Response to referees' comments on:
On the information content in linear horizontal gradients estimated from space geodesy observations

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Introduction

We appreciate the referees' comments and for the time spent on the manuscript, not only for pointing out where clarifications were needed, and identifying a couple of mistakes, but also for suggesting additional comparisons that we think have made it possible to be more specific in some cases, and less specific in other cases. We first describe the major overall changes in the revised manuscript and then we give responses to the individual comments from the referees.

Overall changes

The original manuscript did not have any equations included. Basic equations for the atmospheric refractivity and the type of gradients assessed in the study are now added in Section 2.

The GPS data from the Onsala site during 2013-2016 were reprocessed using three different elevation cutoff angles, alternative mapping functions for the hydrostatic and wet delays, and with and without elevation dependent weighting. As pointed out by both Referees #1 and #2 the discussion paper by Kacmarik et al. (2018) in AMT had found that an elevation angle cutoff at 3° gave the best agreement for estimated gradients. We were now able to confirm this using water vapour radiometer (WVR) data. This means that the results in Section 5.1 are to a large extent new and the section is expanded, e.g. with two new Tables 6 and 7, and the new Figure 11, showing the total gradient sizes from GPS and WVR and the strong correlation between monthly means of gradient size and ZWD (these issues were suggested to discuss in the referees' comments). As suggested by Referee #2, Figure 12 in the original manuscript was removed.

Also gradients estimated from the WVR data were included in the VLBI comparison in Section 5.2 (for completeness and to confirm to what extent gradients seen by the space geodetic techniques, VLBI and GPS, were of atmospheric origin). We removed the old Figure 15, that showed correlation between VLBI and GPS gradients. We think it caused more confusion than understanding. When we now also added the WVR data it seemed reasonable to summarize these correlation coefficients in a table (Table 8 in the revised manuscript).

When WVR data were added to the 15-day long period of the CONT14 campaign we also included the new Figure 16 of the ECMWF gradients in order to show the impact (which is really small in terms of variability) of the hydrostatic gradients added to the WVR wet gradients. The variability seen in the WVR data during the CONT14 also motivated us to go into more detail presenting the zenith wet delays (new Figures 17 and 18) allowing us to discuss the fact that gradients tend to occur during changes of the air masses above the site.

In the following we deal with each referee's comments one by one. The text is colour coded roughly as follows:
Referees' comments are in black font.
Authors' general responses are in blue text and changes in the manuscript in red text.

Referee #1:

The study of the horizontal variability of the atmosphere is currently of great interest. Linear horizontal delay gradients are considered advanced GNSS meteorology products and it has been proved that they are a powerful tool to identify problems with GNSS data tracking. Although not yet assimilated into NWP models, they are fundamental for the reconstruction of the slant delay and, in turn, in the 3D water vapour fields derived by tomographic inversion of GNSS based slants. The analysis of the causes of the time variability on different time scales, from months to minutes, reported in the manuscript adds new insights in the research area of these advanced GNSS meteorology products. In the manuscript, tropospheric gradients estimated from GPS observations are evaluated with respect to independent techniques as WVR and VLBI and independent data as ECMWF in order to assess their quality. I think it would be interesting in future to repeat the same kind of analysis in other regions. However, I would raise the following issues, which have to be clarified prior to the publication.

1. Mapping functions: GPS and VLBI data are analysed using different mapping functions: VMF1 for GPS and Niell for VLBI. No information about the gradient mapping function is given. I guess that in GPS data processing Bar-Sever et al. (1998) gradient mapping function is used, while in VLBI Chen and Herring (1997) gradient mapping function is applied. Kacmarik et al. (2018) recommend to agree on the gradient mapping function when tropospheric gradients derived from various sources are to be compared, since a systematic effect up to 0.3 mm is observed between Bar-Sever et al. (1998) and Chen and Herring (1997) gradient mapping functions. Having this in mind, I think that information on the gradients mapping function has to be provided in the manuscript and properly discussed.

It is correct that the Bar-Sever gradient mapping function is used in all GPS solutions and that the Chen and Herring mapping function was used in the VLBI solution.

This is now clearly stated in Section 3.

We are aware of that the estimated amplitude of the gradients depend on the gradient mapping function chosen.

In the revised paper, we primarily investigated the impact of several different elevation cutoff angles on the resulting gradients. Mapping functions for gradients were not changed.

2. Elevation cut-off: GPS data are processed at 10° and 20° elevation cut-off angle, while no info is provided for VLBI. Kacmarik et al. (2018) obtained better results using 3° elevation cut-off angle and GPS+GLONASS data. I recommend to process at least 1 GPS station with 3° elevation cut-off angle and evaluate the results. Should geodetic data processed with different cut-off angles depending on the application and on the tropospheric parameter of interest (ZTD or gradient)? A comment on this is really appreciated. The manuscript is within the scope of this special issue.

