Dear Reviewers

Thank you for the very careful reading of the manuscript. Your comments and questions have improved the quality of the manuscript significantly.

Here below the replies/answers to questions and remarks and actions in blue that were taken.

See you Peter Kluit

## Reviewer #1: Please find minor line-by-line comments below:

50: readout -> read out

Done

50: SPIDR -> "SPIDR"

Done: replaced by (SPIDR)

61: In this paper,

Done

Table 1: was water content measured or determined from drift velocity measurements are described below?

Answer: We did not have a (e.g. sensor) measurement of the water content - only an estimate from the drift velocity measurements. NB: This point is mentioned in line 235.

232, 241: Please give values for the errors even if they are small.

Done: Dt 287.2 +- 0.5 (B=0) Dt 120.3 +- 0.5 (B=1)

279: Specify which hits were excluded - radius around pillars?

Answer: Only chips were excluded (detailed in lines 274-277).

The hits near the pillars were not rejected.

Hits near the edge in zone of 5 (10) pixels were removed.

As mentioned in lines 290-291.

289: Due to the presence of the dike,

Done

290: Therefore,

Done

312: Because of limited statistics,

Done

340: bias -> basis

Done

349: Telescope -> telescope

Done in many other places it was changed

363: place reference at end behind number to improve readability

Done: rephrased like: " for the T2K gas by \cite{Garfield},"

367: Time Projection Chamber -> TPC - already defined above

Done

## Reviewer #2:

The manuscript entitled "Towards a Pixel TPC part I: construction and test of a 32 chip GridPix detector" reports on the construction and first test beam results of a Time Projection Chamber (TPC) read out by 4 x 8 GridPix chips, 256 x 256 pixels each, and a maximum drift length of 40mm. Building upon previous detectors employing a single or four GridPix chips, the results presented in the manuscript are significantly new and important, a priori justifiying publication in Nucl. Instr. Meth. Before doing so, however, a number of issues, related to the presentation and the analysis should be addressed.

## Major comment a):

a) The manuscript is a bit sloppy about the use of the terms "resolution and residual". I insist that this is corrected. The resolution is related to the intrinsic performance of the detector. It does not include external uncertainties like the uncertainty of the reference track. The residual is the difference (measured - expected) hit positions for a single track/plane. The residual distribution is the distribution of residuals over many tracks. Being a distribution, it has a mean and a width. In this sense, e.g. Eq. (2) gives an expression for the width of the residual distribution, not the resolution. Figure 5 shows the measured width of the residual distribution (could be called residual width, if defined like that). Figure 7 shows the mean of the residual distribution (could be called residual mean, if defined). Similar examples can be found all along the manuscript. In addition, it is not explained in unambiguous terms, how exactly the residuals in xy and z are calculated. For every hit, one normally calculates the 3D-vector of the closest distance to the track. This vector projected onto the xy plane gives the residual in xy, its projection onto the z axis gives the residual in z. What value is used for the z coordinate (hit z position or track z position at DCA)? Please explain in the text. In Section 5.4, the authors talk about "tracking precision in the middle of the TPC". Please clearly define the term and explain how the numbers were obtained. Uncertainties (or rms values of the corresponding distributions) should be given for the numbers quoted.

## Answer to  Major Comment a)

Indeed, possible confusion about the residuals and the detector resolution should be avoided.

The definition of residual in xy and z is added. We use the standard definition:  the distance of closest approach of the track in the xy plane is used and at that point the xy and z residuals are defined.

The text was corrected - in the following places - as proposed by the reviewer to avoid confusion.

### Added at the beginning of section 5:

“The single electron hit resolutions in $xy$ and $z$ will be extracted from the residuals with respect to the fitted track. The track residual in $xy$ is the closest point of the track in the $xy$ plane to the hit at the center of the pixel. The residual in $z$ is calculated at this point of closest approach. “

### Rephrased subsection Hit resolutions in the pixel plane

"The residual of the hits in the pixel plane ($xy$) was measured as a function of the predicted drift position ($z_{\rm drift}$).

The spread on the residual in $xy$  for an ionisation electron is given by:

```
\begin{equation}

    \sigma_{xy}^2= \sigma_\text{track}^2 + \frac{d_\text{pixel}^2}{12} + D_T^2(z_{\rm drift} -z_0),

  \label{eq:sigmax}

\end{equation}
```

where  $\sigma_\text{track}$ is the uncertainty from the track prediction,  $d_\text{pixel}$ is the pixel pitch size, $z_0$ is the position of the grid, and $D_T$ is the transverse diffusion coefficient. The last two terms correspond to the single electron detector resolution (squared).”

