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Interactive comment on "Sensitivity of GNSS tropospheric gradients to processing options" *by* Michal Kačmařík et al.

Anonymous Referee #2

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General Comments

The manuscript evaluates eight different approaches when estimating horizontal gradients in the atmospheric refractive index using signals from two GNSS, namely GPS and GLONASS. As far as I know the content is unique and provides new knowledge, but it also raise questions that I think shall be addressed.

Most important, I think, is the long section with the Conclusions. My interpretation is that the present version has the form of a summary of the results, rather then what is your message to the community on how to handle tropospheric gradients. My conclusion is that it does not really matter which of the different processing option that are chosen given the data that you have studied (excluding the near real time and real time solutions, as expected). Also the small impact of adding GLONASS data may be an

issue to raise for further investigations, possibly related to a higher temporal resolution of the estimated gradients.

Another important question is to what extent your conclusions holds during more general circumstances, because it seems as you have selected the two most extreme months for the benchmark data set. It is of course a lot of work to address this question and give a reliable answer, but it does not prevent you from an initiated discussion in the present manuscript.

An overall question is that I would like to see a more critical discussion related to the numerical weather prediction models. First of all their resolution is poor, given that probably most of the large gradients occur in the atmospheric boundary layer. For example, for an elevation angle of 3° the propagation path at the height of 500 m will be approximately 10 km horizontally from the ground-based reference station. That corresponds to the resolution of the limited area model (WRF). One possibility to investigate the scale (temporal as well as spatial) of the gradients is to use the WVR data mentioned in Section 2.1. Since you mention that these data exist the reader will wonder why you do not use them for an assessment, even if the WVR data only exist at a couple of sites.

In terms of how to present your results, I find that your maps in many of your figures give excellent pictures of the systematic spatial variability at specific time epochs. However, I miss examples showing the temporal variability of the gradients over a longer time period that, for example, can give information on for how long time does a large gradient exist and how frequent are the very large gradients.

Specific comments

In the abstract, in Section 2.1, and in the conclusions, you mention observations from 430 GNSS reference stations. It is misleading because as far as I understand the study use data from 243 stations only. This is stated in Section 3. Perhaps the results presented in Section 4 are based on 430 stations? In any case, this issue can be explained in a better way.

I do not understand Figure 1. I assume that one data point represents one observation from each one of the 243 (or all of the 430?) stations towards each visible (GPS and GLONASS?) satellite? It is stated that it "shows the fractional contribution of the tropospheric gradients". A fractional measure has the unit percent, ppm (or similar), but the units are in mm? Why is the figure included? Even though I did not understand it it did not stop me from reading (and understanding, I hope) the rest of the manuscript. I think that Figure 1 can be removed or otherwise explained more clearly. Furthermore, as I understand, the figure displays results from your analysis, and if you think these results are important you can move the figure to one of the existing result sections (or a new additional one).

In the first paragraph of Section 3 you say that the GNSS gradients are updated every 5 minutes, the WRF model every hour, and the ERA model every 3rd hour. Then you say that the GNSS - NWM comparisons are done every 3 hours. This raise two questions: (1) How did you calculate the GNSS values to be compared to the NWM models (averaging or the actual values at the time epochs given in the NWM time series)? I assume it depends on what is represented by the values in the NWM models. (2) Why not use also the higher temporal resolution available from the WRF model?

When you derive the gradients from the numerical weather models you use a ray tracing method down to elevation angles of 3 degrees. It could then be expected that you find the best agreement when comparing to the GNSS gradients estimated including observations down to an elevation angle of 3°. I wonder if you can answer the question: if the ray tracing of the numerical weather models would have stopped at an elevation angle of 7°, would then the GNSS-based gradients, using observations down to 7°, be the solution with the best agreement?

Technical Corrections

page 1, line 26: vapor? American English, although Ann. Geophys. is a European

journal?

page 1, line 27: numerical weather models -> Numerical Weather Models

page 4, line 13: FLT is a strange acronym for "Kalman filter in RT solutions". Also the acronym SMT is difficult to relate to an expression? I cannot find a definition in the manuscript.

page 4, line 20: "Three additional solutions" are these not the same three solutions that are mentioned in the previous sentence. If so they are not "additional".

page 5, line 9: I assume it shall read (1/sin(ele))ˆ2 ? You say that all variants used this weighting, but it is no longer true in Section 4 where other weighting schemes are investigated.

References: I am not sure how important it is for Ann. Geophys. For most of the journals you do not use the common abbreviations, e.g. Journal of Geophysical Research -> J. Geophys. Res. and Geophysical Research Letters -> Geophys. Res. Lett.

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