Kaˇcmaˇrík et al (2018), Sensitivity of GNSS tropospheric gradients to processing options, Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2018-93

We reprocessed the data obtained from ONSA and ONS1 using elevation cutoff angles of 3°, 10°, and 20° from 2013 to 2016. The resulting gradients were compared to the ones obtained by primarily the WVR in Section 5.1, but the 3° solution also to VLBI gradient results in Section 5.2.

Two new tables with results were added and a short discussion is also added in the Conclusions about an optimum elevation cutoff angle.

Below specific comments.

Introduction Page 2 Line 6. I suggest adding the amount of improvement of multi-GNSS gradients compared to GPS-only gradients.

The following text is added in the revised paper "Using multi-GNSS observations, Li et al. (2015) found a significant increase in the correlation coefficients of gradient to about 0.6 when compared to ECMWF gradients, while the one for the GPS-only is usually below 0.5. In addition, they found that the RMS difference of the gradient is reduced to about 25–35 % by multi-GNSS processing."

Cause of horizontal gradient In the manuscript, the mathematical model used to describe the tropospheric path delay is not reported. This can be added in this section and the title should be changed in 'model and cause of horizontal gradient'.

Done.

Instrumentation and data Page 3 Line 11-12. Complete the sentence 'We compare ...' adding at the end '... with respect to VLBI estimates, WVR and ECMWF data' I suggest adding in the section a table summarizing the characteristics of the instruments used for the evaluation, referring to the specific sub-section for further details.

The sentence was modified, but slightly reformulated because we regard the gradients from all these sources to be estimates. When trying to design a table that would include the important characteristics from each instrument we did not end up with a solution that was sufficiently compact, so no new table was introduced.

GPS In this section, and also somewhere else in the manuscript, the term 'site' is used both referring to a local geographical area or referring to a unique geodetic marker. Please review it and use site for a local geographical area, where one or more geodetic markers are available, and station to indicate a unique geodetic marker at a site. Figure 2.

This makes sense and has been adopted in the manuscript.

You present the sky plot of GPS observation for May 12, 2014. Why did you select this specific day?

We selected this day because we have simultaneous observations from GPS and VLBI and the results for this day are further discussed in the corresponding result section.

This now explicitly pointed out in the caption to the figure.

Gradients during the CONT14 VLBI campaign Figure 14. Add mean and std of the differences (GPS-VLBI), are these values affected by the different gradient mapping function?

In the revised paper, we added Table 8 with mean and standard deviation (plus correlation coefficient) obtained when VLBI gradients are compared to GPS and WVR. Because we did not change the gradient mapping function we refer to Kacmarik et al. (2018) to point out that the estimated gradient amplitude will depend on the mapping function.

Typos

Pag.2 Line 4: delete ')' after VLBI

Corrected

Table 2 replace 'igs_1740.atx' with 'igs08_1740.atx', correct?

Corrected, also noted that igs08_1869.atx was used in the reprocessing of the 4 years of data from the Onsala site.

Pag.22 Line 20: horisontal -> horizontal?

Corrected

Referee #2:

As expressed in the title, the manuscript deals with the information content in linear horizontal tropospheric delay gradients estimated from space geodesy observations, namely GPS and VLBI. The topic of the manuscript is highly actual. In past, the tropospheric delay gradients were estimated mainly for improving horizontal positioning (coordinate repeatability), although it was not always clear that it improved the troposphere modelling as the gradient parameters are highly sensitive to other error sources affecting the data analyses. So far, the gradients were also rarely estimated within an operational troposphere monitoring because the information content was often either too noisy or too much smoothed by low temporal resolution or constraints. The situation is going to change in future when providing advanced tropospheric products on troposphere asymmetry monitoring, in particular with upcoming availability of more satellites from multi-GNSS constellations. Tropospheric delay gradients are also pre-requisite for delivering other products from GNSS such as retrieving slant delays, the reconstruction of three-dimensional refractivity field or generating severe weather event indicators. Attempts for developing assimilation techniques for GNSS-based tropospheric gradients emerged recently when it seems preferred way, compared the utilization of slant delays, due to the better production quality in (near) real-time. In this context, the manuscript contributes to a better understanding of how linear tropospheric delay gradients are able to characterize wet and hydrostatic effects of the neutral atmosphere on space geodetic observation analyses; both in actual situation or in a long-term trends and useable in geodetic, meteorological or climatology applications.

1. Adding brief introduction of the model of calculating gradients from the NWM would be helpful for discussion of results, e.g. gradient mapping function, distribution of raytracing points, elevation angle cut-off.
A description of the calculation of gradients from the NWM was added. However, it is our interpretation of the paper by Boehm and Schuh (2007) that the gradients are calculated using three vertical profiles of pressure, temperature, and humidity, without the use of a gradient mapping function, raytracing, or elevation angle cutoff.

