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Interactive comment on "Observing geometry effects on a GNSS based water vapor tomography solved by Least Squares and by Compressive Sensing" *by* Marion Heublein et al.

Anonymous Referee #2

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Although the manuscript is overally well written and generally contains work which is worthy to be published, I have some comments and questions including three major ones. I would like to ask the authors to adress all of them.

Major comments:

Section 2: Related work

I miss references to some of the important works related to GNSS tomography. To be more specific: - I mainly miss the work of Bender and Raabe (2007) dealing with preconditions to GNSS tomography and Bender et al. (2011) presenting evaluation of benefit using multi-GNSS tomography instead of GPS-only tomography. Paper by

Bender et al. (2009) is also related to the topic of reviewed manuscript.

- P2L22: i.e. Rohm et al. (2014) also used a Kalman filter for GNSS tomography - a novel and interesting approach for a geometric discretization of the tomographic network can be found in Ding et al. (2018)

Section 3.3:

- P6L28: I do not understand why you selected only 5 vertical layers in your study. I can hardly imagine any practical usage of such a coarse information about wet refractivity (water vapor) vertical distribution - i.e. your boundary layer has thickness of 1300 m while according to Seidel (2002), under standard weather conditions 50% of all water vapor is distributed below the height of 1.5 km above the surface. You mention that the boundary layer thickness is set to 1300 m in order to that signals pass the neighbouring voxel within the same height layer. Since I haven't seen this requirement in any other GNSS tomography study, could you please give reasons for it? In this regard I want to also ask why you have selected cut-off elevation angle of 7° while nowadays good quality slant delays could be obtained down to 5° or even 3°?

According to results of other existing GNSS tomography studies (which many of them you cite in your manuscript), using between 10 and 15 vertical layers is definetely feasible. Therefore I would like to ask you to repeat your study with a higher number of vertical layers (at least 10) and provide the new results in the manuscript since I guess this setting can influence the results.

Section 4:

I have some questions and comments related to results presentation:

- P8L13: you write that figure 3 shows results "for an exemplary voxel of the lowest voxel layer". Which voxel is the selected exemplary one? The center one in 5x5 network? How have you selected right this voxel? Have you realized an overal evaluation across all the voxels? If yes, what were its results? Did you find (significant) differences among

individiual voxels or not? If not, I encourage you to do it and provide the results in the manuscript. Have you also studied the distribution of differences at various height levels? If yes, what were the results? I.e.: did LSQ or CS provided better/worse results either in boundary layer or at top of the troposphere? What was the impact of using more signal directions/GNSS sites? If you have not done such an evaluation, I again strongly encourage you to do it and present the results.

- P9L10: I know that you state in your conclusions that your findings are valid only for your limited study and encourage (yourself) to do more work, however I am afraid that one 48 h session can hardly provide enough data for a reasonable and trustworthy evaluation. Firstly, you should provide information on what period it was (summer, winter season), what were meteorological conditions during it and why you have selected it. Secondly, I would recommend to use at least two different periods since the results can be related to weather conditions and I encourage you to do so for your next version of the manuscript.

Minor comments:

- P2L11: you mention various applications of Compresive Sensing (CS) in this paragraph and some of its characteristics in the following paragraph, however the reader can be interested in a basic description of CS principles. Although you probably have a detailed description of CS in your cited papers (Heublein et al., 2018 and Heublein, 2019), I think it would be worthy to provide a short one (excluding the formulas which you have in Section 3.2) also here.

- P5L11: you mention a usage of "prior knowledge from surface meteorology". Could you be more specific on this? What exactly do you use for this purpose and what is the source of meteorological data (blind model, numerical weather prediction model, in-situ observations)?

- P5L20: just an information which you probably already know: although the water vapour (wet refractivity) standardly decreases exponentially with increasing height

above the surface, inversions in its vertical profile at various heights can commonly occur. And GNSS tomography solution should be ideally able to reconstruct such inversions.

- P7L0, Table 1: I wonder if it is worthy to keep the table in the manuscript since most of the information is also given in the paragraph below the table. I would like to warn you that GPS is using a basic constellation of 24+3 with a maximum possible number of 36 satellites since 2011, see i.e. https://www.gps.gov/systems/gps/space/. The 21+3 constellation stated from Hofmann-Wellenhof et al., 2008 was a previous one.

