Introduction: line 33, does not seem enough to clarify how this paper relates to the 2 existing ones. Given that already 2 articles were published in the past, this is certainly something the reviewers will complain about if it is not corrected. I'd suggest making a clear sentence / small paragraph summarizing what was learned in the previous tests and how the results in this article add up to it.

Some text to explain difference with respect to previous Test Beams is given at L. 49 and some more has been added

- line 42, mention explicitly this configuration.

Done

- line 68, I would mention explicitly the reduction in the box thickness (e.g. 10cm to 4cm, check exact numbers please). Reduction from 12cm to 4cm of the distance between gas volume and the outer wall

Done

L.107 "and the 3D position of the track is reconstructed." Propose to remove since it misleadingly oversimplifies what is going on here

replaced with "The resulting pattern of illuminated pads corresponds to the trajectory of the track."

L.110 "avalanche is smaller compared to the pad size for small drift distances" This is true for both techs and therefore can not be a difference between them

Rewritten as: The main difference between the bulk-Micromegas technology used for the existing ND280 TPCs and an ERAM is that, in the case of bulk-Micromegas and for short drift distances the position reconstruction is limited by the pad size that is larger compared to the avalanche size. In the ERAM, instead, the anode is covered by a foil of insulating material with a thin resistive layer on top, spreading the charge over several pads. This allows a better reconstruction of the track position.

L.142 "we will present, for the first time, comparisons" Propose to change this in -¿we will present the first comparisons

changed

-line 158, the units of the transverse diffusion are mm/ cm. In line 499 you use m/cm please, use a common convention.

this should have been unified now

-line 162, is there some easy justification for this choice? Can some small explanation be added or reference added?

This is due to the fact that the size of the avalanche for each primary electron is exponential. For many electrons (typically we have n 100 per pad) this results in a Gaussian distribution with mean n*G. I couldn't find many references but this should be the Yule-Furry distribution explained for example in A.2 here: https://inspirehep.net/files/4c382455a2dca38efab985b542253071link

Formula (1) Make the distribution properly normalized

it is fine now

L.386-399 A word should be said about the vertical tracks which give resolution insensitive to Drift Distance. This is a problem since simulation (fig

25) shows that this should not be the case Nothing is these lines can help to understand this.

The plot for 0, 45 and 90 deg Z scan is added to demonstrate that for 90 deg tracks, in fact, depend on drift distance while the tracks reconstructed with diagonal fit (45 deg) are not. We tried to understand what's going on with the points at 70 and 80 degrees but we couldn't find a reason for which the Z dependence is not observed at those angles

** Formula (1) is strange. What is tpeaking? it should be (t). E(t) should run over t. What is the definition of tau and what is it numerical value? you mean tau(peaking) is tau the value of the peaking time?

Explanation is added

L.191 The two WFs on this fig 3 are saturating from below This is unfortunate and misleading about the ERAM WFs

that's true but I don't think we have access to the original WF from Sergey and since in this paper we only use Qmax and Tmax it doesn't affect any result.

L.194 On fig. 4, the resolution is patently linear in Z unlike data case This can not pass unnoticed in the text

The plot of the Z dependence has been moved to a later section because, at this point of the paper, we didn't discuss yet the Z dependence of the SR. Here we replaced with another plot showing that there is no differences between optimized and not optimized simulation. But it's true that dependence looks different in the simulation and we added a line to mention this effect.

-line 238, 'mm/m' -¿ Should be mm/µs. (moreover, isn't this usually reported in cm/µs ?)

rewritten as Most of the data were taken with the cathode High Voltage set at 26.7 kV corresponding to an electric field in the TPC of 275 V/cm.

L.276-278 "with mean multiplicity smaller than 3.4 for horizontal tracks and 2.2 for diagonal tracks with 200 ns peaking time." There is no way to get from this sentence what happens for 421 ns peaking time The sentence should be rephrased in a more direct manner

Rewritten as: For the analyses presented in this paper we select tracks with mean multiplicity smaller than 3.4 for horizontal tracks and 2.2 for diagonal tracks with 200 ns peaking time. For 412 ns peaking time only horizontal tracks were taken and, for this sample, we required a mean multiplicity smaller than 4.3.

