Author's response on review comments on *Sensitivity of GNSS tropospheric gradients to processing options* by Kačmařík et al.

Reviews are given in standard black text, author's responses in red italics.

Anonymous Referee #1

* General comments *

This paper investigates the estimation and modeling of GNSS tropospheric gradients from a benchmark dataset set up for the COST GNSS4SWEC project. Different analysis strategies are evaluated (gradient mapping function, GNSS constellation, cutoff angle, satellites orbit and clock latency: PP vs RT, data weighting) by cross comparisons and also compared with respect to NWP model retrievals. The results are pretty conclusive; comparisons of tropospheric gradient maps are noteworthy (except for some RT cases). PP analysis agree. Positive impact of low elevation observations and multi-constellation is observed. RT analysis induces increase of standard deviations wrt NWP models. Systematic differences induced by the modeling of elevation dependency of gradients (mapping functions) are also observed; they may be reduced by the use of an observation elevation weighting. Some recommendations about the use of gradient mapping-function are then expressed according these results.

This paper is very interesting, clear, well organized and also well written. References are relevant and appropriate (and also well formatted). I recommend the editor to accept the papers with minor revisions according to the following specific comments and technical corrections.

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* Specific comments *

.p03/l13: is it not hazardous to include post-fit residuals into STD formulation? PFRs represent mis-modeling of troposphere, but also for antenna mis calibration, multipath, liquid water, unmodeled solid earth displacements, etc.

We agree and we removed post-fit residuals term from the formula and from the text.

.p03/l30: why do not describe further the tilted mapping function as BS and CH?

We now provide a formula for the tilting gradient mapping function as for BS or CH together with a reference to Meindl et al. (2004).

.p03/l33: in my opinion "Gn*cos(a)+Ge*sin(a)" is not "the projection of the horizontal gradient vector in the direction of the individual satellites": it has to be multiplied by mfg(e), otherwise it is the projection onto zenith of horizontal gradient magnitude.

The projection is just in the horizontal plane, thus not using the gradient mapping function. We substituted the word 'direction' with 'azimuth' as it was used in the figure caption.

.p03/l31-p04/l08: I wonder if figure 1 is really useful. A simple comparison of mapping functions plotted according elevation will highlight the maximum values of each mf. The right part is shortly described in text, but it is not used to support any statements. Moreover, the black dots (for a single epoch, 20:30UTC) do not help to support any statements either. Maybe you could just replace this figure by a mfg comparison.

We decided to remove the dependence on elevation angle and black dots for a single epoch. We then kept the original left figures when directly comparing mapping factors (range in x-axis) and ranges of gradients (scatters in y-axis) for three gradient mapping functions. Based on this figure, we could focus on the two extreme mfg in the following part.

.p03/l15: why did you not use the tilted mfg? I think that its use is not essential since it takes values between BS and CH, but you have to mention it clearly (as a consequence of figure 1).

Corrected. We have added a conclusion about tilting from Figure 1, meaning we could further focus on BS and CH only.

.p06/l5-p06/l10: as you mention, gradients retrieved from NWP depends on mfg (BS or CH). Why do not use your ray-tracing algorithm to compute gradient with their closed form expression depending on NS and EW horizontal gradient of refractivity? (See Davis et al., 1993, RS)

We calculate the tropospheric gradients from ray-traced delays by least square adjustment to be as close as possible to the method applied in the GNSS analyses for parameters estimation.

.p06-p07: Did the gradient modeling affect the estimation of positions? Maybe you could complete Table2 with comparisons of position (height?) repeatability?

There were already published some studies which dealt with positioning changes related to tropospheric gradients estimation which we cite in our paper. Since our focus is only the quality of tropospheric gradients, we would rather not provide results for positioning.

.p07/Table2: I think it is important to have an overview of gradient time series in order to understand the comparisons. Especially, unlike for ZTD we do not have many ideas about gradients magnitude (maybe some ideas from figure 1): is a 0.01 mm bias significant? and a 0.76 mm stdev? These values may be put into perspectives with gradient magnitude.

We do not provide gradient time series in this paper, the reader can find them i.e. in the cited publication Li et al. (2015). However, we added a paragraph into the section 3.1 to describe typical values of gradient components under standard and severe weather conditions and we provide information on ZTD rate of change due to tropospheric gradient with an increasing distance from the receiver. The bias of 0.01 mm is not significant in respect to typical values of gradient components and also according to estimated standard deviation of comparisons.

