

Interactive comment on “Tropospheric products of the 2nd European GNSS reprocessing (1996-2014)” by Jan Dousa et al.

Responses to Editor, Dr Olivier Bock.

All corresponding major revisions are marked in manuscript using red colour.

Dear authors,

Thank you for carefully revising the manuscript according to the comments from two referees and from myself. The manuscript has been significantly improved and augmented. I think it provides interesting results to the atmospheric science community, especially with the addenda of sections 4.4 and 5. Since I asked for these addenda, I didn't send the revised manuscript back to the referees but did the 2nd review myself. Please see the specific comments and questions below and a few edits in the annotated manuscript.

Specific comments

GNSS processing: did you include GPS and observations from other systems? I couldn't find this information.

GPS data were used only as explained at P3L103 and indicated in Table 1.

P4L131-134: I guess you refer to the monthly statistics of ZTD and/or gradient differences (GPS – ERAI)? This is a post-processing QC, so maybe it should not be mentioned in this section.

The corresponding sentences moved to Section 5.3 (originally 4.2).

P4L147-158: on the impact of 3-day combination vs. 1-day solution. This discussion is useful and answers a comment from 1st referee. However, I think Figure 2 should not be presented in this paper since it is from a different analysis centre. Please removed the figure and revise the text.

The figure was removed and the paragraph revised accordingly.

P6L225-226: Explain why the improvement is smaller for GO4.

It is not straightforward to compare the two results of different processing and different station set. As the effect depends on selected stations, a slightly higher impact in a global scale might be attributed to the distribution of stations including differences in term of latitude and altitude.

P7L255-258: Why do you think the residuals are dominated by errors due to modelling of tropospheric parameters, and particularly the wet contribution?

The quality of the troposphere modelling in GNSS analysis has a high seasonal dependence (in Europe at least) which is clearly visible from the comparison of tropospheric products either for GNSS vs. NWM or GNSS vs. GNSS, reaching up to a factor of 2-3 higher standard deviations in summer for both zenith total delays and tropospheric gradients. Obviously, the GNSS tropospheric model is simplified in term of modelling a horizontal tropospheric asymmetry as well as dependence on elevation angle. The former is mainly the effect due to a high spatio-temporal variability of humidity in the troposphere. The tropospheric delay has definitely the most pronounced seasonal signal in GNSS analysis and resulting ZTDs are correlated with estimated station height. It is well-known that the modelling of the first-order tropospheric asymmetry improves station horizontal position, however, the effect is about an order smaller compared to ZTD variation and its impact on height. We tried to reformulate the sentence considering that not all readers are familiar with GNSS analysis.

P7L266-278: this discussion calls for more careful interpretation. I think there is no evidence that the bias is due to ERA-Interim data (L268). If this statement is based on the GNSS comparison, assuming GNSS is the reference, it should be proved first that GNSS can sense the absolute ZTDs with an accuracy better than 1.8 mm. The fact that a similar bias was observed with other GNSS software doesn't mean that the bias is not in the GNSS estimates (L269). It just indicates that the bias is not software-dependent. A common bias might be due to the use of the same satellite products, similar tropospheric model, the regional-scale network...

Thank you for your comment, the paragraph was revised.

P7L274-278: These comments don't add something to this study.

The comments were removed.

P8L299-301 and Figure 6-7: It is not clear what the statistics mean exactly. Are all values put together or is there an order of computation with respect to time and stations? It should be clearly explained how the means, standard deviations and error-bars are computed. The error bars seem very large for standard errors (by definition representing the uncertainty of the computed value). My interpretation is that the mean bias and standard of deviation of ZTD differences are first computed for each station and each month, and then mean and standard of deviation of these values are computed and the means are plotted +/- 1- standard of deviation. This would mean that the error bars represent the 1-sigma range (or dispersion) of values over the ensemble of stations. This is something different from standard error and uncertainty.

The corresponding paragraph and figure were corrected using the term 1-sigma range of values over the ensemble of stations.

Table 4: similar comment: explain how the mean +/- dispersion (?) values are computed.

