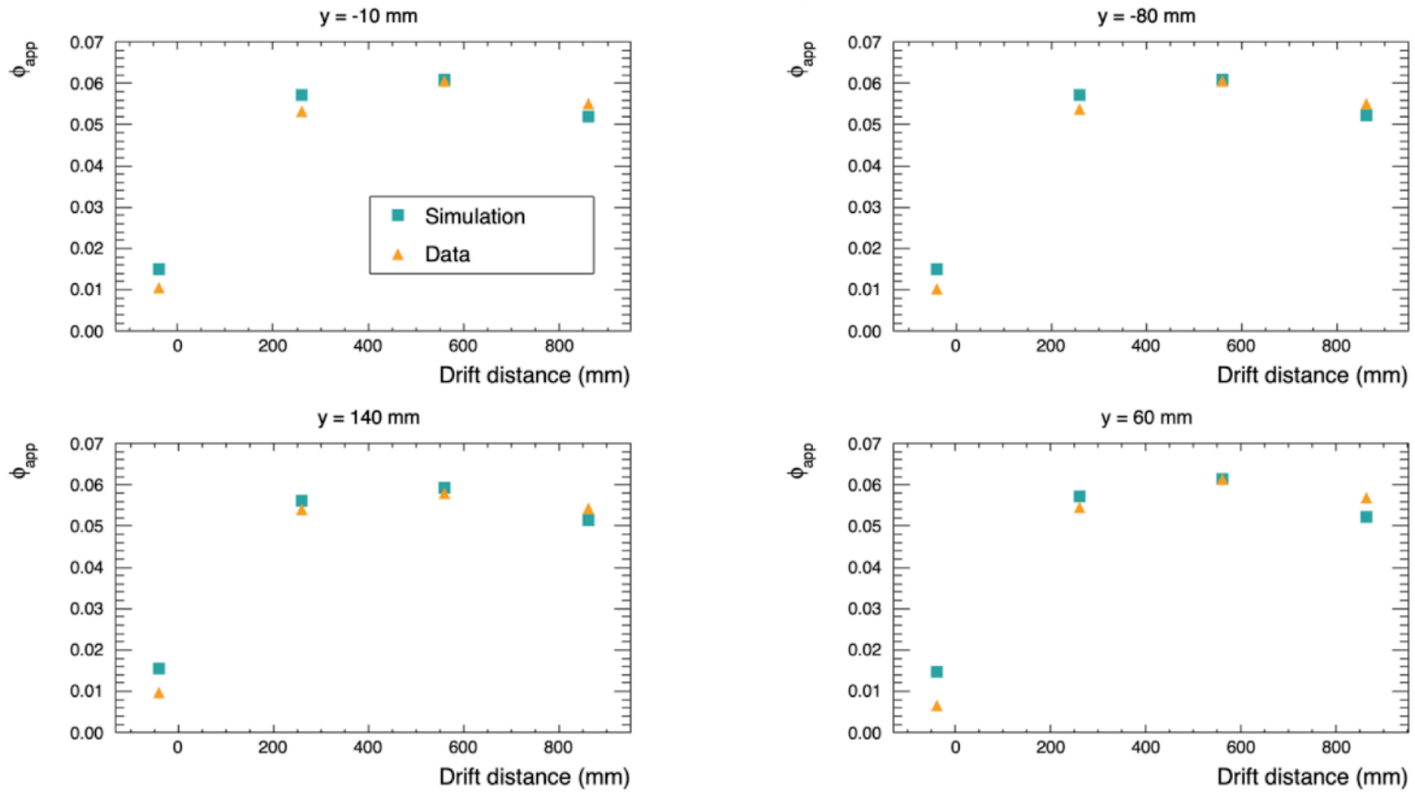


Fig. 31

Fig. 30



Comments: ExB effect (text & equations)

From L.575 to formula (10) Give a reference for the maths and keep only the final formula.

Unfortunately, we are not aware of any references. Maybe you can propose one? There is a proposal to put derivation in the appendix.

Figure 26: can you remove the red box from the left plot? or replace it with another plot.

To Be Done: Not sure who did that plot? Maybe we can remove it? —> leave only accumulation plot

Figure 28: do the red points contain an error? They are not visible. Please add in the caption about the error. If they are statistical and small and why we do not have nay systematic error?

To Be Done: The errors should be small given that we have a very large statistics for each sample but we should check with Lorenzo —> the errors are very small

Similar comment for figure 30 and 31, where are the errors?

To Be Done: For simulation there are no errors, for data —> check

Please add in caption of figure 30 that the simulation is performed using Garfield, that the reader got not confused with the other simulation results using GEANT4.

Added: The simulation uses Garfield++ as described in the text. —> will be added

All the rest of the comments will be taken into account and the plots will be corrected!

Thanks a lot for all your comments and contribution!