2. Although the manuscript study a comparison of gradients, there are no information about gradient mapping functions used in estimating procedures of different techniques. Kaˇcmaˇrík et al. 2018 (submitted for discussions in ANGEO) showed that gradient mapping function could introduce systematic effects into estimated gradients. Similarly, no information about elevation-dependent weighting (if applied) was given neither for GPS nor VLBI. Please, add these information in corresponding tables or text paragraphs, and whenever useful, consider their impact in discussions as these might be more critical than the other processing settings.
In the revised paper, we added results obtained from comparisons of gradients given by different elevation cutoff angles (3°, 10°, and 20°), weighting and non-weighting, and different mapping functions (NMF and VMF1). Tables 6 and 7 and a corresponding discussion were added.

3. The differences in size of estimated gradients estimated from different techniques are not discussed. These are visible in Figure 9 between GPS and Numerical Weather Model (NWM) and in Figures 11 and 12 for GPS compared to WVR. These could be attributed to various aspects such as used gradient mapping functions, limited resolution of numerical weather model data (ECMWF), observation sampling over the sky or others. Were similar characteristics common to the other stations?
For easier reading, I would also suggest to consider splitting sections 4.2 and, optionally

5.1, into two parts with more specific subtitles for more better clarity of different comparisons, see bellow in specific comments.

The new Figure 11 helps us to discuss the (large) uncertainty in the estimated gradient amplitudes. We use ECMWF mainly to remove the hydrostatic gradients which seems to be possible to interpolate between the 6 h data points with sufficient accuracy (Li et al., 2015). The new Figures 9 and 16 showing time series of hydrostatic gradients from ECMWF also confirm the low variability during time periods of 6 h.

Section 4.2 was split into two parts. (We agree that these were two rather different topics). We did not split Section 5.1. It has changed significantly with the testing of several different GPS solutions.

Specific comments

P1L11: 15-day long continuous

Corrected

P2L4: VLBI, GPS and WVR (remove closing bracket).

Corrected

P2L12: a 4-year period

Corrected

P2L13: a 15-day long VLBI campaign

Corrected

P2L20: over long-time scales

We are not sure about this. "long" is an adjective and "time scales" are two nouns. After talking to a native English speaker we decide to keep it as it is.

No change in the manuscript.

P6Tab2: add gradient mapping function and reference frame and PCV values applied (IGS08 or IGS14?)

The Bar-Sever et al. (1998) the mapping function was used for the gradient estimation. The reference frame is IGS08. Table 2 describing the GPS data processing has been updated with this information.

P6L6: (consider modified wording) ... estimated gradients are independent in adjacent epochs ...

Instead we write: "estimated gradients are independent of the ones estimated at adjacent epochs ..."

P9L15: ... as piece-wise linear offsets ... Do you mean representation with a piecewise linear function when represented with the interval end-point offsets? Or do you mean just constant offsets for individual intervals? Please, reword or clarify.

We changed the wording to "piecewise linear continuous function"

This is also illustrated in the new Figures 14 and 17, but of course the reader may not yet have seen these figures while reading this section.

P11L5: the overall mean negative north gradients is also partly attributed to the flattening of the earth atmosphere, see Meindl et al. 2004, I suggest to add in the discussion in this paragraph.

We do not find any statement in Meindl (2004) about the flattening of the earth atmosphere. We also find that Meindl (2004) only refer to the negative north gradients as a function of latitude. Therefore, we removed the word "pressure" from our text in order to give a correct citation.

P12 Figure 9: discuss the different ranges of gradient sizes,

Gradient sizes are now discussed together with the new Figure 11.

P13L6: by long-term averaging ...
Corrected

P13L18: ... at this low humidity level ...
Corrected

P13L19: I suggest to add here new sub-sections for discussing long-term trends in gradients. I found it confusing when mixed in a single section.
Done

P13L21: a possibility ...
Corrected

P13L22: trends in the total amplitude value of the gradients (I don't understand what is meant exactly by the trend in the total amplitude value of gradients. It would be helpful to clarify it here)
The total amplitude is > 0. If the variability increases along one coordinate there will be larger gradients, both positive and negative. This can happen without a net increase (trend) in the east and north gradient component.
This text is rewritten and the mathematical definition of "total amplitude" is added.

P13L22: A positive trend in the amplitude will occur if there is an increase in the variability at the side which can happen even if there are no trend ... (confused again how to understand the meaning of the sentence).
See previous comment.

P15Sec5: Consider modifying Sect 5.1 title by adding WVR so it is easier to distinguish which paragraphs compare GPS to WVR, and GPS to VLBI (5.2). Optionally, split 5.1 into sub-sections dealing with original and averaged comparisons.
Titles are modified — additionally now also WVR data are compared in Section 5.2.

P15L5: ... sites share several error sources ... (suggest to specify them more)
We now mention satellite clock and orbit errors and mapping functions.