.p07/Table2: I wonder if the computation of correlation will be helpful to investigate the comparisons. A linear fit?

We now provide linear correlation coefficients in table 2 to and table 3 and we updated the text in this regards.

.p07/Table3: same comments as for table2. Maybe the computation of correlation or linear fit will be more relevant here.

See answer for the previous comment.

.p10/l13: the RT3GxCH3 do not use Glonass satellite. Why will this solution be affected by Glonass RT corrections?

You are right, it can't be affected. The manuscript was updated.

.p11/l20: Are there any other indications to help to identify these two outlier stations? ZTD, position estimates? Formal errors?

We checked ZTDs and formal errors for ZTDs and tropospheric gradients, but we haven't found anything extraordinary for these two outlier stations. We have not checked their coordinates so far.

.p13/l3: I think that figure 4 may be described more deeply. First by comparing the impact of the two OEW, then the combined impact of OEW and mfg.

More description is provided in updated manuscript according to the proposed way.

.p13/l5-12: I do not succeed in fully understanding Figure 5 and your remarks related to it (see also next comments). It should be clarified.

We updated the corresponding paragraph, hopefully improving its clarity.

.p13/l5: Are differences cumulated during the full day?

We substituted 'cumulated differences during the full day' with 'mean differences calculated over the full day', and we hope it is more precise and clear in understanding.

.p13/l6: "In this case ... ": I am not sure to understand: did you mean that this figure helps to highlight that systematic differences affect both magnitude and direction?

No. We just noted that both the magnitude and direction of an estimated gradient resulted in different systematic errors when decomposed into North/East gradient components. The paragraph has been reworded.

.p13/l7-l9: Could you explain these two sentences: "A positive difference... points to east" & "Negative... opposite directions". I do not understand (1) how can differences remain positive if you compute A minus with B>A for example (2) how negative values are obtained when gradients point to the opposite direction. This is maybe trivial, but I do not succeed in getting it!

Indeed, the description was reversed when considering B>A. It seems that we misattributed B and A after some changes in the representation of gradient differences. Thank you very much for careful reading!

.p13/l10: The decrease of maximum systematic differences with OEW SINEL2 is not obvious.

When displaying now all the OEW schemes, we hope the decrease of systematic errors in Figure 4 and 5 become more visible.

.p13/l11: Why do not show other weighting, especially SINEL4 which is mentioned to reduce systematic differences?

Initially, we thought it's too much showing all of them, in particularly when SINEL1/SINEL2 is the most commonly used OEW in analyses. We added now all of them for a complete comparison.

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* Technical corrections *

I recommend the authors to improve legibility of figures (by using a better resolution)? I also recommend the use of an equation editor for mathematical expressions.

In the updated manuscript version all the equations are created using an equation editor.

Anonymous Referee #2

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.

From our point of view, we provide a recommendation to the user everywhere we think it can be given based on our own results: 1, we recommend using observations from very low elevation angles to get better gradients (this was already shown also in paper by Meindl et al. (2004) which we cite). 2, we find a small positive impact coming from adding GLONASS in our processing (it can be however different when using other products with satellite ephemerides and clock error corrections, different weighting of observations from various GNSS, etc., and some other investigations related to multi-GNSS data processing in general will follow). 3, we present the penalty in quality of tropospheric gradients from real-time processing and we show that this penalty is mainly related to the quality of used products. 4, we show that selection of gradient mapping function does not affect general quality of estimated tropospheric gradients but their magnitudes (one has to be careful then with comparing gradients from various sources due to existing systematic differences).

We updated the conclusion section of the paper to address your comment.

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.

We based our analyses on a data set from wet spring/summer season when the gradients could provide a valuable information for meteorological applications. Although the time period covers some severe weather events, it also contains a lot of days with standard weather conditions with tropospheric gradients close to zero. So, the results should provide a good overview on the situation in Europe during the warmer part of the year. On the other hand, we agree that new studies based on different GNSS software a data sets should be done to strengthen and confirm our results.

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.