### Caption of Figure 5 is also rephrased:

```
\caption{Measured spread on the residuals in the pixel plane (black points) fitted with equation \eqref{eq:sigmax} (blue line).}
```

### Rephrased subsection Hit resolutions in the drift plane:

The spread on the residuals in $z$ of the ionisation electrons $\sigma_z$ is given by:

```
\begin{equation}

  \sigma_z^2= \sigma_\text{track}^2 + \sigma_{z0}^2+ + D_L^2(z_{\rm drift} -z_0),

  \label{eq:sigmaz}

\end{equation}
```

where $\sigma_\text{track}$ is the expected track uncertainty, $\sigma_{z0}$ the detector resolution at zero drift distance and $D_L$ the longitudinal diffusion constant. The last two terms in the equation correspond to the single electron detector resolution (squared).”

Recall the definition of "the middle of the TPC"

The exact location of the "the middle of the TPC" was defined in line 177 as (at y = 1436 pixels). We added the location to the text in section 5.4 to remind the reader.

## Major Comment b)

b) It is somewhat surprising that the statistical uncertainties on the measured diffusion coefficients are so small. What is the reduced chi2 of the fits in Figs. 5 and 6? The authors say a few lines later that "the values of the diffusion coefficients depend on the humidity that was not precisely measured". In Line 192f, they write about "changes in the relative humidity of the gas volume due to leaks". Why does this not affect the extraction of D_T and D_L from the data? In addition, the statements in Line 234ff are hard to understand. Do the authors want to say that they obtained an estimate for the humidity from a comparison of the measured drift velocity with Magboltz, and that these humidity values were then used to obtain the \pm 4% quoted for the Magboltz "prediction" in Line 233? In any case, uncertainties (statistical or systematic or both) have to be attached to the measured values of the diffusion constants.

## Answer to Comment b):

We agree that uncertainties for the transverse diffusion should be quoted in the paper (the uncertainties on the longitudinal diffusion were quoted).

The uncertainties are now quoted in the paper.
Dt 287.2 +- 0.5 (B=0)  Dt 120.3 +- 0.5 (B=1)

The uncertainties are small because of the high statistics.

Q; What is the reduced chi2 of the fits in Figs. 5 and 6?

Answer: The fit chi2 for Figures 5 and 6 are:
Fig 5 left: 120.485 NDF 137  right: chi2 18.9573 NDF 35
Fig 6 left: 120.775 NDFz 121  right: chi2 47.5922 NDFz 34

Q: Why does this not affect the extraction of D_T and D_L from the data?

Answer The humidity will affect the MagBoltz predictions. The procedure to determine the humidity that was not measured is well described by the reviewer. The prediction from MagBoltz gets an uncertainty that is quoted.

Concerning systematic uncertainties. it was found out that there is a systematic uncertainty that affects the extraction of the longitudinal coefficient. The paper was corrected for this and a systematic error of 14 mu m /sqrt(cm) is quoted. Uncertainties (statistical or systematic or both) are attached to the measured values of the diffusion constants in the new version of the paper. Text is added to the paper to explain this.

## Major Comment c)

c) It is somewhat difficult to reconcile the results for the resolution, the deformations and the efficiency, because it seems that they are based on different data sets or at least on different cuts applied to the data. For example, the track selection seems to be much stricter for the resolution studies in terms of fiducial cuts and amplitude (ToT) than for the efficiency. I understand that there may be reasons of

statistics, etc. to do so, but I still want to express my concern that in the end, for physics measurements, it is the combined performance for a given set of cuts that counts, not the best possible value for each single parameter, obtained under different conditions.

## Answer to Comment c):

There are good reasons to apply selection and acceptance cuts. In particular for the studies that focus on the systematics in the TPC one has to apply strict cuts on matching cuts of the Telescope to the TPC to reject backgrounds.

We share the concern of the reviewer and the question "how does this extrapolate to a real experiment?" For TPC tracking in a real experiment the tracking efficiency is very close to one for the module because of the high single electron efficiency and the high number of electrons per crossed chip (See Fig 11: 124 B=0 and 89 B=1) and the high number of hits on the track that crosses the module of 8 chips (8*124 and 8*89). Note that the number of electrons per crossed chip is after the hit selection (including a ToT cut).

