

Interactive comment on “The CM SAF ATOVS tropospheric water vapour and temperature data record: overview of methodology and evaluation” by N. Courcoux and M. Schröder

Anonymous Referee #1

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General comments

This paper introduces a data set for tropospheric water vapour and temperature based on space-borne measurements for 1999–2011. In particular the vertically resolved tropospheric water vapour data constitute a very valuable data set.

As stated in the paper, a strong motivation for considering tropospheric water vapour is the fact that it accounts for more than half of the present day green-house effect and is the most important gaseous source of infrared opacity in the atmosphere (Held and Soden, 2000; Schmidt et al., 2010). However, to avoid misunderstandings the paper

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should also point out in the introduction that atmospheric CO₂ constitutes the principal “control knob” governing Earth’s temperature, in the sense that atmospheric water vapour only makes the climate more sensitive to forcing by non-condensable green-house gases. It is also important for the paper to notice that water vapour at greater altitudes in the tropics particularly important (e.g., Dessler and Sherwood, 2009). However, in spite of its climatic importance, water vapour in the upper troposphere is only badly known (e.g., Hurst et al., 2011; Kunz et al., 2013), which makes the data set presented here particularly valuable. The authors might want to stress this point more strongly.

The data on the specific humidity at 200 and 300 hPa might be of particular interest when the issue of the present day green-house effect is investigated using this data set. However, the existence of this product is not mentioned in the abstract and its validation is not mentioned there as well (in fact there is rather little effort to validate this data product in the paper). I suggest putting more weight on this data product in the presentation throughout the paper. One could for instance show vertical profiles of averages of the data set (e.g., northern and southern hemisphere mid-latitudes, tropics, Asian monsoon region etc).

A further issue that needs more attention is the vertical resolution of the data sets. Table 1 list layers and pressure levels on which the data are reported, but this is not necessarily the true vertical resolution of the data set. I assume that in the optimal estimation algorithm used here, information on the vertical resolution should be available as averaging kernels. Why are those not discussed in section 2? Why are the averaging kernels not part of the provided data set? I suggest showing a figure presenting the averaging kernels.

There is also relatively little discussion of the ATOVS temperature data set in the paper. In contrast to the water vapour products, no attempt is made to quantify the uncertainties of the data set. A possible reason could be that the temperature data set is considered here only as an add-on, while superior temperature data exist. But if this is

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the case, then this point should be explicitly be made in the paper. Likely, the authors would then like to remove the term 'temperature' from the title of the paper.

One further point is that the paper should be more accessible to the non-specialist. One major problem here is the excessive use of acronyms, for example there are already 14 acronyms used in the abstract. The title mentions "CM SAF"; the information content of these letters is zero for the nonspecialist. The excessive use of acronyms makes the paper very difficult to read in certain sections. I understand that the names of many satellites are acronyms (e.g., ATOVS or AIRS). But the usage in the paper goes much beyond this points and forces the reader to constantly translate acronyms while reading the paper. As an example, followers of particular sports might not be confused about the use of terms like "Hibs and Hearts", "Gunners and Blues", or "05er and Kickers" when reading a text, but other people will have to constantly check again what is meant here. And you even introduce FCDR in the last sentence of the manuscript.

In summary, this paper needs work in the sense that the presentation should be re-worked throughout the paper. There could be a somewhat better motivation for improved water vapour products, the issue of the vertical resolution of the data should be better addressed and the balance between the presentation of the different data products should be improved. Also the quantification of the error estimates for the different data products presented here should be more balanced. However, I believe that a substantially revised paper on the ATOVS data products could be a very valuable contribution to ESSD.

Comments in detail

Title: the title should be changed: remove "CM SAF" (see comments above) and introduce the information of vertical resolution of tropospheric water vapour into the title.

Abstract, l. 1: The information about the release of the data should be dropped from
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the abstract.

Abstract, l. 15/16: it would be important to list the layers here explicitly. Or alternatively, give some impression of the vertical resolution and of the vertical range covered.

Last part of the Abstract: Only TWP comparison with sonde data is reported here; also the comparison for LPW should be given here. Further, the measurement geometry and spectral range (say IR and microwave) of ATOVS could be briefly mentioned here.

p. 129., l. 15: Give a bit more background about what GCOS is, who stands behind it?

p. 129., l. 21: water vapour is more than just one of 44, discuss the prioritises in the essential climate variables.

p. 130., l. 27: there are also retrieval schemes that do not use optimal estimation and those that do not use a-priori.

p. 133, l. 26: why was the reprocessing stopped in the year 2011 rather than continuing the time series?

p. 134., l. 13: this basic information should be given in the introduction already.

p. 135., l. 6: the HIRS 6.7- μm water vapour channel was changed to 6.5- μm for HIRS/3 (Shi and Bates, 2011); is this shift important for the time series presented here?

p. 135., l. 19, 20: please clarify: there is no wind retrieval here, so winds at 10 km cannot have been used as a-priori.

p. 137, l. 24: does this mean that surface pressure is a retrieval product? This is nor clear from the discussion in the paper so far. I suggest providing a list (or table) of all the retrieval products in above (e.g. in section 3.3).

p. 138, l. 17: the GUAN sondes are used here for testing the satellite data set. But how reliable are the GUAN sondes especially at greater altitudes, where water vapour concentrations are low, but where the concentrations matter most in terms of climate,

as discussed in the introduction.

p. 140., l. 3: what is the impact of not applying the corrections? Are they unimportant?

p. 142, l. 1, 2: reference for the TPW annual cycle?

p. 143, top paragraph: what are the error estimates for the TPW values? Are these differences significant?

p. 144: Why is the specific humidity product at 6 pressure levels not compared to the GUAM sondes in section 4.2.2.

p. 149, l. 24: is there a reference for the SSM/I TPW product?

p. 151: Section 4 ends here but without discussing a the temperature product at all. Why is there no discussion on the temperature data set (which is even mentioned in the title)?

p. 153, l. 8: it is not a conclusions that contributors have been discussed. Or is the conclusion that the bias remains unexplained in spite of the discussion?

Figures: The figures could be improved by dropping the dashed vertical lines which are present in many plots. I do not consider them useful. In any case they should not intersect with the legends. Probably the most easy solution is dropping them.

References

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