Editor comments on manuscript amt-2018-318-version 5 “On the information content in linear horizontal delay gradients estimated from space geodesy observations” by Gunnar Elgered and co-authors.

Thank you for providing a revised manuscript and answers to my comments.

The two major points I mentioned in the previous review have been clarified but the text still needs be clarified to make these two points fully understandable. A few other points need also to be further discussed and explained. Below are my comments and suggestions for revision.

Please note also that the reference numbering in the revised manuscript did not work properly (all references in the text appear as ??). In order to avoid publication delays, be careful that this is corrected in the next version.

Section 5.2 on the discussion of the difference of amplitude of gradients from GPS and WVR

P24L290-294: suggested revision:

“(2) The WVR gradients for one 15-minute period do not depend on earlier or later estimates whereas the GPS gradients are estimated using constraints on the variability. A constraint has a similar impact as a low-pass filter (peaks with a short duration~~, requiring rapid changes,~~ will be reduced). ~~This is a valid argument for the mean gradient amplitude, but not for the mean values of the individual east and north components, because they are expected to be very close to zero regardless of the value of the constraint.~~”

=> the variability of the GPS estimates is actually controlled by the random walk parameter. A value of 0.3 mm/sqrt(h) is given in Table 1. How was this value chosen? Based on the results of this study would you suggest to increase this value? Did you test this? According to the paper by Gradinarski et al., 2000, increasing the RW parameters doesn’t improve the RMSD. A short discussion of this point might be relevant at this stage.

P24L295-302: suggested revision:

“(3) The fact that the WVR and the GPS gradients are computed for different elevation cutoff angles has two possible impacts: (i) the larger volume sensed by GPS (with a 3 degrees cutoff angle) includes different air masses and introduces an averaging effect that reduces the variability and mean amplitude of gradients, similar to averaging over longer time periods as shown in Table 6; (ii) the higher cutoff angle of the WVR observations (20 degrees) results in larger formal errors, and thus larger variability and larger gradient amplitudes. Table 10 shows the impact of changing the elevation cutoff angle from the GPS observations for ONSA and ONS1 over the 4-year period 2013–2016.”

P24L303-304: suggested revision:

“We conclude that the use of different constraints and cutoff angles are the likely explanations for the differences in gradient amplitudes estimated from GPS and WVR data but cannot based on these results determine their relative importance.”

=> Add the standard deviation of gradient amplitude in Table 10 to support the idea that variability is increasing with cutoff angle. Add also the WVR results (mean and standard deviation of gradient amplitude) to be compared to the GPS results at 20°.

Figure 10: add the information that the GPS results are for 3° and no elevation dependent weighting. Add which time resolution is used to compute the monthly mean and SD.

Section 4.3 on the trends

Thank you for proving a graphical explanation of your reasoning in the answer. Now I understand what you were meaning. So yes, I agree that the mean gradient components can have no trends while their mean absolute value can have a trend. However, this situation is purely theoretical and not based on observation (at least not documented in the manuscript). It is also a bit misleading because it appears as a contradiction where there is none. Indeed, since the gradient is a vector, it is important to check mainly its amplitude and direction rather than its components because amplitude and direction have more physical sense. But actually, I think that this discussion is unnecessary in the manuscript.

Moreover, I have a concern with the two following sentences:

P20L225: “A positive trend corresponds to a larger variability” => this is incorrect if you consider a trend in the variability of the direction of the gradient vector with constant amplitude. And a positive trend can also be simply due to an increase in amplitude without a change in direction, i.e. not only due to larger variability.

P20L227: “A trend of larger east gradients can be balanced by a similar trend in larger west gradients”. This sentence sounds awkward. A directional trend would be either eastward or westward but not both… I think what you mean is that more positive values (eastward gradient) can be balanced by more negative values (westward gradient) resulting in a null mean value, but this situation is rather speculative and unnecessary.

=> As a consequence, I recommend that you remove the sentences between L223 and L228 “An estimated gradient has a direction… but a trend in the gradient amplitude.”

The rest of the Section 4.3 should also be revised because both the GPS and ECMWF trends have large uncertainties. The GPS series are not homogenized and the ECMWF product used in this study is probably elaborated from operational ECMWF data (i.e. not homogeneous) and has some inherent limitations discussed by Kacmarik et al., 2018 (see the comparison of the gradient product by Böhm and Schuh, 2007, and other ray-traced products in their final publication). Landskron, D. & Böhm (2018) recommend also to use their new refined discrete gradient product computed from ERA-Interim reanalysis instead of the earlier product by Böhm and Schuh (2007).

Landskron, D. & Böhm, J. J Geod (2018) 92: 1387. https://doi.org/10.1007/s00190-018-1127-1

Kačmařík, M., Douša, J., Zus, F., Václavovic, P., Balidakis, K., Dick, G., and Wickert, J.: Sensitivity of GNSS tropospheric gradients to processing options, Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2018-93, in review, 2018.

P20L235-236: “Nevertheless, to study long time series of estimated gradients is motivated by the monitoring of the quality of the data from a GPS station.” This statement should be made earlier (e.g. at beginning of the sub-section or in the Introduction). If made in this sub-section, it should be better explained in the context of trend analysis (e.g. hardware malfunctioning can produce drifts or breaks in gradient series and thus artificial trends).