## Major Comment d)

d) The procedure leading to Figs. 8 and 10 is not understandable, at least not to this referee. The text in Lines 263 - 301 and in Lines 323 - 336 has to be completely rephrased in my opinion.

Answer to Comment d): the text is rephrased (see details below).

Q: "the module was regrouped in four 256x256 pixel planes put side by side on the horizontal axis",

Answer: To explain this better it was rephrased and a sentence was added

"the module was regrouped in (4$\times$256)$\times$256 pixel planes put side by side on the horizontal axis, as shown in figure \ref{fig:deformationsGroupedB0}. E.g. the selected chips from the upper left and bottom left quad detectors are combined into the 0-256 (x) and 0-256 (y) plane"

and

"Similarly, regrouping the module in 256x(4x256) pixels put them side by side on the vertical axis,"

Q: "A bias in the mean residual..." => shouldn't this statement also be true for Figs. 7 and 9? If yes, it should be moved there.

Answer: Indeed we move the sentence up.

Q: "due to the presence of the dike pixels..." => dike pixels have not been defined

Answer: a comma was missing. "due to the presence of the dike, pixels"

Q; "the region near the edge of 5 pixels was removed"

Answer: "the region near the edge of the chip of 5 pixels"

Q: "a region of 10 pixels was removed"

Answer: "a region of 10 pixels near the edge of the chip was removed

Q: what is the "expected statistical error"?

Answer: It is the expected uncertainty on the r.m.s. in the pixel (drift) plane for the regrouping and the available statistics. This is obtained by propagating the uncertainties. This is now added to the text.

## Major Comment e)

e) Generally, the presentation towards the end of the manuscript (End of Section 5 and Section 6) seems to decline in quality with respect to the other Sections. I suggest that the authors have a closer look at these sections.

Answer: we followed up on the suggestions.

Q: In Section 5.4, no details are given on how the numbers are defined or extracted (see also comment a) above).

Answer: the uncertainties on the track parameters (position and angle) are propagated and the mean of these are given in this section. This explanation is added to the text.

Q: In Section 6, the difference between the number of hits (corresponding to number of ionization electrons) and the time over threshold (corresponding to charge after amplification in the Micromegas grid) should be made clear. In addition to the distributions of the number of hits, also the distributions of the ToT could be shown.

Answer: A sentence is added to explain that the ToT is related to the deposited charge. We will refer Fig 5.5 of the thesis of C Ligtenberg for a ToT distribution.

Q: The authors mention that the measured mean number of hits is in agreement with the prediction from ref. [13]. But given the Landau fluctuations of the number of ionization electrons and the large tail, wouldn't the most probable value be a better number for this comparison? Alternatively, one could show the predicted distribution along with the measured one.

Answer: From the MagBoltz calculations we have only the mean number for the T2K gas. We did not perform a full MC event-by-event simulation with Garfield/MagBoltz of the detector that would allow to extract the most probable value.

# Additional comments:

### Abstract:

- Line 19f: quoting the result for transverse and longitudinal diffusion coefficients without specifying the gas does not make sense.

Answer The uncertainties are quoted. Also a line is added to specify the gas.

- Line 20: remove "D_T is"

Done

- Line 22: the phrase "the diffusion measurements have negligible errors" is not valid. Every measurement must be accompanied by an estimate of the uncertainty!

Answer The line is removed and the results are quoted with uncertainties.

## Introduction:

- Line 36: what clusters are meant here? Ionization clusters?

Answer: The primary clusters that each several ionization electrons.

- Line 43f: with an ENC of 70 e-, why is the threshold set to more than 7 sigma?

Answer To suppress the noise. The distribution has a non-gaussian tail. We don't want to flood our byte stream with noise hits. See also our previous publications.

- Line 45: be quantitative instead of claiming "high efficiency" I did not find the information on the number of pixels per TPX3 chip in the paper.

Answer: Indeed it is better to be quantitative. In the single chip paper an efficiency of about 80% is stated. In this paper we show a 85\% single electron efficiency. We added in the introduction the 256x256 pixels.

We rephrased as "and a high efficiency of about 85\% - demonstrated in this paper - to detect single ionisation electrons."

In the abstract we put: "high efficiency of about 85\% ..."

- Line 50: "readout" => "read out"

Done (in many places)

## Section 2:

- Line 72 and 89: "guard" => "guard electrode"

Done in more places we changed guard to guard electrode/strip etc.

- Line 86: "diameter"

Done

- Line 86: check use of "and"; what are the "guard strips"? Can they be indicated in Fig. 2?

