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Atmospheric Measurement Techniques Discussions

Interactive comment on "Tropospheric products of the 2nd European reprocessing (1996–2014)" *by* Jan Dousa and Pavel Vaclavovic

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Dear authors, please find below a few additional comments to those provided by the two referees.

1. You write that the EUREF recommendations are followed which specify that "weekly coordinates should be used to estimate tropospheric parameters on a daily basis" (L129) and that the coordinates were fixed to these values (L144). Did you fix the coordinates for all stations or only the fiducial stations? Fixing station heights is known to produce biases in ZTD estimates due to un-modelled station motions (tidal and non-tidal, e.g. seasonal) and other error sources (because of the correlation between estimated parameters). Abrupt changes and drifts over time that impact stations height would then also map into ZTD estimates. Can you comment on the uncertainty in the

ZTD estimates, and possibly also gradients, due your specific processing strategy?

2. The relevance of this study is that several processing variants are produced with the same software. The results are thus not obscured by inter-software biases. However, the discussion of results from the different variants is quite short in the manuscript. The accuracy of tropospheric parameters is only analysed based on Table 5 and Figure 5 and 6. Table 5 compares the biases and standard deviations over all stations and all times for the different variants. It is striking that the differences in these numbers are tiny. I would not be surprised that a spatialized analysis reveals significant impact of changing the cutoff angle and mapping functions at sites in different climatic regions, in mountainous areas, or close to the sea. If relevant, I suggest that you complement the paper with spatialized results.

3. Table 4 comparing the GOP solutions to the outdated EUREF repro1 is not relevant. This comparison might be done as an initial consistency check of the new solutions compared to the legacy EUREF reference. I thus suggest removing this figure and the related text from the manuscript.

4. The temporal homogeneity of long time series is crucial when trends are to be estimated. Given that there is presently a high interest of the GNSS/climate community in estimating trends, I think it would be useful to complement the results with an analysis of trends for the different processing variants. There are many questions like: which cutoff angle and mapping functions choose to get the most homogenous time series? What is the impact of changing quality in GNSS observations over time? Again, the conclusions might be station dependent and both overall and spatialized analyses might be necessary to document them properly.

5. The impact of the temporal resolution of gradient parameters is intriguing. Indeed, better accuracy is expected when combing the 6-hourly to 24-hourly estimates. With 4 times more observations the standard deviation is expected to be divided by a factor of 2. However, according to Table 5 the improvement is only by a factor of 1.3 suggesting

there is serial correlation in the errors. Is this reduction factor is uniformly distributed over stations and stable over time? Can you be more specific about the correlation between gradients and other parameters suggested in the manuscript? (L280 and 309)

6. The drift in the gradient estimates at station MALL (Fig. 7) is impressive. How did the ZTD estimates evolve during the period when gradients drifted? Did you detect other cases like this? In the case of MALL the cause was identified as a tracking problem. Did you detect other causes which could produce such drifts in gradients or ZTDs? It would be interesting to include a check on gradients as part of a data screening method. I suggest considering this idea in the discussion.

Other specific comments are given in the annotated PDF.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2017-11/amt-2017-11-EC1-supplement.pdf

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