Editor comments on manuscript amt-2018-318-version 4 “On the information content in linear horizontal delay gradients estimated from space geodesy observations” by Gunnar Elgered and co-authors.

I would like to congratulate the authors for the implementation of the corrections and suggestions provided in my previous report and their careful review and revision of the text and figures. I have two concerns related to some new results included in the revised manuscript and a few additional minor comments.

Table 11 shows the mean values of gradient amplitudes from two GPS stations for 3 different cutoff angles (3°, 10°, and 20°). The result is quite striking. The mean amplitude is clearly increasing when the cutoff is increasing, or the other way round, the mean amplitude is decreasing when the cutoff is decreasing. You adopt the 2nd point of view and speculate that the decrease is due to averaging of a larger air-mass, a result that is compared to averaging over longer time periods. I am not convinced by this explanation. I think that changing the cutoff angle has primarily an impact on the correlation between gradients and other parameters and on the accuracy of the estimated parameters. Actually, decreasing the cutoff is expected to improve the accuracy and provide more realistic gradients estimates. I think this is supported by the enhanced agreement with WVR results (the 3° GPS solution agrees better with WVR results). On the other hand, increasing the cutoff to 20° seems rather an unfavourable situation for estimating accurate gradients and it may be that the GPS gradient estimates are actually biased in this case, as well as the WVR gradient maybe? The increased standard deviation of differences and decreased correlation coefficients between GPS and WVR indeed support the idea that GPS and WVR agree less well in that case, i.e. the uncertainty in the GPS gradients is larger as also predicted by the larger formal error. I think that this effect is larger than the effect of sensing different air-masses at lower elevations. At least both points of view should be discussed and if you want to maintain your idea, more evidence should be provided to support it.

My second concern is with your statement that there can be a trend in amplitude without a trend in components (also noted in the previous review). Since the gradient is a vector, if its length increases, the components necessarily increase. This statement should be removed or clarified if I didn’t get what you mean.

Below are a few additional minor suggestions.

Though you slightly extended the last paragraph describing the organisation of the paper, I think there is still a paragraph missing in the Introduction on the motivation of this work and the rationale of the study scenario. Please add a short paragraph between line 10 and 11.

P2L26-28: Meindl et al. (2004) discussed the global north-south temperature gradient. In your work, regional high and low pressure systems are more important. I suggest to remove the sentence related to work of Meindl et al. (2004), and rewrite this part as follows: “Hydrostatic gradients are usually dominated by pressure gradients and exist mainly over regional scales (e.g. persistent high and low pressure systems) and synoptic scale (e.g. weather systems). For the area of interest in this study we specifically mention the Icelandic low pressure system…”

P3L8: linear in what? Do you mean that the processes cannot be represented by gradients that are a linear function of time?

PL21: Add “While total gradients are estimated, they can be interpreted as the sum of hydrostatic and wet components as well. In the following we will subtract the hydrostatic component computed from ECMWF from the total GPS gradient to get the GPS wet gradient.”

P12: Are the ECMWF gradients computed from operational analyses or a reanalysis? Note that operational analyses should not be used to analyse long time series because of changes in the model and assimilation system over time.

P13L8-9 (line numbering in the manuscript doesn't match) why is the formal error for the north component larger?

Figure 9: add some comments on the year-to-year variability in the results and differences between GPS and ECMWF.

P17L11: The sentence suggests that ECMWF gradients are more accurate if used to validate GPS gradients. I suggest to reformulate as "The correlations seen in all cases confirm that a consistent atmospheric signal in terms of gradients is detected by the GPS observations and ECMWF analyses."

P17L14: rather than being better modelled by the ECMWF analyses, I think that larger scale features agree better because the representativeness differences between the gridded model fields and GPS point observations are smaller.

P17L25: "…but the relative differences between the sites" it is not clear what relative differences are meant, suggest to be more specific: "GPS gradients are larger by a factor of ~1.5 and this factor is roughly the same for all sites."

P17 last sentence: "e.g. instrumental" can you be more specific?

P19: I think the section on trend results is not relevant. First, it poses the problem of the homogeneity of the data (both GPS and ECMWF). Second, the values reported in Table 8 are confirmed in the text to be statistically insignificant. Third, the main interest of inspecting trends is said to be for the detection of hardware problems. This is well illustrated in Dousa et al., 2017, but not here. I suggest to remove this section and Table 8 and replace it with one or two summarizing sentences in the Conclusion section.

Table 11: over which period of time are the mean values computed?

P23: Section 5.2: discussion of the factors that can cause a difference in GPS and WVR gradient amplitudes should be revised (see above).

I don't understand point 2) Constraints on the variability should not impact the mean amplitude.

At bottom of page "Before studying the correlation…" remove this sentence as GPS and WVR were already compared in the previous section.

P25L13: As expected => As seen previously from total gradients…

Figure 12: use the same range for x and y – axis in all plots.

P30: remove the last sentence (it is not demonstrated neither in this paper nor in general that the GPS gradients can help to validate high resolution NWM models). Also P32 remove "both for evaluation of the performance of the model and".

P32. First sentence: the study doesn't really explain the GPS gradients based on meteorological phenomena, but rather presents a statistical analysis and comparison with other data sources. Please correct the sentence.

“horizontal gradients” => GPS gradients

Revise the interpretation of impact of cutoff on GPS gradients

“simultaneously estimated” => remove “simultaneously”