Done "and" is removed. Guard strips are on the 4 sides of the module (in the quad plane).

Done: Fig 2 was updated: the different guard wires etc. are indicated

- Line 92: "two 50 um thick Kapton windows"

Done

Figure 1: should be enlarged

Done

- labels and a scale should be added

Not needed: Scales are in the text and in Figure 2

- "rendering"?

Done

- stick to either "32-GridPix module" instead of "8-quad module" throughout the text

Done

Figure 2: add labels, e.g. for guard wires, guard strips, guard electrodes, pillars for field wires, etc.

Done: We added color coding and explanation in the caption

## Section 3:

- Line 114: "At DESY, the Mimosa26 silicon..." (add comma and remove parentheses)

Done

- Line 124: "parameters"

Done everywhere parametres->parameters

- Line 125: add "and" before "oxygen"

Done

- Line 134: "systems"

Done

- Line 137: "of" => "with respect to"

Done (rephrased)

Figure 3: is this a top or side view?

Answer Side view:
Captions reads as "Photo of the detector setup - side view - at the centre of the PCMAG magnet (the circular contour) . The Mimosa26 planes M0 and M3 are indicated in red as well as the beam direction (yellow)."

The main text now has "The stage positions the TPC module with respect to the beam and the Mimosa26 planes can be adjusted."

Table 1:

- is the number of runs important?

Answer: Not crucial, but this is important if somebody wants to redo the analysis (Open Data Acces)

- are the beam momenta relevant? (see also corresponding comment below)

Answer: Yes, the multiple scattering is a bit different and the electron/trigger rates are different.

## Section 4:

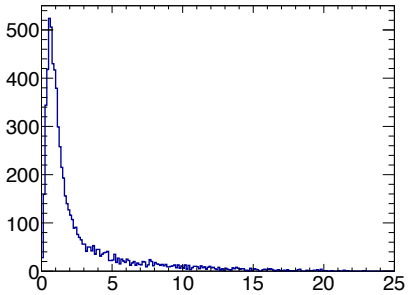
- Line 155: check language in "Telescope tracks were selected with at least 5 out of 6 planes on the track" => "Telescope tracks were required to have hits in at least 5 out of the 6 planes"

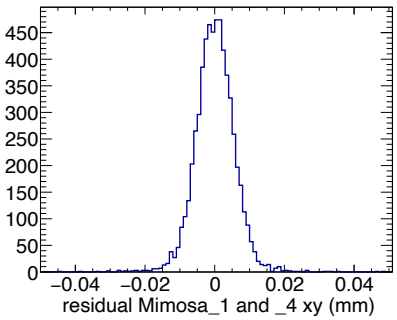
Done

- Line 156: why is the track chi2 cut so large? What is the distribution of the reduced chi2?

Answer: The value was chosen to keep a high tracking and silicon plane "on track" efficiency.

Plots for the chi2/ndof and the residuals in the M1 plane




- Line 176: with respect to what has the acceptance window for GridPix hits been defined?

Answer: it reflects the size of the quartz window

- Line 177: "was" => "were"

Done

- Line 179: check language in "quadratic track B=1 T model"

Done

- Line 180: give values for the "expected uncertainties". How were they obtained?

Answer: A sentence was added to clarify this "The expected uncertainties were derived using the parametrisations discussed in section \ref{sec:hitResolution}."

- Line 181f: the phrase "outlier removal at respectively 10, 5 and 2.5 sigma level" is hard to understand, please rephrase. What sigma do you refer to here?

Answer: Here sigma means the expected uncertainty on the hit. Text change to "The fit was iterated three times to reject outlier hits at respectively 10, 5 and 2.5 sigma."

- Line 183: why do only 25% of all hits lie on the track? Figure 4 looks much cleaner. Please explain.

Answer: the text says "More than 25%". In most (clean) cases all the hits end up on the track.

- Line 185: what orientation does the "plane in the middle of the TPC" have?

Answer: y = constant. So x and z any value: so orthogonal to the main beam direction.

- Line 199: add "from" before "run"

Done

Figure 4: - red and blue lines are hard to distinguish- green points are hardly visible

Answer: not easy to make this better without spoiling the overall picture. In the electronic version one can zoom and see it.

- "driftplane => "drift plane"

Done

## Section 5:

- Line 208: "Secondly" would require a "firstly" before

Done Added Firstly,

- Line 220: "staying 20 pixels away from the chips edges" => are all 4 chips edges meant here?