-line 278, what has been the criteria to choose these very specific values? (3.4 and 2.2). –¿ We wanted to remove events with high multiplicity to remove double tracks or big d-rays but the cuts were not really optimized. WE just wanted to keep the bulk of the distribution

Fig.8 and 9 1/ in the captions add the data period (DESY 2019 or 2021)

done

2/ these plots are terribly messy

we tried to rebin them and make them easier to read

3/ with effort one can get that the Data and MC distributions widths do not agree. It can not be that no comment is done on this. To comment on the MPV is not enough

We added a comment on the fact that we hope to improve the simulation by using realistic maps of RC from the X-ray scan. But to us the distributions agree pretty well also for the widths

** Figure 11: The PRF for inclined track seems to have a feature near 0, a kind of dip, do you understand that? I suggest that you show another example. Also the fitted function is in red in the right plot and with another color on the left. Please use the same color.

The dip is probably a feature of the PRF fit itself and of the freedom it has at zero. This is only observed for inclined tracks and it doesn't affect the results.

Fig.12 Fit parameters can not been given without errors and in inconvenient units (m)

We redid the plot in mm

Formula(7) A Z is missing

added

Figure 13 and L.357 "perfect agreement" can not be claimed while it is obvious from the plot and the Xi2 that the errors of the points on the figure are too large to make sense As far as the reader can understand, the errors should come from a fit such as the one shown on fig. 12 b and they can not be as large as those shown on figure 13

the error bar in the original version of the paper represented the RMS of the spatial resolution for the different columns. The idea was to show that there are no large differences between columns but we replaced them with the error on the mean.

2/ the value of the C_diff used in formula (7) for the fits should be given

$C_diff^2/Neff$ is the parameter that is fitted and the value is shown in the figure.

L408-410 "The bias per sample is defined as the arithmetic mean of the biases of all clusters. Fig. 17a shows the ratio between the bias and the spatial resolution as function of the drift distance for horizontal tracks."

2/ it should be said why the ratio to spatial resolution is an interesting quantity since most certainly, the reader expects the genuine bias in micrometers

We replaced bias/SR with bias for all the plots

-line 412, any explanation for this?

We do not have a clear explanation but it could be due to ExB effects and the plot is moved to the newly ExB section.

L.427-432 "At 0 T biases depend on the drift distance linearly. The linear dependence is expected, since for larger drift distances the contribution to charge spread of longitudinal diffusion is bigger which leads to an increase of PRF function width. However, under conditions when the magnetic field is on, the linearity is broken and distortions are observed (Figure 19) for short and long drift distances." 1/ these considerations are about the resolution not the bias

2/ "longitudinal"-¿transverse

3/ the dependency is not linear since the formula (7) holds. Better to say "increasing with Drift Distance"

4/ Anyway the effect of the field is to give a "sinusoidal" shape to the tracks due to the EXB term as it has been shown explicitly by Pierre Granger and this explain bias when field is on

this part has been moved at the end of the ExB section and rephrased.

Figure 14, the points do not seem to cover the full number of pad rows, neither the spacing between points in x is constant. Is there some reason for this, e.g. some bad runs? I'd try to clarify this.

Those are all the good data we took in the X and Y scan. There are some distortions at the position of the Y dead pad as well as at the edges of ERAM where short tracks are reconstructed. Those points were removed.

-Figure 18, some legend is missing to understand what are the red and black dots. Also the size of the text numbers here is particularly small.

done

End of section 9 It is not mentioned that we do know that the reconstructed beam profile is biased. I don't understand this comment at the end of section 9

L.499 "286" Earlier on it is said that it was 290 Making the numbers consistent will help the reader.

Done

Fig26 a The difference RC 55/100 should decrease with Z since the resolution is more and more dominated by the transverse diffusion term as shown by formula (7). This is not the case and could be related to the fact that the MC resolution is almost linear in Z in contrast with data.

OK but the message of this plot is just to give an hint of how much the resolution depends on RC.

** Figure 3 (b): why the undershoot is truncated in the MC. It is an electronic limitation in data but it should not be the case in MC? I have the impression from the plot that the undershoot stops sharply and is continuing as flat. Can you replace it with a plot where the undershoot is better described.

Same comment as before

** Please remove section 4 as I have written you in a previous mail

** Line 131-134: "The response linearity of the FEC has been measured with a dedicated campaign and showed a uniform response of all the channels with typical deviations between neighboring pads not larger than 2%" Please remove the sentence about the response linearity work was not yet completed for all the cards and we have seen case with up to 7% non-linearity. The calibration work deserves a long paragraph in the new paper we are proposing.

The linearity for the cards used in DESY was tested and, as was shown by Ulysse, the mean value between adjacent pad is 0.7%. So I think the sentence is correct and we can defer additional studies to the calibration paper.

** Line 158 : a transversal diffusion of (trans) = 0.29 mm/sqrt (cm) while in line 499 another number is given which is 286. Please use one uniform number over the paper.