The description was added in Section 5.3 (originally 4.2), the second paragraph.

P9L333-342 and L350-359: recombine these two paragraphs

Originally, the two paragraphs were related to different description – Table 4 vs Figure 7. Paragraphs in Section 5.3 (originally 4.2) were revised starting with the description of overall statistics in Table 4 (and all variants) followed by description of impact of gradient combination.

General comments on Section 4.4:

- The comparisons presented in this section are relevant and the discussion is well organised, comparison after comparison, referring to Table 5 and the different figures.
- I suggest to go in the same order for each discussion, commenting first on the median and min/max values in Table 5, and then on the latitudinal and height variations, and finally on the time evolution.
- Since latitudinal variations are more relevant I suggest switching the order of Figure 8 and 9.

The Section 5.5 (originally 4.4) was revised according to the abovementioned suggestions and the figures were also changed. The geographical maps provided in supplements were added and commented.

- There is in general no trend with height, except in GO1-GO0. However, the increased scatter at low heights might be mentioned (for all comparisons). Is there an explanation?

The explanation is supposed to be mainly due to the general increase of ZTD (both ZHD and ZWD) with the decrease of altitude, consequently increasing an overall impact of differences in applied models.

- Add median values (e.g. as solid line) on the plots in Figure 10. It is important to check if there are drifts over time.

Yearly mean biases and standard deviations were included to Figure 14 (originally 10).

- The maps provided in the supplementary material could be included in the manuscript and referred to in complement to the latitudinal and height variations (Fig. 8, 9).

The maps provided in supplements were added and referenced in Section 5.5 (originally 4.4).

- Instead, it would be appropriate to provide in the supplementary material a table or a text file with the results plotted in the maps and in Figure 9 (overall statistics by station, including latitude, longitude and station height).

ZTD trends for all variants estimated for 172 EPN stations with time series over 10 years were provided in table as supplements additionally indicating lengths of time series, number of available data and mean formal errors of the trend estimates.

- Use the term 'bias' throughout the text to be consistent with the figures and tables, instead of using sometimes 'systematic errors'. Specific comments on Section 4.4

Corrected.

P10L383: I think the differential performance of GMF/GPT and VMF1 has been studied and published by Boehm et al., and the increased errors of GMF/GPT at higher latitudes is a known issue. Please check the literature and provide adequate comments and references on this issue.

Thank you. We included the reference in the discussion. Our results are consistent with findings by Steinberger et al. (2009). Our results demonstrate a partial compensation of the atmospheric loading effect by using a blind model compared to a model based on actual weather conditions. In case the atmospheric loading effect was not corrected for, the errors are assimilated mainly to zenith total delays (less in the former

P10L386: Effect is attributed to more low-elevation observations over time. It would be nice that you document the time evolution of the number of (low-elevation) observations to provide evidence for this effect. Since you processed the data you have all the necessary information. This is usually not possible to check for end users (e.g. from the meteorological community).

New figure – example for WTZR station – was added indicating an increase in number of observations at low elevation angles during 1996-2015, which is, generally, common to a majority of EPN stations.

L395: conclude on the sensitivity of results on the cutoff angle.

Concluding sentence completed.

L398: could the degradation prior to 2002 be due to a change in the processing method or in pressure data used to compute the atmospheric loading?

The strategy was the same during all the 2nd reprocessing. As the degradation is observed only for G04-G01 comparison, it might be related to the quality of pressure data used to compute atmospheric loading.

L401-403: the scatter in bias and std is the largest for this comparison (G04-G01). Since coordinates are fixed, I guess that uncorrected atmospheric loading in G01 solution is compensated by ZTD biases. Is this what you mean?

Yes, the sentences were really confusing and we modified them to be clear now, hopefully.

L414: why would the asymmetry be more imperfectly modelled with one tropospheric gradient every 6 hours rather than one per 24 hours? It seems more logical that the model with more parameters is better.