As suggested in the previous review, this sub-section could be totally removed, or at least limited to the analysis of trends in total GPS gradients (not using this particular ECMWF gradient product for which the uncertainty may not be adapted for trend estimation).

Section 4.2 on the GPS and ECMWF results

The differences between the GPS and ECMWF gradients in Table 6 and 7 should be further discussed, not just the differences between stations. There are large differences in the mean gradients and a systematic underestimation of the variability in the ECMWF data by a factor of 2.

The quality of the ECWMF gradient product by Böhm and Schuh (2007) should be better stated (see below) or assessed/discussed in this section. This could be added as a specific objective of this study.

Section 3.4 on ECMWF data

There are different methods used to compute horizontal gradients from ECMWF data and several products exist. The product used here is usually referred to as LHG (linear horizontal gradients) by Böhm and Schuh (2007). Note that it has been replaced by a refined discrete gradient product using ERA-Interim reanalysis (Landskron and Böhm, 2018). Other ray-tracing methods used to compute gradients from NWM data were developed by Zus et al., 2012, and Zus et al., 2015, and are discussed in Kačmařík et al., 2018 (ANGEO, in review).

Zus, F., Bender, M., Deng, Z., Dick, G., Heise, S., Shang-Guan, M. and Wickert, J.: A methodology to compute GPS slant 15 total delays in a numerical weather model, Radio Science, 47, RS2018, doi:10.1029/2011RS004853, 2012.

Zus, F., Dick, G., Heise, S. and Wickert, J.: A forward operator and its adjoint for GPS slant total delays, Radio Science, 50, 393–405, doi: 10.1002/2014RS005584, 2015.

Minor comments on the revised manuscript (version 5):

P2L35-38: the question about the seasonal changes of gradients was previously studied by Koulali et al., 2012, please cite here.

“if these make sense given present knowledge about the meteorological conditions” change to “if they can be explained by the influence of regional-scale weather systems”.

The scientific question leading to the “comparison of GPS gradients with high temporal resolution” is not clearly formulated. Why or for which applications is a 15-minute resolution more interesting/useful than a 6-hour resolution?

Add a scientific question regarding the assessment of the ECMWF LHG data by comparison with GPS?

P18L213: the “SD” of the GPS gradients are larger … (add SD and maybe define the acronym here if used first time).

P18L215 and 216: “significantly smaller”: how is the significance measured? “comparable”: what is the limit used to distinguish between smaller and comparable? Based on the values in Table 6, the ratio of daily values for KIR0 and SPT0 is 0.32/0.38 = 0.84, so KIR0 is 16% smaller, but the ratio of monthly values is 0.13/0.16 = 0.81, so KIR0 is 19% smaller in that case, so not comparable… Please revise the statements and refer to the numbers in the Table(s).

P18L217-218: “at this level the hydrostatic gradient and other effects, e.g. signal multipath effects, become important.” The SD of monthly hydrostatic and wet gradients over 4 years are given in Fig. 8. They should be quoted here and used to estimate the contribution of both components to the total gradient (if they are valid for the 11-y period, if not compute the SD for 11-year here).

Why would “signal multipath effects” by larger in the monthly SD than in the 6h and daily data?

P21L252-255: Reformulate as: “The GPS wet gradients for ONSA and ONS1 are computed by subtracting the hydrostatic gradients from ECMWF, linearly interpolated to match the time epochs of the GPS gradients, from the total GPS gradients.”

P21L262 “This confirms the results presented by Kacmarík et al. (2018) using a GNSS station network in central Europe.” Be more specific or don’t cite Kacmarík et al. (2018) here because they did not use a WVR.

P22L277: “the correlation is here reduced to around 0.6” rather 0.66 at 3° (average of 0.64 and 0.68).

P33L387-388: “gradients calculated from meteorological analyses of the ECMWF” refer to the LHG product used in this study as the results may depend on the NWM model, the computation method and mapping function used in the method as discussed by Kačmařík et al., 2018.

P33L389-391: “No significant long-term trends were detected for the GPS gradients. If small gradient trends are detected in the future, we recommend to critically assess if they could be caused by station problems or confirmed by a nearby (or even collocated) station.” I think these sentences should be made less general because the study was conducted in a limited area. It cannot be said that there are no detectable trends in other parts of the world, though it is true in general that when a trend is detected the time series be visualised and data inspected to check the nature of the trend…

P33L392-393: quantify “most of the variability”

P33L393: “implies a better agreement” => based on the correlation coefficients mainly (the SD of differences may actually decrease because the GPS gradients at 3° are of smaller amplitude).

P33L394: Kačmařík et al., 2018, did not use a WVR, and they only compared 3° and 7° solutions. A thorough optimization wrt cutoff angle might require testing more values.

P33L395: “Related to this is…” suggests that it explains the above results though it is the opposite! Change to: “despite…”

P33L396…

“We interpret this result as the combined impact of two possible causes” => “We interpret this difference as the result of two combined effects”

“(1) the decrease of mean amplitude and variability at the lower cutoff angle results from the averaging of a larger air mass (similar to averaging over longer periods)”

“(2) the increase of mean amplitude and variability at the higher cutoff angle results from the increase of uncertainty and thus larger scatter in the estimates”.