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Interactive comment on "Interpreting the time variability of world-wide GPS and GOME/SCIAMACHY integrated water vapour retrievals, using reanalyses as auxiliary tools" *by* Roeland Van Malderen et al.

Anonymous Referee #1

Review of the manuscript

"Interpreting the time variability of world-wide GPS and GOME/SCIAMACHY integrated water vapour retrievals, using reanalyses as auxiliary tools" by Roeland Van Malderen, Eric Pottiaux, Gintautas Stankunavicius, Steffen Beirle, Thomas Wagner, Hugues Brenot and Carine Bruyninx

The manuscript present the results of a study focussing on the variability of integrated water vapour across the globe as provided by two different datasets (sub-daily GPS

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Discussion paper

and monthly-mean GOMESCIA estimates).

I find the topic important and the results really quite interesting, and I liked the summary Figure 10. My main concern is about the way the scientific questions are addresses (too vague), the motivation behind the approaches proposed (not discussed), a lack of precision and explanations in numerous parts of the manuscript as well as too many speculations. As a result, this is sometimes confusing, so that I recommend at least a major review.

Some examples of lack of precision: 1) the teleconnection indexes are presented with no logical link, for instance, you do not explain your choice, nor systematically provide the associated fluctuation time scales; as a result this is confusing; 2) The section describing the GPS dataset is quite long but I could not find information on how the authors dealt with missing data; 3) in the regression analysis, I am not sure surface pressure and atmospheric temperature are taken at the site location or over the region to which the site belongs; 4) sentences such as "for (sub)tropical sites and sites in East Asia two distinct lognormal distributions are needed, probably related to the monsoon and ENSO" why ENSO? 5) in Figure 4, there are more tones of colours than indicated in the figure caption (e.g. pale green versus darker green, same for yellow to green). 6) "Comparing our mean IWV trends with the 0.26 mm decade-1 GPS IWV trend quoted by Wang et al. (2016a), we found slightly lower rates of 0.19, 0.08 and 0.11 mm decade-1", 0.10 is NOT slightly lower than 0.26!

It may be a good idea to separate the manuscript in at least two parts, so that it may be easier to present proper presentations and discussions of the results (which are very interesting and numerous). Including more material in appendix may help too.

Also, I think it would be excellent to take advantage of the global dataset (GOMESCIA) to add maps covering the whole globe rather than only presenting maps with results shown at GPS sites (data are sparse in large areas of the world and very clustered in others).

On another subject, while I can understand your choice for a regression method where you test very numerous potentially explaining variables, I think that you should discuss more how your results compare or not with other studies and mechanism (I understand that it is difficult because there is a lot in the manuscript). Also, I did not understand your motivation for including the linear trend as an explanatory variable while it was already taken into account in Eqn (1) and finally did not explain much more.

Below I present some specific comments on the first part of the manuscript. This is not exhaustive, but I hope they can help the review process and help you to revise the following sections and conclusions accordingly for a second round of review.

Specific comments

Abstract: you need to be more precise about the time periods and time scales of analysis.

Page 1, line 14, "IWV variability": please precise at which time scale and over which period, and the IWV sampling time step.

Page 1, line 18, "on average": this is too vague, on average over what?

Page 1, line 20-21, "the seasonal behaviour and the long-term variability are fitted together": this is not exactly what I understood. Rather, you aim at reconstructing the time series of monthly-mean IWV from the mean annual cycle, linear trend and explanatory variables.

Page 1, line 25, "long term trend": please precise, i.e. linear trend over the period [year1,year2]

Page 1, line 26: variableS

Introduction

In the last paragraph of the introduction, you present the work presented but I could not really find a clear presentation of the question(s) you want to address. In my opinion,

an analysis of a new type is not a good enough motivation per se.

Page 2, line 4, 'on local scale': are you referring to mesoscale here? I would rather say that "At all scales" rather than local scale because precipitation for instance is not simply related to "local water vapour" alone; it typically involves larger, e.g. synoptic atmospheric circulations as well. In addition to the diabatic processes you mention, I would add radiative processes. I would remove "Of course".

Page 2, line 7, 1st sentence: a reference is needed there.