We split our reaction into two parts:

1, we would be very careful about the statement that large gradients occur in the atmospheric boundary layer. Please see i.e. a paper from Elosegui et al. (1999). According to his findings GNSS tropospheric gradients are more sensitive to tropospheric features at larger heights, in different words – i.e. the same type of tropospheric feature at the height of 3 km would cause a larger gradient value than while occurring at the height of 0.5 km. And also sometimes (even during not winter season) a hydrostatic gradient can prevail in total tropospheric gradient estimated by GNSS. And these hydrostatic gradients are related to large scale (up to several hundreds of km) features, not to local station asymmetry. Of course, we are aware of limitations of NWMs we use in this study and we also state them in the paper. On the other hand, this is the first time when a NWM with a 10 km horizontal resolution was used for comparisons with GNSS results and there is a visible increase of its gradient magnitudes compared to outputs of global NWMs with 1° horizontal resolution used in Douša et al. (2016).

2, the usage of WVR for this study is problematic from several reasons: a) it is available only for a single station (POTS, Germany) for the benchmark campaign; b) WVR measures IWV therefore it can deliver only wet delay gradient, a hydrostatic gradient would need to be added from an external source (NWM) to get a total gradient which is delivered by GNSS; c) data quality of WVR observations at elevation angles below approximately 20-25° is generally poor (see i.e. Kačmařík et al., 2017, AMT).

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.

In this paper we focused on GNSS data processing and we provided only few examples of maps of tropospheric gradients to demonstrate some systematic effects. Currently we are working on another study where we utilize mainly these maps including longer time series so we are not going to extend this paper in this direction. The duration of large gradient presence is simply given by the prevailing meteorological situation and therefore is strongly variable. Usually it ranges between several tens of minutes and several hours.

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.

In total, benchmark data set contains data from 430 GNSS stations which were processed for all introduced variants of solution. However, from given reasons the statistics presented in section 3 are based only on a subset of 243 stations. To make it more clear for the reader we updated the manuscript in the introduction par of section 3 and in the conclusion section.

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 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).

You are right, we removed the word 'fractional' and kept just 'contribution'. Horizontal gradients, projected into azimuths of individual satellites, are expressed in mm. Figure 1 shows gradients from all 430 stations. Based on comments from the Reviewer #1, we removed one of the plots, improved the text description and concluded with further study of extreme mfg only (BS and CH).

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?

Newly we base our GNSS versus NWM comparisons on 1-hour interval, all the results in the paper were updated accordingly to that and they changed insignificantly. To answer the question, we used GNSS results from the time epochs of NWM outputs for which they were estimated in the GNSS data processing, we have not done any averaging. NWM output is always given for a specific time epoch.

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?

To answer your question, we derived a new complete set of tropospheric gradients from both ERA5 and WRF models using a 7° cut-off angle for the ray-tracing. Firstly, we compared gradients from such two versions of solution from one NWM and found a difference at the level of 0.1 mm or below 0.1 mm for RMSE. Secondly, we compared these outputs with GNSS solutions and we always found a better agreement between any evaluated GNSS variant of solution and the NWM solution based on the 3° cut-off angle – in terms of bias, standard deviation and correlation coefficient. We comment on this in section 3.2 in the updated version of the manuscript.

We interpret this situation as follows: GNSS use only a limited set of observations and have to deal with a set of (unknown) parameters like receiver position, troposphere, satellite clock error, etc. And errors in estimation of some parameter(s) can influence other parameter(s). Observations from very low elevation angles include a strong influence from tropospheric delay and therefore can help to accurately estimate it. On the other hand, the applied NWM ray-tracing technique uses a much larger number of "observations" which are not affected by other effects or observation noise to estimate the tropospheric gradients and is therefore not dependent on observation elevation weighting or applied cut-off angle.

Technical Corrections

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

Corrected.

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

Corrected.

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.

FLT means a filter, SMT a smoother. We do not find these acronyms as problematic, therefore we kept them in the paper.

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".

The word additional was deleted.

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.

Formula was corrected to avoid misunderstanding. All variants of solution introduced in section 2.2 (table 1) and evaluated in section 3 used this weighting scheme. We don't want to confuse the reader with extra information on elevation weighting in section 2, therefore we keep the sentence as it was.

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.

Corrected, manuscript was updated.