Changed

**Line 176 : "The pad is divided into several smaller regions e.g. 3x3 or 5x5 You need to add the unit, you mean 3x3 mm x mm? or um?

We mean that the pad is divided in a 3x3 grid. If the pad was 1.0x1.0 cm2 it would be region of 0.33x0.33 cm2.

** Line 184 : "the pad in a grid of 10×10 similar comment as for line 176. Add the unit

same as before

** Line 241-246 : Please remove information about RC non-homogeneity and just describe that a scan in x-y position where performed. By the way, we miss in the paper a plot or a short description of the axis. what is the beam axis?...

we remove the reference to the scan

** Line 303: I don't understand why the ratio Q2/Q1 is fitted by a Landau.?

it could have been fitted with gaus but landau was working a bit better

**Figure 8: You don't comment on the shift in the shape. I think we need to explain the possible source of shift

we added to the text some comments on the fact that the shapes do not agree perfectly and some optimization could be done

** Figure 9: I think that you have removed the negative side of the curve (magenta) because at the pad border, the neighbor can arrive before the leading and you can have negative DeltaT

the plot has been extended to negative values

** Line 310-311: please remove the sentence

Done

** Line 300 : "For this study we use tracks close to ERAM plane to minimize possible effects induced by long drift."

Here in this comparison, I understand the argument of line 300, but at the same time in real life we have 1m of drift and we know that at the end the diffusion and the charge spreading are completely correlated and are responsible of the WF we observe. Can you show the same comparison for another value of the drift , will the RC value obtained be compatible with the one shown here?

Please find different comparison at different drift distance in Fig. 1, 2, 3. In general the agreement is better at larger drift distances. Notice that by doing a comparison with MC with RC = 55 we get plots in Fig. 4. Some plots have been added to the paper

Figure 13, It seems that the error bars are overestimated. This is quantitatively confirmed by the chi2/ndof. How is the error calculated?

The error was computed as the RMS of the SR over the columns to show the spread of the SR. We replaced it with the error on the mean and now chi2/ndof is reasonable.

line 310, Although the agreement is overall good, there are some MC-data discrepancies that can clearly be spotted in Fig10. I think a fair complaint from the reviewers might be: 'Do you have any explanation on the origin of the MC-data discrepancies' and 'how do you establish what level of disagreement is reasonable'? If you have any insights on this I'd add some comments to this line.

A possible way would be to optimise the value of RC used in the simulation based on the X-ray scans. We added a sentence in that sense and a sentence to