P16Fig11: I suggest also discussing more ranges of estimated gradients, which seem different for GPS and WVR. GPS gradients are generally smaller. E.g. it could be due to constraining in GPS solution, mapping function, elevation angle cut-off, elevation dependent weighting or other effects. Similarly, it seems for GPS vs VLBI, where VLBI gradients seem to be more smoothed than GPS, most likely due to the 6-hour temporal resolution.
We think the amplitudes of the GPS and WVR gradients are now also addressed by the new Figure 11. Concerning the VLBI gradients we think it is difficult to make a general statement about their amplitude size. However, given that the sampling of the atmosphere is much more sparse, a short lived gradient in combination with assumption of linear functions in 6-hour segments, will probably reduce the variability in the estimated amplitude. We added this comment when discussing the new Figure 15.

P16L7: wet gradients from both GPS stations, ONSA and ONS1, ... (suggesting for a better clarity)
Corrected

P17Fig12: it seems that giving correlation coefficients in the text is enough, without further need to show both plots which characteristics are the same as in Figure 11.
We agree and remove the figure but keep the text.

P18Fig13: Consider merging four plots into a single one with the x-axis ranging in 2013 to 2016

We did consider this, but think that it is easier to identify the specific month and compare it between the different years. We keep it as it is.

P22L6: we find the wet component of the gradients cause most of the variability. (removed 'to')

Corrected

P22L15: if small gradient trends ... (remove plural)

Corrected

Referee #3:

There was a time when tropospheric delays were considered as error-prone parameters that had to be corrected by meteorological observations from other instruments. Successive methodological improvements have led Zenithal Wet Delay retrieved by GNSS to be sufficiently accurate for use in meteorology and climatology. The information content in linear horizontal delay gradients estimated from space geodesy observations is the next step, the central issue that must be treated rigorously so as not to lead to misinterpretations or over interpretations whose consequences can be unfortunate for the applications that come out of them. At first sight, the article is presented as providing general answers to this problem.
However, the results obtained are valid only in Sweden, in a particular meteorological context (the Icelandic low pressure system), in a particular geodesic context (the poorer sampling of GPS data on the sky north of the zenith direction due to the geometry of the GPS satellite constellation which is particularly the problem at high latitude) and mainly using the statistical notion of correlation coefficient. These results deserve to be verified on a global scale even if this study is an interesting intermediate step. However, the title should reflect the true scope of this study.
We do not agree that all results are only valid in Sweden. The meteorological processes mentioned, such as changes of air masses and systematic patterns in the pressure, are frequent in many regions. Also the possible instrumental effects related to anti reflecting material at the antenna is a general result. Since we clearly state in the abstract that we only use 5 sites in Sweden we want the title unchanged. Although we are not native English speakers it is our interpretation that the first two words of title indicate that it is a modest contribution to a complex subject.

Moreover it would have been interesting to provide more bibliographic references to list the results previously obtained for other regions and to compare them with the results of this study. This study deals with the concept of total, hydrostatic or wet gradients, of pressure gradient, of temperature gradient, of water vapor gradient, of wet gradient retrieved by WVR. However, these notions are not sufficiently defined or sufficiently documented by bibliographic elements, which undermines the clarity of the article. The reproducibility of the results of this article should be facilitated: -> Where can we download GPS, WVR and ECMWF data? -> How to estimate the gradients with the WVR? -> What are the options used to process the data in detail? If there are studies, technical reports or more general articles that can provide quick and accurate answers to these questions without lengthening the article excessively: perfect, if not the addition of elements in the supplemental material could be a good option.
It would have been necessary to give some elements on the comparisons between ZWD estimated by the different techniques before deepening the comparison of the gradients.
We believe that we have addressed all these points in the revised manuscript. They overlap with several of the specific points from all three referees. We will come back to these in the specific comments below.

One of the listed points of the summary is the comparison of the GPS gradients with the corresponding ones from the ECMW analyses. How GPS gradients can confirm known seasonal effects both in the hydrostatic and the wet components whereas GPS gradients are total gradients exclusively? In fact, it is an assumption that ECMWF hydrostatic gradients are reasonably accurate (P13L13). Before subtracting the ECMWF hydrostatic gradients from the total GPS gradients, rigorously, it would have been necessary to ensure that the hydrostatic gradients calculated by the ECMWF and felt by the GPS measurements are equivalent, which seems very difficult to verify.
We do say that it is an assumption. In order to a give a bit more motivation, showing that it is a reasonable assumption, in the revised manuscript we have two new Figures (9 and 16) that show the

different behaviour both in amplitude and in temporal variability of the wet and hydrostatic gradients from ECMWF.
The assumption is also motivated by that when WVR gradients are correlated with GPS gradients the correlation increases when the hydrostatic gradient from ECMWF is removed from the GPS total gradients. This improvement in correlation coefficients is now explicitly given in the caption to the new Figure 13.

