Comments on « Decadal Variations in atmospheric water vapor time series estimated using ground-based GNSS »

By F. Alshawaf et al.

The authors used several datasets, including ground-based GNSS, to estimate variations and trends of precipitable water vapor over three stations in Germany. They first compare different methodologies to get PWV and then extract different factors which generate the observed variability : the trend, the seasonal cycle and the short-term variability. The study deserves to be published, however several aspects should be improved before it can be accepted for publication. I thus recommend major revision following the following comments :

- introduction : the objectives of the paper should be better explained.

- The authors say they analyzed time series of PWV at the 84 GNSS sites but they only show the results at two sites and do not discuss the results of the global analysis. Please add a more regional discussion of the results (the authors propose a regional analysis in future work but I think part of this analysis should be inlcuded in this paper).

- The relationship between PWV and temperature trends shoudl be better assessed : does it follow Clausius-Clapeyron relationship ? If not explain.

- We do not know if the computed trends are significant or not. Errors are missing.

- According to equation 15 or 16, it is not clear how you compute trends at the beginning and end of the time series.

- Figs. 7 and 8 : there are some differences between the three methods that are discussed. The black line is the fitting straight line of which dataset ?

- Tm shows strong differences when using surface temperature or vertical profiles of ERA-interim. The authors do not explain the huge bias at site 0285. They finally conclude that they can use equation 10 because the mean difference they generate in the computation of PWV is weak. However, I would like to see a scatter plots of these differences because a mean differnce is not enough to convince the reader it does not impact the value.

- Another issue on the methodology is the use of a constant (in space and time if I understood well) lapse rate of temperature. Isn't it a big approximation ?

- The part with snow and precipitation is too poor. Either you bette analyse the role of snow and precipitation (other sites, trend in the occurrence of T over 0°…), either you remove it from this study.