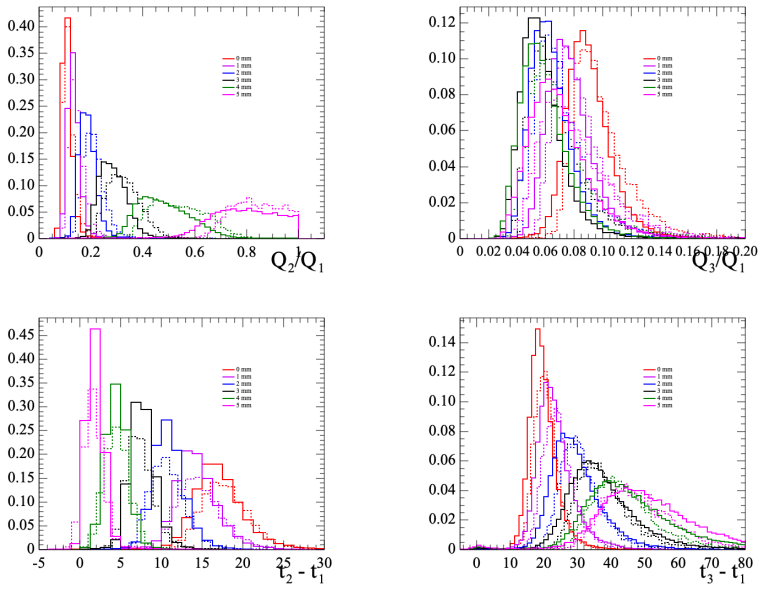

Figure 1: Agreement data/mc for Z = 5 cm

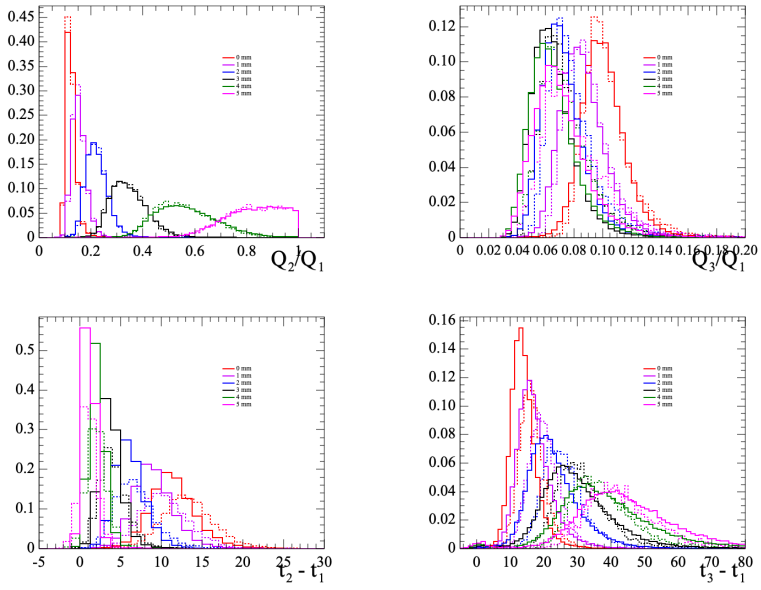

Figure 2: Agreement data/mc for Z = 50 cm

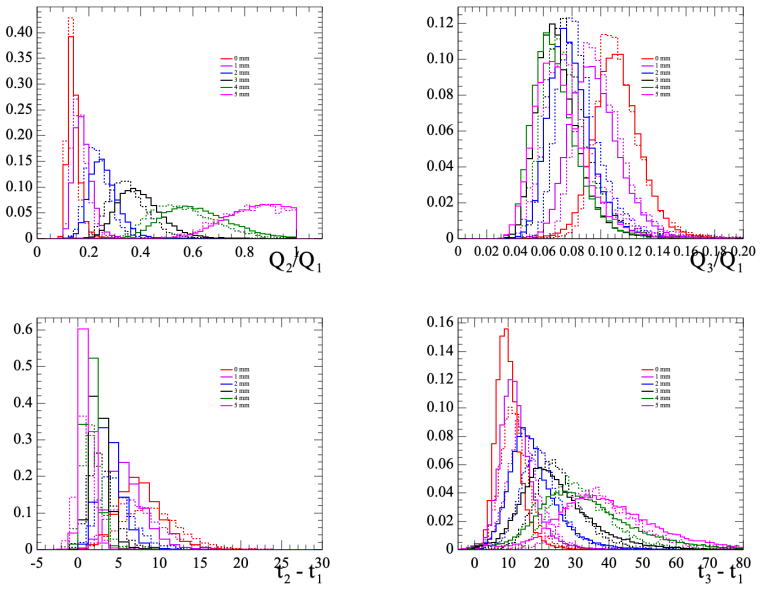

Figure 3: Agreement data/mc for Z = 90 cm

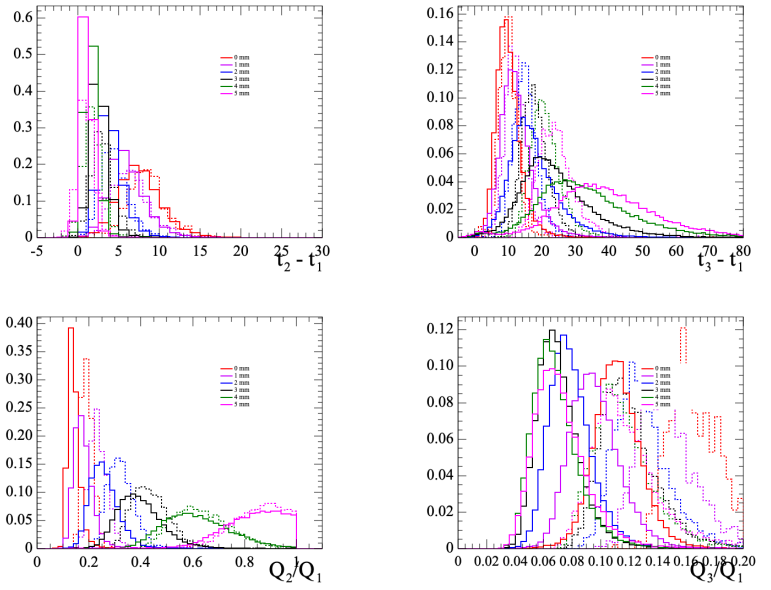

Figure 4: Agreement data/mc for Z = 90 cm but MC with RC = 50

explain that these differences do not affect performances.

** Line 359-361 : "A larger peaking time results in a higher amplitude in the neighbour pads. Thus we have more robust information for the PRF fit and the track position reconstruction is more precise.