The main statistical comparison tool is based on the notion of linear correlation. However, the article does not explain the advantages, disadvantages and limitations of this approach without any specific bibliography for this type of study. Again, it undermines the clarity of the article and the scope of its conclusions.
We have added standard deviations (SD) and state that correlation coefficients also are presented motivated by the different gradient amplitudes. They are used in a relative sense if an agreement is better or worse given identical input data but processed differently. Although we note that the comparison of the agreement between WVR and different GPS solution give the same result using SD as using correlation coefficients.

With just 13 lines in the article,
the results of the CONT14 VLBI measurement campaign seem insufficiently exploited.
The sampling of the sky is a critical parameter according to the authors, what are the further studies to be conducted to avoid or reduce this problem?
We have now added WVR gradients and make additional comparisons (see the overall changes at the beginning of the document).
We have added a short paragraph about the future of geodetic VLBI with the VGOS system.

Taking into account the preceding remarks and clarifying the points raised, the article could then serve as a reference for future studies.

Here are my specific comments.

P1L1 : " . We assess the quality of estimated linear horizontal gradients in the atmospheric propagation delay " versus
The "versus" comes in the 3rd sentence of the abstract which should be fine, or did we not understand?

P1L21 : " the reproducibility of estimated geodetic parameters " improper term ? / Repeatability?
According to e.g. NIST https://www.nist.gov/pml/nist-tn-1297-appendix-d-clarification-and-additional-guidance, the so called repeatability conditions are:
- the same measurement procedure
- the same observer
- the same measuring instrument, used under the same conditions
- the same location
- repetition over a short period of time

We think that for long time series of geodetic parameters, the term reproducibility should be used.

P2 : Figure 1 is a very rough picture of the real situation. Orders of magnitude are given without any explanation. Is the Earth modeled as an infinite plane or sphericity is taken into account? " The scale heights, hs of the hydrostatic refractivity and the wet refractivity are approximately 8 km and 2 km, respectively. " 8 km / 2 km ... Proof? References?

With the added theoretical background we can now refer to Equation (2) for the refractivities and we pointed out that there are large variations as well as that the sampling of the volume can and will be different for different instruments. The text of the figure is rewritten saying that it is a sketch, that the scale heights vary a lot, and referring to Eq. (2). We think every reader knows that the mapping functions used later in the modelling takes the sphericity of the earth into account, or ...?

P2L4-6 : " Gradinarsky et al. (2000) found that using different constraints for the variability of the horizontal gradient in the VLBI and GPS data analysis did not have a significant impact on the agreement with the WVR estimates. " Can you be more explicit and provide quantitative data?
The above sentence is replaced by:
Gradinarsky et al. (2000) found that when varying the constraint for the gradient variability from 0.2 to 5.6 mm/\sqrt{\rm h} the weighted root-mean-square (rms) difference compared to the WVR gradients varied between 0.8 and 1.0 mm for both the GPS and the VLBI gradients.

P2L6-7 : " A more recent study by Li et al. (2015) reported on the improvement obtained by Using multi-GNSS constellations instead of GPS only. " Can you explicit with quantitative results?
The following text replaces the one above in the revised paper "Using multi-GNSS observations, Li et al. (2015) found a significant increase in the correlation coefficient to about 0.6 when compared to ECMWF gradients, while the one for the GPS only was typically below 0.5. In addition, they found that the RMS difference of the gradient is reduced to about 25–35 % by multi-GNSS processing."

P2L16-17 : OK it is known but provide the major references ...
A reference to Rüeger (2002) is added together with the equation for refractivity and additional references follow in the same section.

P2L18: " Hydrostatic gradients " These terms are not defined in the article and are not commonly used.
These are now defined in the theoretical background in the beginning of Section 2.

P2L18-20: unclear
See the response above.

P2L20: see IERS conventions (2010)
We added also this reference in the theoretical background text describing the model.

P3L1: Provide more recent references. What are the scientific questions raised by this climatic specificity?
It is a well known meteorological feature (that affects space geodesy data). It is a confirmation that the gradients estimated from space geodesy data are correct (in this sense).
Two additional references are added.

P3L3: "Temperature and especially water vapour can show relatively much stronger horizontal gradients over small (kilometre) scales. The temporal variability is typically also much higher than that of the hydrostatic gradients, see e.g. Li et al. (2015). ": equations? References? Which temperature? Ground? Column? How are obtained these order of magnitude? [Typo : kilometer]
Equations have been added in Section 2.
We have also added the two new Figures 9 and 16, showing time series of hydrostatic and wet gradients from the ECMWF data.
We use British English and according to our dictionary the spelling is "kilometre"

P3L6 : " be significant during a passage of a weather front, especially for distinct cold fronts." order of magnitude

With the extension of Section 5.2 in the revised manuscript gradients associated with the change of air masses are shown. This is however, not very helpful here in the introductory part. Since we did not find a reference of gradients estimated during the passage of a "distinct cold front". We changed the text and referred to Kacmarik et al. (2018) and their estimated gradients during an occlusion front.

P3L7-8: Provide references that study these phenomenons with GNSS data.

