

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 #2

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Journal: ESSD Title: The CM SAF ATOVS tropospheric water vapour and temperature data record: Overview of methodology and evaluation Author(s): N. Courcoux and M. Schroeder MS No.: essd-2014-25 MS Type: Review Article

This paper is an attempt to summarize in one paper a large body of work performed at the EUMETSAT Climate Monitoring Satellite Application Facility to process more than a decade of historical satellite data and provide validation. The authors state that this may be the first consistent reprocessing of the ATOVS observation record between 1999 and 2011, however there are good reasons why a truly "consistent" reprocessing has not been performed to date and it's not at all clear that simply running the original

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data through a single piece of processing code leads to a consistent record. It is necessary but not sufficient. I would like to see more of the deficiencies identified and an expanded discussion of the future work implied by this paper. In my opinion, this paper is trying to cover too much material and so does not have sufficient detail in critical parts including a comprehensive discussion of uncertainties. Perhaps that is the subject of a future paper?

This paper introduces the CM SAF ATOVS tropospheric humidity and temperature data record. The ATOVS observations are consistently reprocessed with a fixed processing chain. This paper is based on the Algorithm Theoretical Basis Document (ATBD) and the validation report available at <http://www.cmsaf.eu/docs>.

Quoting from the ATBD: The core of the CM SAF ATOVS data set processing chain is the IAPP (International ATOVS Processing Package), a retrieval software which was developed by the University of Wisconsin in Madison, USA (Li et al., 2000). The IAPP needs ATOVS data and first guess data as input. Here, the ERA Interim reanalysis from the ECMWF (European Centre for Medium-range Weather Forecast) are used as first guess data. [...] the AMSU-B (and MHS) data are intercalibrated by applying SNO coefficients to the I1c brightness temperatures. [...] The IAPP is then fed with the ATOVS I1d data and the ERA Interim data (both arranged in 3-hourly time slot files). The water vapour and temperature profiles are retrieved on 42 pressure levels. A quality control is applied to the IAPP outputs and afterwards the profiles are sampled, integrated and averaged. Finally, a Kriging routine (Lindau and Schröder, 2010) is applied to obtain the daily and monthly means (together with the extra-daily standard deviation for the monthly means, the random error for the daily means, and the number of observations per grid point).

General Comments: The abstract and the conclusion use the words "optimal estimation" to describe the ATOVS retrieval. However this is not how either the ATBD or the Li (2000) paper describe the ATOVS retrieval method. The proper term is "maximum likelihood solution". These are very different retrieval approaches so more clarification

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on the methodology actually applied and the difference between these two methods is warranted for a peer reviewed paper. The lack of detail on the algorithm description, the details of the implementation, what observation channels are actually being used (microwave versus IR), how clouds are being handled, and the first guess dependence of the product makes the interpretation of the results difficult. The reader is left to guess at the important details and he/she may make the wrong assumptions. Certainly implying an optimal estimation approach when one is not being used is not helpful given the relatively low information content in the ATOVS spectral channel set.

Specific Comments:

#1. As a minimum, a table should be included in the paper containing all the fixed parameter thresholds used in the configuration of the IAPP and other relevant software. This table would provide the level of specific detail, similar to what is described in ATBD page 10 and 11, which would be needed to independently reproduce the results.

#2. Further detail on exactly which channels are used in the ATOVS retrieval for each satellite with details on which L1 channel brightness temperatures are original and which channels are "adjusted" using SNOs or other methods. The L1 adjustment method needs to be documented and referenced.

#3. The paper refers to the IAPP software as having been "tuned" against NOAA-15 data. The Li et al. (2000) paper describes in great detail the use of radiance bias adjustment obtained in an extensive study of observations minus calculations where the data is matched to radiosonde truth. This bias tuning accounts for several independent sources of error; 1) sensor calibration error for that channel, 2) radiative transfer error including both spectroscopy uncertainties and lack of knowledge of the spectral response function of the channel (SRF shifts), and 3) errors in the truth input to the radiative transfer model. There needs to be some clarification in this paper of exactly what radiance bias adjustments were made for each of the ATOVS channels for each satellite. This information should be included in a table so that it can be referenced in

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future publications as an area of future improvement. Some statement should be made as to why this radiance bias adjustment was NOT done for each satellite sensor and if it will be done in the future.

#4. My main criticism of the current CM SAF reprocessing approach is that a consistent set of L2 reprocessing needs to follow a consistent set of L1 reprocessing. The last statement of the conclusion needs to be expanded and stronger statement made that the results presented are only preliminary and whether the data can be used for climate studies. This paper should make a clearer statement about whether the current CM SAF ATOVS products can be used in climate studies by external users or not and what caveats and limitations apply. Presumably the purpose of publishing this paper is to provide scientific guidance to users of the CM SAF data.

#5 Include an expanded version of the Limitations section of the ATBD.

