

Interactive comment on “A new approach for GNSS tomography from a few GNSS stations” by Nan Ding et al.

Anonymous Referee #2

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The manuscript is overall well written and truly presents a novel approach for wet refractivity vertical profile reconstruction using GNSS slant wet delays. I don't have any significant major comments, but I would like to ask the authors to address some mostly minor comments and correct some small language issues.

Comments:

- line 104: I would slightly correct the statement, that ionospheric delay can be cancelled out using an ionosphere-free linear combination. This LC takes care only of the first order effect, not completely everything.
- line 110: term R in given formula (1) is usually explained as a post-fit residual, not as "the unmodeled delay", because it is definitely not given only by the tropospheric delay.

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Have you applied post-fit residuals to your SWD used for tomographic reconstruction since you give them in the formula? Have you cleaned them from systematic effects as multipath? See i.e. paper <https://www.atmos-meas-tech.net/10/2183/2017/amt-10-2183-2017.pdf>, this is a rather important step before using post-fit residuals in slant delays.

- line 146, caption of Fig. 2: please try to correct the form in which dates are given (in the used way the sentence at UTC 0 on 1(DOY 121), ..., in May 2015 is little bit tricky to understand)

- section 3.1: you don't provide any information about the initialization of your tomographic system for the reconstruction, what is an important thing. Have you used some external data as NWM fields, or you used only standard atmosphere water vapor distribution?

- line 316: I would try to correct the sentence where you say that "the number of SWD observations from a tomographic epoch was seven". I would rather write that SWD observations from seven epochs stacked to one tomographic modelling interval were used

- line 318: you present a meteorological situation during May 2015 with total amount of precipitation, but it would be also good to know how the rain events were distributed in time. Were there any severe rains? Or was the precipitation somehow evenly distributed over whole month?

- line 323: I agree that water vapor decreases exponentially with height during standard atmospheric conditions. But there can easily occur inversions of water vapor (wet refractivity) in vertical profile, at some latitudes and time periods they are quite common. Have you analyzed the radiosonde (RS) data on this or not? I mean how you checked if vertical profiles from RS evinced some inversions and if yes, then it would be good to say how often, at which heights, etc.

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- section 3.2: do I understand it correctly that you interpolated GNSS tomography results on the RS profile to compare these two techniques? With the term "all RS sampling points" you mean all points where RS provided its measurements? If I understand it correctly then I am not sure if this way of comparison is an optimal one, because RS provides its measurements at slightly different heights everytime. So I think it would be better to horizontally interpolate GNSS tomography results to the position of RS, and then vertically interpolate RS values to individual layers of your tomographic network. Then you would be comparing all the time the same. Can you comment on this?

- line 352: are you sure that the reason of difference between "the worst and the best results" lies only in the distribution of slant signals used for tomographic reconstruction? Can't there be also an relation with weather conditions or quality of input SWD signals or something else? I would not be so strong in your statement regarding this.

- line 368: you state that statistic values listed in Table 1 are "all small", what means that your "new proposed tomography approach" is feasible". Could you put these values in relation with some other published tomography studies which ideally used a similar territory and a similar season? It could put your numbers into some perspective.

- line 386: I am a little bit surprised with your statements that "... near the ground surface, ... the density of the GNSS signals is very high" - generally the GNSS tomography technique is considered to have troubles to reconstruct water vapor fields well in the boundary layer of troposphere, since the number of slant delays at important low elevation angles is rather limited. What elevation cut-off angle did you use for your GAMIT processing and later in your tomographic reconstruction? Can you comment on this and ideally provide some statistics of how many voxels were penetrated by (how many) signals at various heights? This would support your given statements.

- line 407, table 2: you present a number of outliers, but not the total number of compared values. Were the outlier values included in or excluded from the presented overall monthly statistics?

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- I would propose to decrease size of most of the figures in the manuscript

Language correction:

- line 43: use tomographic instead of tomogpaphic

- line 164: I propose to use "... are named pierce points..." instead of current "... are name pierce points..."

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