- P7L11: is there any reason why do you omit the chinese global GNSS system BeiDou in your study?

- P7L21: can you describe how do you define the "48 signal direction samples"? Using a real observation characteristic of satellites from individual GNSS systems or using something else, i.e. a regular azimuth/elevation spacing for signals?

- P8L7: could you provide at least basic information about a) raytracing technique which was used to compute slant delays? At least a reference should be given; b) WRF model used for your study - i.e. information about who operates this WRF model, for which area, what horizontal/vertical resolution is used, what is the source of initial and boundary conditions, etc.

- P9L4: probably a verb is missing in the sentence in the first half of the line

- P10 Figure 3: the scale of y axis is -25 ppm to +25 ppm, not -20 ppm to +20 ppm as you write in the figure caption. I suggest to provide mean and standard deviation values in numbers as the min/max value. They will be definetely better readable then. You should also increase font size of all text inside the figure.

- P11 Figure 4: I wonder if the figure 4 brings any useful information for the reader (you do not even describe its content in the manuscript). If you want to keep it, please provide information what you want to show with it (in my sense all the vertical profiles

are very similar to each other and are of an expected shape) and improve it (i.e. how does the voxel numbering works? Where is voxel 16 and where is voxel 17? For which date/period is the figure valid?).

- P11L1: I think the first sentence of the paragraph is completely expectable and logical and could be therefore deleted.

- P11L17: What exactly do you want to say with your sentence "I.e. this study recommends the use of LSQ resp. CS for water vapor tomography disposing of GPS-only resp. of multi-GNSS SWD"? That LSQ is recommended for GPS-only and CS for multi-GNSS tomography? If yes, please reformulate the sentence since it is not fully clear and can be confusing for the reader.

- P12 Figure 5: increase the font size of all the text in the figure (especially the text with "improvements" information)

- P12L15: could you please namely repeat here which of your tested number of stations corresponds to the rule of thumb of Champollion et al. (2004)?

- P13L1: I would rather write that LSQ seems to be not as sensitive on number of signal directions as CS than provide any "recommendation"

- P13L8: I am sure further tests should also be realized for various weather conditions and for various vertical distributions of vertical layers (changing total number of vertical layers and/or their heights) - please see my major comments in this regard.

- P13L29: Could you please briefly inform the reader why "Even in the case of a very high number of observations, the tomographic system cannot be solved in a pure data-driven way."?

- P14L6: I struggle to understand what you want to say with your last paragraph. Could you please reformulate it? At least I do not understand what does the "in order to well the refractivities of as much voxels as possible" means.

References

- Bender, M., Raabe, A. Preconditions to ground based GPS water vapour tomography, Annales Geophysicae, Vol. 25, pp. 1727-1734, 2007

- Bender, M., Dick, Wickert, J., Ramatschi, M., G., Ge, M., Gendt, G., Rothacher, M., Raabe, A., Tetzlaff, G. Estimates of the information provided by GPS slant data observed in Germany regarding tomographic applications, Journal of Geohysical Research, Vol. 114, D06303, doi:10.1029/2008JD011008, 2009

- Bender, M., Stosius, R., Zus, F., Dick, G., Wickert, J., Raabe, A. GNSS water vapour tomography – Expected improvements by combining GPS, GLONASS and Galileo observations, Advances in Space Research, Vol. 47, Issue 5, pp. 886-897, 2011

- Ding, N., Zhang, S., Wu, S., Wang, X., Kealy, A., and Zhang, K.: A new approach for GNSS tomography from a few GNSS stations, Atmos. Meas. Tech., 11, 3511–3522, doi:10.5194/amt-11-3511-2018, 2018

- Seidel, D. J. Water Vapor: Distribution and Trends. The Earth System: Physical and Chemical Dimensions of Global Environmental Change, John Wiley & Sons, Ltd, 2002

- Rohm, W., Zhang, K., Bosy, J. Limited constraint, robust Kalman filtering for GNSS troposphere tomography, Atmos. Meas. Tech., 7, 1475–1486, doi:10.5194/amt-7-1475-2014, 2014

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