I am not sure I understand this sentence and I don't agree with it. With large peaking time we have small amplitude and large WF. But the argument why the spatial resolution is better is due in my point of view to the fact that with large peaking we have higher cluster multiplicity which lead to more precise position determination. (It makes sense)

I think the two arguments are the same. If we have larger multiplicity we should have also larger charge in the neighboring pads, right? I remember Samira saying that only the 3 pads with largest charge in the cluster are used in the analysis so the larger multiplicity shouldn't help to improve spatial resolution but the larger cjharge in the pad does

* Paragph 8 : I think the bias should be shown. You are showing only the mean of absolute value of the bias. I don't think that we need to hide this. We show it and we say that we are still investigating. I have similar comment on showing bias/resolution and commenting that is around 10%. I don't think that the quantity bias/resolution bring any information about the detector performance.

we added a plot with the bias as a function of the column for different drift distances

** Line 429 : " longitudinal diffusion You mean transverse diffusion? otherwise I don't understand the argument.

yes but this section has been moved

L.466-467 "does not suffer from the pad signal double-counting effect" There is no chance that any reader can understand what this is supposed to mean. Elaborations are needed.

rewritten as: The deposited energy per cluster is calculated by taking the maximum of the sum of the waveforms of the pads constituting the cluster, that, as shown in [**?**], results in the best deposited energy resolution.

-lin 490, no interpretation is made of the results in Fig 22. The perpendicular data has a more stable trend. Has it anything to do with the comment in line 242?

This was due to some data at large Y position where the track was exiting the ERAM module resulting in a worst dE/dx resolution. We removed those samples from the plot. The intepretation is added.

** Line 499 : Please see my comment on Line 158. Those numbers are taken from Magboltz simulation and we have found many different numbers depending on the reference. Those number are also depend on E*T/P and B of course. I have asked David to produce a new plot that should be used as a reference. So I would suggest that you add a reference to where you have taken this value. The most recent plots can be found here https://publications.rwth-aachen.de/record/766357/files/766357.pdf

The value we used is the default one used in ND280 simulations of the vertical TPCs. We have rewritten asL: The dependence over the drift distance was found

to be slightly different using the default value used in the ND280 simulations of the vertical TPC that has a transverse diffusion $\sigma_{trans} = 286\ \mu m/\sqrt{cm}$.

** Line 501, I don't agree that the transverse diffusion is a free parameter. The transverse diffusion depends on E*T/P. You can say that we have taken T/P conditions into account. David started producing the plot and we can clarify this point before the paper submission.

We agree it is not a free parameter but what we say is that we changed the value of Transverse diffusion in the simulation to better reproduce the data.

** Figure 23: why don't you fit with formula number 7? I think it is important. We need also to know what is the resolution at the mesh and the number of effective electron Neff By eye, I have the impression that the simulation is more linear with strange behavior at low drift. and this can be confirmed by the fit

Yes this is true. Please find in Fig. 5 the fit with pol1 and Eq.1 for data and simulation. We added a sentence saying that we observe this effect but for now it is not understood.

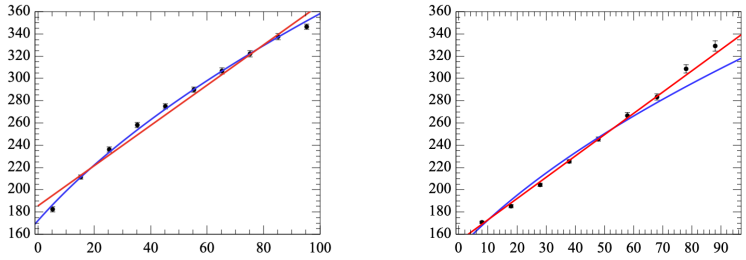

Figure 5: Fit with pol1 and with sqrt(z) for data(left) and MC (right)

Fig 23 A word should be said on the fact that the MC resolution is almost linear in Z contrary to the data

See previous reply and fit in Fig. 5

The conclusions do not even mention the simulations, which is one of the major advances in this article.

Added

-line 527, I'd add 'for all drift distances' as this is the novelty in this study. -In general I think the conclusions need to be extended. It is very important to summarize all results and to highlight the main differences between this article and those already published. Simulations must be mentioned and their importance remarked.

