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Atmospheric Measurement Techniques Discussions

Interactive comment on "Trends in the atmospheric water vapour estimated from GPS data for different elevation cutoff angles" *by* Tong Ning and Gunnar Elgered

Anonymous Referee #2

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Referee report of manuscript amt-2018-279 "Trends in the atmospheric water vapour estimated from GPS data for different elevation cutoff angles" by Tong Ning and Gunnar Elgered

General comments

This manuscript reports on the impact of GPS data elevation cutoff angles and other processing options on IWV trend estimates in Scandinavia. This work is very similar in the concepts, ideas, and methods to a previous publication by the authors (Ning and Elgered, IEEE, 2012). The processing options are evaluated based on the correlation coefficients (and RMS differences) between the GPS trends and radiosonde and ERA-

Interim trends at 13 GPS sites. Compared to their earlier paper, differences are with the length of the GPS series (20 yrs compared to 14), the use of ERA-Interim as a second validation dataset, and the test of other processing options (mapping functions, correction of higher order ionospheric effects, and elevation-depending weighting). The longer time series and the use of a second validation dataset yield more statistical confidence into the new results. However, the conclusions remain unchanged and the authors still recommend using a 25° cutoff angle rather than 10° (though only these two cutoff angles are tested in this study) and note that the other processing options that were tested are insignificant. Little new knowledge is brought actually compared to the earlier paper.

One or both of following directions should be considered to increase the relevance of this study: 1) investigate the reasons of the different trend values found for the different cutoff angles by inspecting carefully the differences in the estimated IWV time series (are the differences due to drifts in the time series? If yes what could be the reasons? Are they due to multiple offsets due e.g. to documented or undocumented equipment changes?); 2) extend the study to other regions/climates where the sensitivity to cutoff angle and/or the other processing options tested here might be different. One can note that this was suggested by the authors themselves in this manuscript and in their previous publications.

Detailed comments

Given the small number of GPS sites used in this study, the statistical significance of the computed correlations and RMS differences is rather small (though not quantified). It seems thus hazardous to draw general conclusions on the choice of the cutoff angle. More insightful analysis is indeed required to convince the readers to use a 25° cutoff angle for trend estimates, especially since the general tendency in the GPS community is to use lower cutoff angles (typically between 3° and 10°) and the IWV comparison (GPS vs. radiosondes and ERA-Interim) shows that the biases and standard deviations increase when the cutoff angle is increased. Moreover, it should be recognised that trend estimates are sensitive to small changes in the mean bias and extremes at the beginning and end of the time series, and thus conclusions based on trend estimates can be tricky.

A case by case analysis may help understanding the reasons why trend estimates change between 10 and 25° cutoff angles at some of the 13 sites investigated (e.g. OVE0, OLKI) and may strengthen the conclusions.

Why are the data not reprocessed for all the cutoff angles, e.g. between 5° and 30° or more, as in Ning and Elgered, 2012?

Why is only antenna, radome, and microwave absorber changes considered as GPS interventions? Did the authors check that receiver changes do generate breakpoints?

Moreover, in several places in the manuscript, the breakpoints in the GPS series not explained by antenna, radome, and microwave absorber changes are attributed to environment changes resulting in changes in multipath. Firstly, this attribution may be wrong because receiver changes are ignored. Secondly, the attribution to multipath is pure speculation as no additional observation/data/information is provided to support this hypothesis. After the receiver changes are checked, I recommend to call the remaining breakpoints 'unknown' or 'undocumented' unless a true multipath diagnostic is provided.

Regarding the choice of period for correction of the GPS interventions, the one with the smallest bias compared to ERA-Interim might not be the best choice since ERA-Interim itself may contain biases. Why didn't the authors use the more recent period following their previous work (Ning and Elgered, 2012)?

The mapping function test and second-order ionospheric corrections performed on only two sites are not significant and don't add anything to the study as the impact of these parameters is known from past studies to be small in the study area. If to be mentioned, they may simply be included in the discussion section along with the elevation weighting

results (Fig. 9 unnecessary).

Use statistical tests to assess the significance of trend estimates and differences.

The authors recommend to compare the trends computed from two different cutoff angle elevation solutions. What should be done when they yield significantly different values? Data from ERA-Interim and radiosondes should be intercompared and checked for inhomogeneities as well. Why didn't the authors use a homogenized radiosonde dataset? (e.g. Dai et al., 2011)

Dai, A., J. Wang, P. W. Thorne, D. E. Parker, L. Haimberger, and X. L. Wang (2011), A new approach to homogenize daily radiosonde humidity data, J. Clim., 24, 965–991.

Table 5: are the ZHD trends significant? They are not discussed in the text. Figure 4: is this figure really useful?

Figure 8, 9: unnecessary figures, but the statistics could be included in a Table.

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