We are not aware of published results that identify estimated GNSS gradients with a specific source, except for the fronts mentioned above. The following examples mention meteorological phenomena that have horizontal variability in the partial pressure of water vapour and with the equations now included in the beginning of Section 2, this should be clear. The text is rephrased and now first mention variability in the partial pressure of water vapour.

P3L16-17: software and references ...
Table 2 is expanded and updated

P4L3 : Ning et al (2013) : It would be interesting to speak about an eventual update of the procedure ... atx file should have been updated for instance ...

The atx file has been updated. This is specified in Table 2, including a footnote for the reprocessing done for the 4 years of data from the Onsala site for the revised version of the manuscript.

P4L4 : " we calculated mean values over 15 min, 1 h, 6 h, 1 day, and 1 month. " Ok but why? Specify scientific questions in term of atmospheric processes
Examples of atmospheric processes that affect the 3D refractivity over time were mentioned in the previous section. The 1 h was a mistake. The new text is:
" we calculated mean values over 15 min, 6 h, 1 day, and 1 month
in order to match the temporal resolution of the comparison data
and to study the variability of the wet and the hydrostatic gradients over different time scales."

P4L5-7: Figure 4 shows Figure 1 is too simplistic. May be presenting the problem like this?
We now point out the issue of sampling already in the caption to Figure 1.

P4L9-11 : references or technical report to provide ? What are the problems of this technique? Advantage and limitations?
A general reference to Elgered and Jarlemark (1998) is added. In the caption to Figure 6 we now mention that observations cannot be done in directions close to the sun and not below elevation angles of 20°.

P6L1-2 : " Therefore, data taken during rain, or when the estimated amount of liquid water is >0.7 mm, are discarded from the analysis. " references ? If we do not pay attention: what are the consequences? Bias? Ok for rain but without rain : precision ?
A reference to Westwater and Guiraud (1980) is added and we especially mention "large positive errors in the wet delay".

P6L2-3 " when the WVR hardware has failed. " Why?
We added brief information for to 2 long data gaps:
The first long data gap in 2014–2015 was caused by a broken mechanical waveguide switch and the second long gap in 2015–2016 was due to broken cables in the so called cable wrap. The cable wrap was redesigned.

P7L21-2: → It would be interesting to provide a reference which explains the observations

and the estimation of SWD with this instrument. It would be interesting to explain how the WVR gradients have been computed. → " where constraints with time are applied. " Specifically with your solution with GIPSY. → It would be interesting to recall what is observed and what is modeled with GNSS and WVR. Or provide references ...
The GNSS gradients were estimated using a random walk model with a standard deviation (SD) of 0.3 mm/sqrt(h)) that was taken from Bar-Sever et al. (1998). A reference to Jarlemark et al. (1998) is added in Table 2 for the GPS constraint for the ZWD.
The reference to Elgered and Jarlemark (1998) above and references therein explains the SWD/ZWD calculation with the WVR. We state that the WVR gradients use the "four-parameter model" in Davis et al. (1993).

P8 Figure 7: → optionally add rainfall? → Difference between [2013,2016] and [2016,2017] about the maximum number of daily data : around 10000 / > 10000 / Homogenous methodology of WVR observation ? → Histogram?
We think it would be too detailed to add rainfall information (since it occurs often and is the major cause for data loss except for the 2 long gaps). We now note that in the text and stress that the same observation sequence is used over the 4-year period as well as mention that observations close to the sun are removed (in the caption to Figure 7).

P9L15-17 : Are you sure of the units about the constraints ?
Yes, we now also note that they are not modelled as stochastic processes.

P9L16-17 : inhomogeneity between mapping functions ...
In the revised paper, we also processed the GPS data using the same mapping function (NMF) and found that NMF or VMF1 did not have a large impact on the gradient correlation with the WVR.

P9 part 3.4 : Focus on scientific and methodological questions ? Not enough details are given : impossible to reproduce the study.
Details have been added both on the motivation for the CONT campaigns and missing information about gradient mapping function and elevation cutoff angle.

P11L3 : Figure 9 : why do not use monthly running average ?
We could have done that but chose to illustrate the 11 years of data for each month to visualize any seasonal variation.

P11L4 : 10° or 20° ? What are the differences of the two GPS solutions? Which one is chosen? Why ?
In the revised paper, we added more results on the gradients given by different GPS solutions, i.e., different elevation cutoff angles, weighting and non-weighting, different mapping functions. We used the gradients given by the GPS 3° cutoff angle solution for all other comparison due to a better agreement when compared to WVR gradients.

P11L5 : " We can clearly see negative north gradient in the winter both in the GPS and the ECMWF results. " provide quantitative results
We added -0.2 mm as a mean value.

P11L7 : " the Icelandic low pressure system (Hewson and Longley, 1944). " Only one reference ... 1944 ...
Two more recent references were added in Section 2. Here we refer back to this.