Totally agree! We rewrote the conclusion to stress the importance of the simulation

1. Other comments

Vocabulary “spreading” “sharing” (resistive-diffusion effects) not defined, one word used for different physics phenomena or two words used for the same phenomena... An overview is missing (the “spreading” is a function of time AND space, sometimes it is not precised that we use the charge and time of the maximum of the waveform...) An short overview and reminder is really necessary, even if it is summarized (you can refer to the previous paper) The vocabulary problem already starts in the abstract: “spreading of the charge”. Here I would add “signal” over several pads as in fact it is an induced phenomenon. ABSTRACT: ========== “spatial resolution better than 800 microns” -¿ but in the conclusion of the paper it is 600 microns... ==========

Fixed

Physics question: the spatial resolution needed is related to the resolution on the track momentum, with varies versus the momentum for the same spatial resolution. The tracks we will get with the HA- TPC will have lower momentum in average than the forward tracks... Is the 800 microns good enough (it was 600 microns ? with the old TPC and there were more points per track as the pads were smaller?). Same thing for dE/dx versus tracks types separation at the momentum of the tracks reaching the HA-TPCs?

With the old TPCs we had resolution of 1 mm for inclined tracks so with the ERAM we are better in all configurations. An interesting study would be to look at the expected momentum resolution given the observed spatial resolution for tracks emitted in neutrino interactions and entering the TPCs but this is beyond the scope of this paper.

INTRODUCTION : ========== Reference to the ND280-Up physics paper? Line 11: Reference to ND280-Up TDR?

both added

It is important in the introduction to make the link with the previous NIM paper to stress the extra knowledge we gain here. New field cage like HA-TPC – DRIFT !!! – Simulation ! Electronics?

We think this is already mentioned in the introduction

Line 19: allow -¿ allows

fixed

Line 20: “systematics uncertainties to the 4-5% level”: Reference ? explain X-sections and flux mainly, how ND280-up will allow to do better, important step also for next generation of LBL experiments... we need to reduce even more systematics

added a reference to the paper and a reference to the Upgrade physics paper

Line 25: reference for time of flight ? in ND280 Up TDR?

added

Line 32: “allows to spread the charge”: jargon – Clear overview and definitions need to be given

rephrased as: allows to detect the charge induced by ionization electron on one pad over several pads (’spread’), improving the space point resolution

I can try to write a paragraph separately for that **** reference to Old TPCs NIM paper could come here (ref [10]) Line 33: “Space point resolution” not defined, meaning hit ? Then need to define cluster of pads

rephrased as: improving the spatial resolution and hence the determination of the momentum of the charge particles

The cluster never was defined, it needs to, even if it is short, with ref to the first paper Line 39: explain the advantage of this new field cage / Old TPCs field cage + reference?

there is a new sentence: This design allows to minimize the dead space and maximize the tracking volume by reducing the distance between the outer TPC wall and the active gas volume from 12 cm to 4 cm. The radiation length of the material composing the field cage is 2%.

Line 46: TPC inside a 1T PCMAG =¿ precise B can be varied up to 1T – 0.2 T was used like in T2K later in the paper – What is PCMAG ??

improved

Line 47: momenta between 1 and 4 GeV : precise it can be chosen, and what would be the typical momenta for the tracks reaching the HA-TPC in T2K

the typical momenta for tracks reaching the HA-TPC from Super-FGD will be smaller

Lines 49-50 : A geometry scheme of the coordinates and different angles / ERAM plane and borders is really necessary, with consistent definitions

this is explained in Sect. 5 for the new version

Line 60-61: again vocabulary problem “charge sharing” used here, not explained and “spreading” was used before for the same thing.“neighbouring pads” not defined incident main pad not defined as well... “Time difference” not defined ... between what and what times (at maximum of waveforms)....

I think this is fine for the introduction, when we describe them later we define that we use maximum of charge and time at which charge was maximum

EXPERIMENTAL SETUP Here a geometry scheme would be more than welcome as explained before In Figure 1 caption: link with geometry ? Drift along the vertical direction of the picture? ERAL module on the top?

added

Lines 65 : reference old TPCs [10] to put here ? (earlier). “insulator” : replaces the CO2 insulating system in old TPCs ? explain

yes

Line 78: explain the choice of materials? reference ?

This should rather be done in a HA-TPC NIM paper in my opinion

Line 91:”exposing the prototype at DESY”exposing the prototype to the electron beam at DESY Lines 101-102: inconsistency between the dimensions. Replace 420 x 340 mm2 by 340 x 420 mm2

done

========== Lines 108-114: TO REWRITE confusing + references missing ? Both old and new TPCs have bclk micromegas? (technic of fabrication) , the new micromegas have an additional resistive layer. Not clear Also the avalanche is small / pad size is true for old and new – pads for new are a bit larger but the phenomenon is the same. Generally there is

a confusion between a single avalanche and the many avalanches that could be on one pad (20 to 100?). It needs to be clarifies There is also a pad size limit for both but differently, and the combine effect with the transverse diffusion that allows several pads to be hit. The role of diffusion and resistive layer have to be really clarified, in particular here in the test / old DESY test, with a long possible drift / short drift Line 113: another "charge spreading" jargon =¿ induced signal Line 114 : "Track position" not definedHits This part could be re-written –

it should be better in the new version

==========

Line 120-122: What is the difference in the electronics between this paper and the old DESY NIM paper?

