Review of “Trends of atmospheric water vapour in Switzerland from ground-based radiometry, FTIR , and GNSS data”

General comments

The paper is a detailed study of the IWV data (and trends) gathered by all available IWV measurement devices available in Switzerland, and extended with the most state-of-the-art reanalysis output available (ERA5 and MERRA-2). The use of a trend analysis model that account for possible biases (inhomogeneities) due to instrument changes is really a strong point of this study. At some places, the study does lack some interpretation of the findings (see my specific comments), and I identified three remaining caveats the study should deal with before publication:

- In the analysis of equations (9), (10), and (11), it is assumed that the relative humidity is constant, as is quite often the case when assessing the impact of the Claussius-Clapeyron relationship. However, in the study of Wang et al. (2016), already referred to in the manuscript, but not in this section, it is shown that the relative humidity varies with temperature. As you might have relative humidity measurements available (in any case you might use the reanalyses output), it would be good to confront your approach with the one described in Wang et al. (2016). In the conclusions section (lines 505-509), you also highlight this weakness in your approach. I think that adding the variation of relative humidity with temperature as an extra piece of information to understand the variation of IWV with temperature in Switzerland should be considered.
- Another major point I want to raise here, is the comparison of trends (especially in section 6.1) between GNSS sites that have a different length of time series. The length of the time series is only considered in the size of the markers in Fig. 8, but will interfere with other effects that have considered to compare the trends (e.g. the altitude of the station, in Fig. 9, to name only one). Due to the inter-annual variability of IWV, the starting/end date of the time period has an impact on the trend value. Therefore, I would urge for a detailed analysis of the impact of the length of the time series on the resulting IWV trends, by e.g. constructing an alternative Fig. 8 with trends calculated from a common time period and/or constructing a figure like Fig. 9, in which the trend is assessed against the starting year of the GNSS time series.
- The section 4.1.1 is definitely not my favorite section. It leans on Fig. 5, in which the trend uncertainty bars should be added. This might be tricky, since, as far as I understand, those monthly trends were computed based on only one value a year, so based on about 20 points (20 years) only. Besides this small amount of points, the length of the time series is very short in climatological sense (where time periods of 30 years are the standard). As a matter of fact, there are some interesting studies that calculate the number of years that is needed to derive a statistically significant trend in IWV (e.g. Alshawaf, F., Zus, F., Balidakis, K., Deng, Z., Hoseini, M., Dick, G., and Wickert, J.: On the statistical significance of climatic trends estimated from GPS tropospheric time series. Journal of Geophysical Research: Atmospheres, 123.

https://doi.org/10.1029/2018JD028703, 2018). So, the main question is: how significant are the trend value differences between the different months? The discussion in lines 309-316 also seems to indicate that the found "seasonal" trend differences (and the shape presented) is not a consistent feature.

Specific comments

- Page 1, lines 20-21: rephrase "it builds the link between temperature and precipitation" , building the link sounds awkward
- Page 2, line 42: specify in which wavelength range the satellites that are restricted to oceans only operate.
- Page 2, line 47: drop "probably" in the sentence "Radiosondes probably provide the longest time series, …"
- Page 5, lines 147-148: it is a pity that you use the Bevis approximation to estimate Tm from Ts, while you could have used the ERA5 vertical profiles of temperature and specific humidity to calculate them. You might comment on the applicability of the Bevis approximation for Switzerland.
- Page 8, lines 237-238: explain what the impact of an antenna and receiver change might have on the data variability. I understand that these changes might cause a jump in the mean of a time series, but it is less clear to me how they might cause a higher/smaller data variability. Please comment on this.
- Page 8, lines 239-240 and Fig. 2b: which differences are you referring here to? GNSS – ERA5 or ERA5 – GNSS. Please specify.
- Page 9, lines 264-and following: when comparing the IWV trends of TROWARA with the GNSS IWV trends, please mention if these trends are calculated for the same time periods, and if not, what the impact of the time period on the calculated trend is.
- Page 10, lines 291-292: to which GNSS stations with deviating trend values, due to instrumental issues are you referring here to? I guess not to Payerne, since the radiosonde measurements give trend values close to the GNSS IWV trends, no? Please specify the GNSS stations here.
- Page 10, line 307-308: I don't understand this argument: strictly speaking, a (constant) bias between datasets will not have an impact on the trend difference between those datasets. Please explain.
- Page 12, line 345: this argument can be easily checked: do the same analysis for the reanalysis grids neighboring the used reanalysis grids at Bern. Please do so.
- Page 12, lines 346-following: as mentioned as a general comment: in this discussion, I was asking myself what the impact was on the constant RH approximation on the results of this analysis.
- Page 12, line 365: here, but also at other places in the manuscript: please use the word "bias" only if you mention your reference. It is not clear from this sentence here if GNSS is biased low w.r.t. FTIR or if FTIR is biased low w.r.t. GNSS.

- Page 12, lines 371-377: as the IWV amounts at Jungfraujoch are very low, there might be another argument for the GNSS dry bias w.r.t. FTIR: as pointed out by Wang et al. (2007), under dry conditions, the GPS is less sensitive to low IWV values as other devices (reference: Wang, J., Zhang, L., Dai, A., Van Hove, T., and Van Baelen, J.: A near-global, 2-hourly data set of atmospheric precipitable water from ground-based GPS measurements, J. Geophys. Res., 112, D11107, doi:10.1029/2006JD007529, 2007). Please comment.
- Page 13: the word “astonishingly” is not very scientific
- Page 13, line 387: what is the trend estimate of the GNSS time series that coincides with the FTIR time series?
- Page 13, line 407: there’s a typo in “autumn”
- Page 14, lines 411-412: as already mentioned in the general comments: how many years do you need to obtain “stable” trends?
- Page 15, lines 454-455: the large spatial variability between the different grids in es changes in Merra-2 is surprising, especially when comparing with ERA5, which have a higher spatial resolution. What is the explanation for this?
- Page 16, lines 460-461: this statement can be easily verified: what is the winter temperature trend in the 1995-2018 interval?
- Page 28, Fig. 4: The datasets that have the smallest trend uncertainties (the reanalyses) are actually the ones of the course time evolution (monthly means), while GNSS data were available as hourly values, if I remember it correctly. Please comment on the trend uncertainties obtained here in this perspective.