Answer: Only the chip edges in local x (orthogonal to the beam).Text is now "staying 20 pixels away in local x from the chip edges'

- Line 221: see above comment on "resolution"

Answer already discussed under Major Comment a)

- Eq. (2): give a reference, e.g. [Yonamine et al., JINST 9 (2014) C03002]

Answer: We think it is not needed to cite a reference: one can also find the expression in our previous papers.

- Line 228f: "sensors", "windows"

Done

- Line 244f: "resolution ... in the drift plane" => should be "residual width in z direction"? The expression "drift plane" has not been defined.

Answer: this has been rephrased. The z direction is the drift direction.

- Lline 249: why is the ToT cut chosen for the z residuals so much higher than the one applied for the xy residuals (0.6us vs 0.15us)? How does this affect the efficiency?

Answer: The time slewing - that depends on the ToT - has an important impact on the resolutiom of the detector. This has been discussed in our single chip paper ref 1. There we used a ToT cut at 0.6us. The efficiency of the cut is about 50%. NB For TPC tracking we use of course all hits with ToT > 0.15 mus.

- Line 257: what do the authors conclude from the discrepancy between the measured value of D_L and Magboltz? Is Magboltz data wrong, or are there systematic effects that were not taken into account?

Answer we followed up on the longitudinal Diffusion this point and found a systematic uncertainty that was over looked - as discussed above. The results are in better agreement now.

- Line 274f: it would greatly facilitate reading if the chip numbers quoted in the text were visible in Figs. 7 and 9, without having to go back a few pages to Fig. 2.

Answer: The reader can find the information and the text also explains where the chips are ("corner chips").

- Line 302: "electrons will drift mainly along the magnetic field lines" seems not entirely correct. What is the value of \omega\tau? I suggest to remove the sentence, as it is not needed.

Answer: Omega tau is about 4.5 and. So the electrons drift mainly according to the B field lines.

- Line 312: mention that these are now biased residuals, in contrast to the ones for B=0T, where the external track was used as reference

Done

Figures 7, 9:- add labels and units for z axis

Answer: We changed the caption "Mean residuals (color coded in mm) ...

- is the binning really 8 x 16 pixels? Zooming in, it seems that there is an equal number of bins in x and y for each chip, which would imply an 8 x 8 binning.

Answer: Thanks for spotting this. The bin size is indeed 16x16 in xy and z. This is now corrected in the paper.

Figures 8,10:- add labels and units for z axis

Answer: see above: we changed the caption "Mean residuals (color coded in mm) ...

- what is the "regrouped expected hit position"?

Answer: "the expected hit position" (in the plane). Text changed.

## Section 6:

- Line 350f: check language in "the B=0T analysis selects the ...." => "For the analysis of the data with B=0T, the chips ... were selected" or similar?

Done

- Line 352: why were chips 12, 13, 20, and 21 excluded for the B=0T analysis? For a proper comparison of B=0 and 1T data, wouldn't it be advisable to use the same data set?

Answer For B=1 chips 12, 13, 20, and 21 could be included because of the different Beam angle in the B=1 data set that allowed a larger acceptance. One could remove these chips (12,13,20,21) for “consistency” but that would decrease the statistics.

- Line 362: typo in "possibility"

Done

Figure 11:

- typo in "per per" - check language in caption

Done

## Section 7:

- the authors emphasize here again that data were taken at two different beam momenta, but throughout the analysis in the previous sections, no mention is made, which beam momentum the data correspond to. Have both momenta been used, e.g. also for Section 6? Probably the authors should state for each Section, which beam momenta were used

Answer: The data correspond to beam momenta of 5 and 6 GeV in all sections. In section 6 too (this has been corrected).

- Line 380f: this statement does not make much sense, unless an explicit public link to the data is given

Answer: One can contact LCTPC/DESY or the author for the links (note that Grid links do change).

## Language and format:

- put symbols for physical quantities in italics: B, x, y, etc.

- do not put units in italics, e.g. GeV, cm

- use mathematical symbols where applicable, e.g. "\times" instead of "x"

- use SI units where applicable, e.g. there is no need to define T as Tesla when talking about magnetic field strength

- check use of hyphens (missing very often, especially in compound modifiers like "32-chip module", "high-precision tracking",...)

- references: should be in square brackets, not in parentheses; use [1,2] instead of (1), (2).

- format tables 1 and 2 for better readibility

- slided => slid

- avoid use of jargon, e.g. "quads", "guard", etc.

- avoid unphysical statements like "great precision", "high efficiency", ...

- check use of commas

## All Done