P11L9-12 : add references / Why WVR data have not been used ?
Two additional sea breeze references were added in Section 2. Unfortunately, WVR data from scanning the sky are not available for the 11-year period only for 2013-2016. We say that sea breeze is

one possible cause, we do not claim that this is what we see. Perhaps in a future collaboration with meteorological expertise we could study sea breeze conditions using both GNSS and the WVR.

P13 Table 4 : 6-hour resolution of ECMWF data It seems difficult to draw conclusions from hourly comparison between GPS and ECMWF.
This was a mistake — thank you for making us aware of it. It is corrected to "six hourly".

P13L2-3 : " We assess the data quality, in terms of correlation coefficients, between the total GPS and ECMWF gradients estimated at the 5 GPS sites using data from 2006 to 2016. These are shown in Table 4. " The linear correlation coefficient is mainly used in this study: what are the advantages and disadvantages of the methodology followed?
Correlation coefficients are used in a relative sense by comparing the agreement at different stations. In Section 5 where we also use WVR data (that in general result in larger gradient amplitudes) we also use standard deviation to compare the agreements. The results are consistent.

P13L10-12 : " . 10 Another result worth noting is that the two sites with the highest correlation coefficients, and especially for the monthly averages, are ONSA and SPT0. These two sites are the only ones that are equipped with microwave absorbing material below the antenna. This could reduce the impact from unwanted multipath effects. The phenomenon calls for further studies. " It would be divergent with page 17 line 10 : "Comparing the results obtained for ONSA with those from ONS1 they are almost identical (in both Figures 11 and 13) meaning that in this case there is no obvious improvement from the absorbing material below the antenna on ONSA."
We give a possible explanation for this: "Our assumption is that the lack of a concrete pillar with a metal mounting plate just below the antenna on ONS1 eliminates the need for an absorber"

P13L13-15 : " Assuming that the ECMWF hydrostatic gradients, linearly interpolated between the 6 h values, are reasonably accurate we have the possibility to subtract this hydrostatic gradient from the estimated total GPS gradient in order to compare the wet gradients at these five sites " Provide a reference to justify the approach
See above — actually there was no interpolation. It is the "six hourly" values that are compared. We now refer to this comparison to be "six hourly"? This is done in both Table 4 and Table 5.

P13L16 : " We note that when the wet gradients are averaged over one hour and one day " Did you subtract the daily average before calculating the hourly average?
The 6 hour averages are the averages of the gradients over the 6 h. No daily average was subtracted. As mentioned above, hourly is changed to 6 h.

P14L3 " Typically they are all well below 0.01 mm/year. " Have you tested the significance?
No we did not, but we should have done that. Now we have calculated two different formal uncertainties and expanded the text. It is clear (we think) that no atmospheric gradient trend has been detected, but we still mention the highest trend estimated, and warn for instrumental effects when searching for trends in the future.

P15L5 " We expect that the two GPS sites share several error sources " OK, more detail should be given about GPS errors
We now mention satellite clock and orbit errors when observing the same satellites and we use the same mapping function (same as Referee #2)

P15L6 " there is a significant common mode suppression of errors " GPS data have been processed by PPP ... Can you explain more what you mean by "a significant common mode suppression of errors"? Do you speak about the common modelling to

process GPS data?
Yes, a common modelling and common errors associated with the same observed satellites (see the previous comment).

P15L6 : " be slightly overoptimistic. " that needs more investigation
We removed "slightly". It is just overoptimistic, because of the shared common errors mentioned above, but we cannot quantify how much.

P15 Figure 10 : There are differences that seem to be systematic over short periods of time ... (presence of dotted curves in this figure unlike Figure 11)
In the new Figure 12, we plot the GPS gradients with the original temporal resolution of 5 min. When compared to WVR data, in Figure 13 (middle and right), the GPS gradients were averaged to a 15 min temporal resolution in order to match the WVR data. We now state this in the figure captions.

P15L17-18 : Amplitude of the North component versus amplitude of the East component?
We did several plots of east versus north gradients and found that they were not correlated at all, so we did not pursue this further.

P15L4 " reduced at the order of 10 % " 10° to 20° reduces the correlation coefficients ... What happens if you reduce the cutoff from 10° to 5° or below ?
The question is dealt with in the new GPS solutions using cutoff angles of 3°, 10°, and 20°. Section 5.1 is significantly expanded with these new results.

P17L2-3 "). The other reason is the much higher variability in the time series from the WVR because no temporal constraints are used when estimating these gradients. " References about WVR and how its gradients have been estimated are necessary for a better understanding. Is it possible to add a stochastic constraint to estimate WVR gradients? I do not know if you can change the procedure to estimate tropospheric delays by WVR.
The description of the WVR and the estimated gradients is updated in Section 3. In principle, there is nothing that would not allow constraints to be applied in the WVR gradient estimation. However, that is a big effort (at least for us). We choose to discuss this as possible future work in the Conclusions section.