#6 Figure 1. This figure is very curious and troubling. The left panel is the mean TPW for a month using the combination of methods of ATOVS retrieval and spatial filling (kriging). It looks beautifully complete with exciting amount of spatial detail until you look at the right hand panel and see that there are almost no ATOVS observations going into the analysis in the tropics or mid-latitudes. The middle panel is the most troubling because the standard deviation of the days within the month have close to zero variability in the tropics where the water vapor amounts are largest. This is quite the opposite of reality where the largest variability within a month is where the TPW is largest and thus has largest variability in the tropics. I can not imagine how this can happen unless the same daily TPW is used in the tropical regions for the entire month as a background to replace the missing ATOVS data. This makes me question the value of the information content of the final product. This is very confounding and I hope the authors can explain this in a way that can justify why the method used is valid.

#7. Since this method appears to use ECMWF interim for first guess, I would expect to

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see at least one plot showing the difference between the ATOVS retrieval and ECMWF AND the final CM product and the ECMWF first guess. That at least would illustrate what adjustment (right or wrong) was made to the input profiles. A map for a month similar to figure 1 would be helpful. Adding ERA-I to Figure 4 also seem very appropriate.

#8 Figure 3. GRUAN. GRUAN is not a true global dataset so comparing global ATOVS or AIRS to a global GRUAN average makes no sense at all to me. This figure should not be shown. Please remove it. The discussion in the text is okay but showing the figure is just misleading.

#9. Figure 5. The analysis of figure 5 appear valid however it is not useful to combine tropical, mid-latitudes, and polar regions all into a single metric, especially in units of absolute water amount. Absolute water amount tends to be dominated by the tropical regions where the TPW is highest. As a minimum, these bias and rms data should be plotted versus TPW amount to show the correlation with water amount. You may find that there is a fractional error that is common among the different climatic regimes. By the way, why do I care about the "operational" product? Are you saying that users should not use it or what?

#10. Gruan and ECMWF I am not sure if GRUAN has been used in the ERA-I re-analysis. Can you clarify this? Would GRUAN still serve as independent source of reference in satellite based WV evaluation when ECWMF is used as the first guess? If the GRUAN sounding was effectively being used in the first guess of the ATOVS retrieval then the agreement at the GRUAN sites might under-estimate the actual bias and RMS error of the ATOVS retrieval. It's good that the AIRS comparison was included even though AIRS v5 (and v6) is also "tuned" to ECMWF. And ECMWF is using the same ATOVS microwave data heavily in it's assimilation! Finding truly independent validation data is not easy.

Discussion of Figure 10. Please include more description regarding the change of

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equator crossing times and why the NOAA-16 data is used beyond the operational time period of Mar 20, 2001-Aug 29, 2005 when it was in a PM orbit close in time to the AIRS orbit. Including a figure similar to the attached Fig 1 would be helpful for interpretation of the diurnal sampling of the satellites used in this study. This is probably the most important issue to address beyond the L1 calibration issues because there must be some feedback of cloudiness with time of day sampling and the ATOVS retrieval results. The actual TPW is probably not varying with time of day but the error contribution in the retrieval due to cloud fraction probably is.

Discussion of Tropical Land Surfaces on page 20-21. I do not disagree with the discussion regarding minima of -2% but would like clarification on whether microwave channels (which channels?) are being used in the ATOVS retrieval over land in in general. Could this be an inconsistency in between the actual microwave emissivity in tropical areas and what is assumed in the ATOVS retrieval? Some detail on the channel "fit" in the microwave and IR channels would be useful to understand where the information content is coming from in the ATOVS retrievals.

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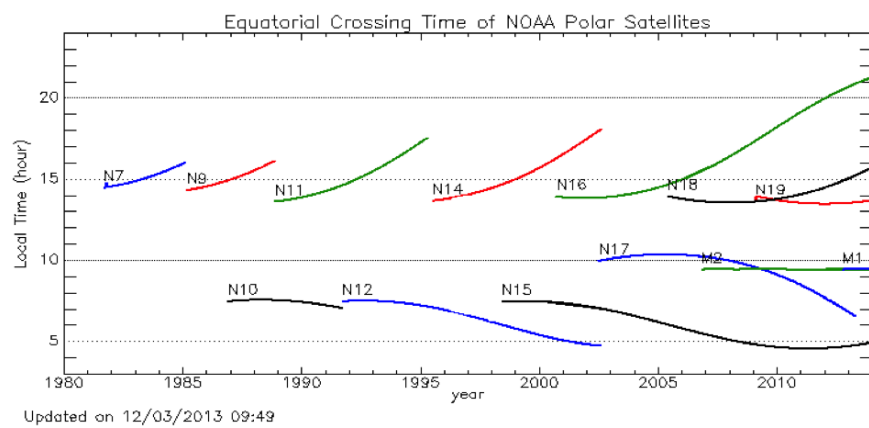


Fig. 1.