For the old paper the ARC electronics was used

Line 133-134 : reference? "Neighbouring pads" not clear, "deviations not larger than 2%" in what? The response is linear but the "slope" can vary (2%)? Not clear.

done

Line 145: "existing TPCs simulation"reference ? Line 151: reference [10] could have come earlier Line 154: converted into the electron cloud -¿ not clear A BRIEF INTRODUCTION OF THE PHYSICS GOING IN THE TPC ON IS NECESSARY BEFORE GETTING TO THE SIMULATION. May be a drawing

I think this is well described at the begin of tje section

Line 155: "propagated to the ERAM plane"Jargon ? drift of the cloud simulated (ELECTRIC field (+magnetic too)

they are simply moved and then spread once arrived on the ERAM. In the simulation perfect magnetic and electric fields are assumed

Line 156: another one "electrons are spread"!! Here it is diffusion in the gas not to be mixed with the resistive effects. Where do the numbers come from? Also not clear between single avalanches and average effective avalanche over the pad

the numbers come from what is used in the nd280 software

Lines 160-162: where does the formula come from?

Line 163: again "SPREAD" jargon Line 167: equation wrong - sign missing + other mistakes "point like" signal not defined as well as "main pad"– where is the avalanche size in the formula – don't mix one avalanche / effective pad avalanche from multiple avalanches

the formulas are for one electrons

"over the pad borders"=¿ over the pad surface?

yes

Line 168: "an error function" - ¿ the standard Gaussian error function – "pad coordinates" coordinates of the pads centers. Main pad and neighbouring pads not defined !!!!!

it's not the pad centers but the pad boarders

Line 171: reference for the electronics response ??? Why derivative ? we don't understand fully but tried different things and this works the best ? We nedd to be honest

because that's what was implemented... the WF is the derivative of the charge convoluted with the electronics response that is equivalent of doing the charge convoluted with the derivative of the electronics response

Lines 172-195: needs work too "optimizations": they are more approximations - The first one is more a physics approximation which needs to be justified and explained (different avalanches/ pad, orders of magnitude of phenomena in that black box) – The second one is a computing approximation (grid) and a time optimization (keeping memories) but still an approximation

changed with approximations

Lines 183 188 : x and y coordinates were never defined Line 184: "grid of 10x10" UNIT ??

no units, see one of previous replies

Line 185: "electronics response" refer to Eq 3 and 4

Line 191: again leading and neighbouring pads were not defined

Line 195: spatial resolution not defined / simulated –true ? for a hit ? which conditions ?

plot changed

Figure 3: leading and adjacent pads not defined, before neighbouring not defined as well

Defined now

Why are the two figures not rescaled and put on top of each other? What are the conditions (coordinates of the average "hit" on leading pad), etc) – Definitions of 2nd and 3rd pads? "The example waveforms"? Waveforms were never explained before – It should have been earlier in the paper Why is there saturation in the Monte-Carlo?

these are just two examples. Saturation is there just due to the plot scale and is not affecting the paper results since we only use the maximum

Figure 4: caption: "resolution" not defined – Optimisation -¿ approximation Why is it more linear than Fig 13?

figure moved

Characterization of the ERAM detector This part needs to be clarified and better explained and linked with the rest of the paper Line 198 "consists into" -¿ consists in

FOR the characterization part, a lot of explanation is missing on the purpose and advantage of this measurement complementary to the beam test. (punctual source / tracks with hits on other pads affecting each other, harder to define a hit on the pad (multiple...), gain and RC maps measurements etc.... Link and complementarity with the rest ? Fundamental The different methods to compute the gain, the correlations, are not well explained. If we don't know the gain, bias on RC. Why knowing RC and the gain are important ? For simulation and analysis (determine the position of the track hits)

Line 212 : "event occurring " not clear, not precise Line 213: "directly hit by the photoelectrons" ? explain Line 217: the fit allows to disentangle the gain and RC, explain why – "a value of RC" associated to the pad but not of the pad. Explain the fit of the waveforms Figure 6 : "RC of each pad" -¿ associated

to each pad Gain map is interesting too but explain better and give motivations and links