Page 2, line 26, "have the potential to be used for climate change analysis, which is the subject of this paper": I do not agree about this statement, the results of this study are more focused on interannual variability (and trend over the 20-year-long period). The present study provides very valuable information about IWV variability in space and time. In time, it goes from the annual cycle to short inter-annual variability to trends over periods of 20 years or less. In my opinion, 20 years is too short to provide robust information on climate change. It is less than the time interval typically used to compute climate mean (30 years, e.g. see http://www.metlink.org/climate/depth-climate-met-office/).

Page 2, line 31: add 'generally' before 'occur' as this is to my knowledge not strictly true for all geographic locations.

Page 3, line 2: remove 'can' as you precisely provides number who illustrate it.

Page 3, line 4: remove "of course" and replace "can be" by "are" as existing studies allow you to be more affirmative.

Page 3, line 6: I am not sure of what you mean by "autocorrelation" here.

Page 3, line 6, about ENSO: it seems to me that, more precisely, the relatively large magnitude of the signal induced by ENSO events at inter-annual scale affects trends computed on periods of 10-20 years. It would be good to reformulated a bit the sentence to be more informative.

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Page 3, lines 16-17: I think it is "on one hand", not "at one hand". More generally would be useful to check English throughout the manuscript. Avoid expressions such as "not surprisingly" or "of course" when you do not provide explanation nor references.

Page 4, lines 7-14, about homogenization: as the dataset in use here is not homogenized, I think this paragraph is unnecessary long. You could mention in the conclusion "for extension/improvement of this study, the use of a future homogenized dataset (as described in Van Malderen et al. (2017)".

Page 4, presentation of GPS data processing: you may consider moving part of it in supplementary material.

Page 6, about ERA5: this is not used, so the whole sentence is useless. It could be used in the future only, so potentially, you could mention it in the perspectives.

Page 6, lines 16-22: I am wondering why you mention this with so much details without linking this to your study.

Page 6, line 32: it seems to me that prior to Chen and Liu (2016), other studies such as by Bock et al. already extensively evaluated ECMWF and NCEP products.

Page 7, presentation of teleconnection indices: these indices are presented without much logic, you must re-write this section in a way that motivates your choice, and explain more over which time scales/regions they are relevant (and add somewhere the precise coordinates of the regions presented in the last figure).

Page 8, section 3: 1) how did you deal with missing GPS data? 2) It is not well suited to use the word 'bias' as you do not have a reference dataset here.

Page 8, "We exclude the GOMESCIA dataset here, as only monthly means are available, which might be problematic to compute significant frequency distributions": I do not understand what you mean. It seems you are mixing statistical robustness and time scale issues. Pages 8, 9, 10 and Figure 4: there are more tones of colours than indicated in the figure caption (e.g. pale green versus darker green, same for yellow

to green). Please clarify. Also, I would like to see the full maps obtained with ERA-I, NCEP as well as monthly-mean GOMESCIA IWV values. This would allow assessing the representativeness of the results obtained at the sites, and provide a clearer picture than emerging now at the end of this section. It is also very difficult to see the results over Europe because circles are overlapping each other.

Page 11, Figures 5 and 6: A discussion of the geographical patterns shown in Figure 5 is missing. In Figure 6, I am wondering about how this graph was made: did you consider all the stations? I guess the uneven location of the stations is playing a large role in the shape of these histograms. I would like to see the same graph with all the global GOMESCIA dataset.

Page 11, section 6: Given the content of this section, I suggest that you modify the title (especially remove "long-term"). You could rather emphasize the idea of the trend over [year 1, year 2] and inter-annual variability in this time window. You could probably shorten your discussion of the statistics, and it would be clearer if you could add a few words about decadal and multi-decadal variability.

Page 11, lines 2-3: "As we have only 15 years available for most of the stations, our time series is too short to draw firm conclusions on the presence or magnitude of a trend." Then, just below you compute trends. The way it is written is very confusing. I suspect you mean an "expected climatic trend" in the first instance.

Figure 8: It is important to add the residual obtained when removing the mean annual cycle and the linear trend to the time series, in order to be able compare it with its magnitude with the magnitude of the residual that you show.

I have more specific comments to come on a revised version of the manuscript where you would have taken into account my general and specific comments, as I think this manuscript should be published (perhaps as a two-part paper).

2018.