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Interactive comment on "A comparison between the GNSS tomography technique and the WRF model in retrieving 3D wet refractivity field in Hong Kong" *by* Zhaohui Xiong et al.

Anonymous Referee #2

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General comments: The article shows an interesting study by combining two different approaches of retrieving 3-dimensional wet refractivity fields. However, in my opinion there are some major deficiencies, which need to be addressed before publication. What is the novelty of your approach and how can the NWP community benefit from this? This is not fully clear especially with respect to the huge effort in creating the tomography fields compared to a simple ZTD calculation. Therefore, I suggest major revisions before publication of the manuscript. Please avoid using "may" all the time. Please make a more precise statement.

As I am not an expert in GPS tomography, I will mainly focus on the modeling part.

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Discussion paper

Specific comments:

lines 55-62: Are there also other NWP models than WRF, which make use of ZTD/PWV data assimilation?

line 77: What do you mean with “vertically flat” in this case. Is your statement related to the altitude difference of 344 m? I do not think that this can be considered as “flat”.

Line 84: maybe “dry” instead of “rainless”.

Lines 85-96: I think it is a good idea to show the applied parameters for Bernese in a separate table. In line 92/93, I guess you mean Niell in both cases. Are all GNSS receivers equipped with temperature and pressure measurements? If not, please mention how you derive the ZTD at the receiver locations.

Lines 99-102: The general purpose of any data assimilation scheme is to obtain the best estimate of the atmosphere not only with respect to ZTD observations. Did you assimilate any other observations than ZTD? It is well known, that one should make use of all available observations to complement each other. Especially as the 3DVAR does not contain any dynamical component. How was the 3DVAR set up? Is it a rapid update cycle with e.g. an hourly update or did you ran the 3DVAR once at the beginning of your period of interest? Did you apply multiple outer loops? What is the ZTD error you used? All these details are important to know as this determines the weight/impact of the observations and the data assimilation in general.

Lines 102-104: Does the model domain only encompass the area shown in Figure 1? If this is the case, you may only have approx. 30*25 grid cells. Assuming a boundary relaxation zone of 5 cells, you effective model domain will be 20*15 cells which is far too small. The model does not have a chance to develop its own state but is mainly determined by the boundary conditions. Please clarify. Did you apply the default layer settings in WRF by setting “eta_levs” to a certin value or did you define the levels on your own? How many layers are in the PBL? This may be important as the majority of

the humidity is located inside the PBL. A lot more information is necessary here.

Lines 109-111: Did you apply the reanalysis of ERA-Interim, which has a resolution of 0.75° and not 0.125° or the operational analysis? Are the forcing data applied on model levels or on pressure levels? In case you applied the former data, I'm afraid that this is not a suitable data to study the behaviour of a convection permitting model especially at these short time scales although data assimilation is applied.

Line 115: To me the applied CV3 method is a major concern. I guess you know that this matrix is derived from a NCEP model climatology at a horizontal resolution of roughly 2° and this is applied on the CP scale in your study. I am concerned if this is a scientifically valid approach.

Line 118: I think the word reanalysis is misleading here as you probably only used ZTD observations. I also do not really see from the Vedel and Huang publication how WR is derived. Please also include the units for k1 and k2. Are T and P only used at the surface? This is not clear here. Also, please use "p" instead of "P" for pressure.

Line 137: Do you mean the ECMWF (re-)analysis or the new analysis obtained from WRFDA?

Line 138: What do you mean with "nearest four grids"?

Lines 139-140: Why did you adjust the radiosonde data? This distorts the radiosonde observations. I strongly recommend to interpolate the GNSS and tomography fields to the radiosonde location. How did you interpolate the unevenly distributed WRF model layers to the tomography layers, which have a constant spacing? The native WRF model output is not on pressure levels but on terrain following coordinates. I think is it necessary to include a short paragraph here.

Line 143: Is "Reanalysis 2" your control run mentioned in line 117 or is this an assimilation run where everything except ZTDs was assimilated?

Line 152: Why does this lead to a decrease in performance in the tomography and the

WRF model?

Line 154: Did you perform any significance tests or do you mean something like “considerably”?

Line 173: Why does the tomography “may” perform better than WRF? I though you did investigate this?

Lines 196-198: This statement is very confusing and queries the results of your study.

Line 208: With ZTD you do not assimilate the column water vapour. The signal delay is assimilated from which TCW can be derived.

Line 210: From your results I do not agree with this statement.

Line 213: How did you assimilate relative humidity only in WRFDA? If you use p and T from reanalysis (again the WRFDA analysis or ECMWF?), the tomography is not model independent anymore.

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