The plot shows only standard deviations from the comparison of GO4 vs GO6, i.e. 6-h vs. 24-h gradient model, thus it does not suggest which parameterization is better. The 6-h piecewise linear model for gradients could be helpful for more precise modelling of temporal variability in the troposphere. On the other hand, none of the parameterization is able to capture a higher-order asymmetry which is increasing towards the equator as mainly caused by humidity in the atmosphere. We modified the sentence to be clear.

General comments on Section 5

The goal here is to assess the impact of variants on trend estimates. Though a detailed analysis of trend estimates is out of scope of this paper, the results shown in Figure 11 should be more thoroughly commented and maybe completed with mean values per variant, or differences of variants like in section 4.4. It seems to me that GO0 and GO1 yield very similar trends, which means that the mapping function and ZHD don't have a strong impact. Cutoff angle has an impact (as previously shown by Ning and Elgered, 2012). Variants GO4, GO5, and GO6 are very similar, but not consistent with GO1, which means that non-tidal atmospheric loading has a significant impact. It would be very valuable if trends were computed for all stations and all variants and the mean values and std summarized in a Table per variant. Conclusions could thus be more statistically significant. Then I would also suggest to provide the trend and uncertainty estimates per station as a supplement for further intercomparison with similar publications in the COST Special Issue (Baldysz et al., AMT-9-4861-2016, Klos et al. AMT-2016-385).

In total, 172 EPN stations were finally selected according to time-series span over 10 years at least. For all these stations, the trend analysis was performed without data homogenization or outlier rejection as our focus was purely on impact of GOP variants. Site-by-site estimated trends from all variants are provided in supplementary materials completed by time-span information, number of data and mean trend formal errors. Description in new Section 6 (originally 5) was revised according to the above suggestions.

Specific comments on Section 5

P11L419: add a reference to Eq (2), e.g. Weatherhead et al., 1998 ; Bock et al., 2014

Weatherhead, E. C., et al. (1998), Factors affecting the detection of trends: Statistical considerations and applications to environmental data, J. Geophys. Res., 103(D14), 17,149–17,161, doi:10.1029/98JD00995.

References added. The second reference already used at other place.

P11L423: *seasonal, sub-seasonal and high-frequencies => be more specific: e.g., annual and 2nd, 3rd... harmonics.*

Corrected: including annual, 2nd harmonics (semi-annual) and daily.

P11L427: Note that the noise is assumed white and thus the formal errors of the trend estimates are underestimated by a factor 2-4 (Nilsson and Elgered, 2008 ; Klos et al. AMT-2016-385).

Nilsson, T., and G. Elgered (2008), Long-term trends in the atmospheric water vapor content estimated from ground-based GPS data, J. Geophys. Res., 113, D19101, doi:10.1029/2008JD010110.

Thank you. Reference added.

P11L428-429: The impact of cutoff angle was already reported by Ning and Elgered (2012).

Ning, T., and G. Elgered (2012), Trends in the atmospheric water vapor content from ground-based GPS: The impact of the elevation cutoff angle, IEEE J. Sel. Top. Appl. Earth Obs. Remote Sens., 5, 744–751, doi:10.1109/JSTARS.2012.2191392.

Reference added, used here and in also in Section 5.3 (originally 4.4).

P11L428-429: differences don't reach 1 or 0.5 mm/year, please correct.

Corrected.

Section 6: suggest to move it after section 3 (so change numbering of subsequent sections) as the focus is more on observation level and data processing.

Moved.

P11L431-432: is this web interface available to the scientific community?

The actual address is temporary, but it is being prepared for IGS Tropospheric WG web-pages www.igs.org.

P11L454: could you add the ZTD difference series on Figure 12? At the end of section 6 it is not clear if the problematic stations/periods were removed from the dataset analysed in previous sections?

Monthly ZTD biases added in the figure. The problematic stations/periods mentioned at the end of Section 4 (originally 6) were not removed from the dataset analysed in Section 6 as there was no scope to set objective criteria for definition of problematic stations. This should be a part of follow-on study.

Table 4: 0.43 is repeated in the last column

All values 0.43 in the last column of the table were corrected. Thank you for this notice.