Most of this discussion has been removed

COLLECTED DATA AT DESY – Transition and link with previous part missing Lines 222-225 again explain the magnetic field and electron beam energies can be adjusted, and / T2K conditions?

modified

Line 227 define horizontal and vertical – Missing geometrical setup definitions more generally (angles, etc...) Line 231 "RC uniformity and charge spreading" another jargon. Link with T2K physics ? Amount of charge spreading? What does it mean ? Is there a too large or too small? What is it exactly?

it should be fine now

Line 233 "cover" -¿ covers Line 236 gain scan -¿ average over the module (DLC voltage is changing) – Explain why you are testing that. What optimum you are looking for

since we do not show it we removed from the list

Line 242 again geometry definition missing

fixed

Line 246 Not exactly but the tendancy is to vary more along Y (but also along X)

removed

Line 248 : be careful between shaping and peaking times – explain why these two values are tested, give order of magnitude between this and the "waveform" i.e charge (space, time) time range evolutions if there were not electronics – search for best compromise between larger signal integration and less noise integration ...

we always say peaking time now in the paper

Line 249 : same thing again: angles not defined

RECONSTRUCTION ALGORITHMS This part also uses clusters and space directions that were not defined before.... Line 257 (cluster), line 259 (multiplicity), line 260 (resistive spreading), lines 266-267 (horizontal and vertical clustering) for examples

should be better now

Lines 275 -278 : how was the selection of track multiplicity done? Was there a monte-Carlo study done now that we have a simulation ?

No, we didn't try

GENERALLY THE SIMULATION IS GOOD AND COULD HAVE BEEN USED MORE IN MULTIPLE PLACES LIKE HERE

But the simulation just use single tracks and do not try to reproduce topologies observed in DESY.

Figure 7: an event is a track ? The mean multiplicity is the mean multiplicity of its clusters? Need to be clarified!! Explain and comment the figure. Draw a conclusion GENERALLY THERE ARE MULTIPLE PLACES IN THE PAPER WITHOUT EXPLANATION OR ONE HARD TO UNDERSTAND? AND NO CONCLUSION ON THE KNOWLEDGE BROUGHT

we say: a cut on the mean number of pad per cluster (multiplicity) of the track is applied.

Lines 279 – 281 – Not sufficient to compare to previous tests (remind previous tests) develop more!!! Other things changed!!

Not sure what you mean but we do not use the pads at the edge of the ERAM

Line 282 - CHARGE SPREADING CHARACTERIZATION !!! Jargon in the title...

changed to ERAM response in data and simulation

Line 284 – some some - "low level variables" what do you mean : in the track reconstruction process?

removed low level

Line 285 – Charge sharing !!! time differences ! not defined

it is defined few line later

Line 290-293 : charge ratio and time difference not defined (at maximum of waveforms) What is the interest of analyzing that ? Explain

because those are the variables describing the features of the ERAM

FIGURE 8 – track position not defined (one dimension, depends on track angle? Position of hit ? not clear) Why is there a shift between data and MC ? It is the ratio of charges and not times ? Is it because the track position is not well known? There is a kind of systematics? Can it be adjusted this bias and understood ? Q1, Q2, and Q3 not defined as well as second and third pad

we added a comment saying that it the agreeemnt is pretty good and could be improved by optimizing the RC value

Line 304 : what is MPV ? "plotted in 10" -¿ in Fig 10?

now it's defined

Lines 308 -309 : We know that a RC difference should affect more the time distributions than the charges ones. We need to explain that better. The effect on the charge is due to the electronics response.

we added a sentence on that

FIGURE 9 and 10 captions : again quantities not defined

should be defined now

FIGURE 10 : how is xtrack –x pad defined ? Fig 10 b point strange in the middle? Comments missing. Not just RC is different ? What were the differences in the electronics between the two test beams compared on this figure ? and the peaking time ? What are the conclusions of all this ?

x_{track} is the reconstructed track position and x_{pad} is the pad center. The reason for which there are two points at 1 cm is that, if the track is at the center of the pad, the second and the third pad have the same distance from x_{track}

Lines 317 to 348 Residuals not defined (what is the reference?) what no study with MC ? truth?

This is not the truth because we want to apply the same method to data and simulation. The defintiion of residual is now added: In this method we define, for each cluster, the residual as the difference between the position of the track reconstructed locally (e.g. in one column) and the position according to the fit

of the track. The distribution of the residuals in each cluster is fitted with a Gaussian and its width represents the spatial resolution.