P17L6 : typo : Lu et al. (2016) Figure 8 of Lu et al. (2016)
Corrected

P17L10 " there is no obvious improvement from the absorbing material below the antenna on ONSA." the cutoff angle is fixed at 10° ... The effect of the absorbing material would be shown using a lower cutoff angle.
We processed the ONSA data using a 3° elevation cutoff angle and the result is the same, still there is no clear difference between ONSA and ONS1.

P12-13 " ECMWF gradients compared to the KIR0, MAR6, and VIS0 sites. Our assumption is that the lack of a concrete pillar with a metal mounting plate just below the antenna on ONS1 eliminates the need for an absorber (see Figure 3). " Good hypothesis that deserves to be confirmed: references on IGS network ?
Sorry, we have not been able to find any independent confirmation, someone has to be first ... However, to be fair, when the Swedish Mapping, Cadastral and Land Registration Authority designed the monument for ONS1 it was suspected that the original design of monuments in the SWEPOS reference network was not optimal.

P18 Figure 13 : The norm of monthly wet gradient as a bar plot would be interesting.

In the original manuscript we did not have any time series plot in Section 5.1. This comment, plus a few other comments, addresses the issue of the different gradient amplitudes. The new Figure 11 address these issues. Here the absolute value of the monthly means of the wet gradients are shown together with the ZWD monthly means from ONSA, ONS1, and the WVR. The idea is that these qualitative graphs show correlations that are clear to the eye offer a better understanding compared to additional correlation plots.

P19 part 5.2 : This part is a little disconnected from others and is not thorough enough to allow a clearer view of the contribution of VLBI to this study. More questions about the representativity of the gradients estimated by the geodetic techniques are araised. We would expect more answers on this issue.
When we added the WVR data for comparisons during this 15-day period we find that the temporal resolution of 6 h is limiting the study. The main result, we think, that is discussed in the updated text, is that we can try to motivate additional gradient studies using the upcoming VGOS.

P19 L6-7 : " We note that the agreement in general is better for the east component compared to the north " amplitudes of East and North Component ?
Better language: “Again we note that the agreement, in terms of correlation coefficients, is better for the east component compared to the north component.”

P19L7-8 : " where a large north gradient is not detected in the VLBI data. " How are estimated the VLBI gradients? Stochastic constraints? Impact of the 6 hour resolution? How gradients are modeled in the VLBI data processing? Step? Piecewise linear function?
The text in Section 3 has been updated to describe that the gradients are estimated as piecewise linear continuous functions and not a stochastic process.

P21 Figure 15 : " and the black dots are linearly interpolated VLBI results with a temporal resolution of 5 min in order to match the GPS data. " The interpolation must be consistent with the gradient modeling used for VLBI data processing. Can you clarify?
The figure has been removed from the manuscript. Instead a table with correlation coefficients for VLBI-GPS and VLBI-WVR has been added.

P21 Figure 15:" using mean values for the period of ±3 h around the time epochs of the VLBI values (6h:) " same remark as before: The 6-hour resampling of GPS estimates must be consistent with the gradient modeling used for VLBI data processing. Here, that implies that VLBI estimates are modeled as a step function.
Since the figure is removed it does not matter. (However, it can be consistent if also the continuous VLBI segments are averaged around the given value ± 3 h.)

P22L6-7: " When studying gradients averaged over shorter time scales, e.g. 15 min, we find the wet component of the gradients to cause most of the variability " Not exactly because you subtracted the hydrostatic gradients sampled at 6 h from the ECMWF. You did not analyze the variability of the hydrostatic gradients.
We have now added a plot (Figure 16) with ECMWF hydrostatic and wet gradients in Section 5.2. It is not 100 % safe but looking at the change from one 6-hour value to the next indicates that not much is happening in between.

P22L10: "during the warmer, and more humid, part of the year " It would have been interesting to use IWV retrieved by GPS.
The new Figure 11 addresses this issue. (ZWD is roughly proportional to the IWV.)

P22L11-12 " s in the east compared to the north direction. " It would have been interesting to better cross the amplitude of the gradients with the correlation coefficients obtained.

We do not understand what is meant, unless it is covered by the new Figure 11 in combination with the old figure with the new number 14? No other action has been taken related to this comment.

P22L12-14 " We interpret this difference to be caused by an inhomogeneous spatial sampling on the sky, which is important when we assume that the model describing linear horizontal gradients has deficiencies. The different sampling on the sky is an important issue for any comparison between different techniques. " This question remains unresolved and would have to be studied later.

Yes, that is what we meant. We have added the suggested sentence.

P22: Lack of " Data availability section"

We now give the IP addresses for the input data. The gradient time series estimated by us using GPS, VLBI, and WVR data have been archived and approved in terms of the documentation by the Swedish National Data Service (SND) and a doi number will be sent to us within days, so that it can be published in the final version. For the time being a compressed file is available via DropBox: https://www.dropbox.com/s/lg4sctpm6qrfto4/Gradient%20data.zip?dl=0