

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

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Anonymous Referee #2

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

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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 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?

Our understanding is that the objective of ESSD is the publication of papers related to data records. This is why the paper is structured in its present form. We agree that we need to be more precise when speaking of consistency. We will speak of "consistent reprocessing" and not of a consistent data record and will include the following cautionary note in section 2, page 133, after last sentence: "Though consistently reprocessed the ATOVS data record may not be considered as a consistent data record, mainly because the input data requires improved quality control and intercalibration". We will include a new section 5 in which we will discuss assumptions and known limitations: "The ATOVS data record was processed using a frozen processing system and up-to-date tools and retrievals. The ATOVS data record is suitable for the following applications: process studies, variability analysis, and model evaluation if the assumptions and limitations described in this section are kept in mind. The data record is not independent from the ERA-Interim model fields since those are used as input to the retrieval. Considering the weighting functions of the ATOVS instruments, the results in the lower troposphere over land surface may be significantly influenced by the model fields. Another related limitation is that the ERA-Interim model fields are not independent from ATOVS since the ATOVS data are assimilated in the reanalysis model. Different satellites are used to generate the data record and the number of satellites which are used for the processing also varies from one to four. The satellites have different local overpass times and some of them drifted with time - these two factors

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might affect the performance of the data record. Furthermore, the data exhibit a lower quality if only one satellite is used to generate the data record because the kriging routine then uses morning and afternoon orbits to estimate the local variance. This is only possible if the morning and afternoon observations are valid at the same location which reduces the number of valid observations. This impacts the quality of the first two years of the ATOVS data record. The quality of the product depends on the intercalibration of the AMSU-A, AMSU-B/MHS, and HIRS brightness temperatures. A missing or not optimal intercalibration might lead to artifact trends. A feasible intercalibration for AMSU-A and HIRS brightness temperatures was not available at the time of processing. Only intercalibration coefficient for AMSU-B channels have been applied for the time period 2001 to 2010 (John et al., 2012). AMSU-B/MHS brightness temperatures are intercalibrated using the SNO method described in John et al. (2012). It is shown in John et al. (2012) that the measurements taken into account in the SNO occur only at the poles, so that only a small part of the dynamic range of the global measurements is represented in the SNO. Consequently, potential non-linear effects as a function of scene brightness temperature are not considered. It has also been shown that there might be scan asymmetry in the AMSU-B brightness temperatures (Buehler et al., 2005 and John et al., 2013) what has not been accounted for here. The impacts of the kriging and the lack of intercalibration reduce the stability of the ATOVS product. This in combination with missing bias correction has a complex impact on the systematic error of the product and together with the limited temporal coverage makes this product not suitable for climate change analysis. The water vapour retrieval is not reliable in case of very elevated terrain (mostly in the Himalaya region), because in such regions, the sounders “see” through the entire atmosphere down to the surface and the signal is contaminated with surface contributions.”

We expand the discussion of future work in the conclusions: Page 153, top: “...in the literature. The bias between ATOVS and AIRS differs between the NOAA satellites and is minimal for NOAA-15. The next step that is needed to improve the ATOVS data record is the implementation of a bias correction scheme which needs to account

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for the various uncertainties of the retrieval including calibration uncertainties. It thus needs to be a function of satellite. This and other adaptations to the IAPP retrieval would require a close cooperation with the University of Wisconsin, also through the International TOVS Working Group.” Page 152, at the end of the second paragraph: “...variability analysis due to a questionable applicability of the kriging algorithm in presence of data from a single satellite. Further analysis is needed to quantify the bias potentially caused by sampling gaps in presence precipitation, as also recommended in Schröder et al. (2013) as a result of the third G-VAP workshop.” Include a new second last paragraph: “The provision of vertically resolved data in the upper troposphere is crucial for, among others, the analysis of the water vapour feedback. In order to ease comparisons and to enhance the reliability of related conclusions the provision of the retrieval uncertainty and averaging kernels at pixel level would be beneficial. In case of the gridded CM SAF product the first next step will be on the implementation of the retrieval error and on error propagation into the gridded product.” Adapt the last paragraph: “...will benefit from carefully quality controlled, recalibrated and intercalibrated sensor data. The development of such high quality Level 1 data is...CM SAF and EUMETSAT and within the European Union project “Fidelity and uncertainty in climate data records from Earth Observations”.” Add a last paragraph: “Metop-B is the last satellite that carries the ATOVS sensor suite. The processing needs to be adapted to account for a replacement of HIRS with IASI. The quality and usability would benefit from the inclusion of data from other hyperspectral infrared and microwave sounders and from a backward extension of the processing by implementing TOVS data.”

Due to the length of the manuscript and IAPP being published already by Li et al. (2000) and being freely available from the University of Wisconsin we decided to keep the description of the retrieval short. Nevertheless the level of details regarding the retrieval will be enhanced (see below for details).

Reference: Buehler S. A., M. Kuvatov, and V. O. John (2005), Scan asymmetries in AMSU-B data, *Geophys. Res. Lett.*, 32, L24810, doi:10.1029/2005GL024747. John,

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V. O., G. Holl, N. Atkinson, and S. A. Buehler (2013), Monitoring scan asymmetry of microwave humidity sounding channels using simultaneous all angle collocations (SAACs), *J. Geophys. Res.: Atmos.*, 118, doi:10.1002/jgrd.50154. Schröder, M., M. Lockhoff, and L. Shi, 2013: 3rd GEWEX Water Vapor Assessment (G-VAP) Workshop. GEWEX Newsletter, Vol. 23 No 4, November 2013.

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 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,

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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.

We admit that the term "optimal estimation" needs to be avoided in this manuscript. We will consistently speak of "maximum likelihood solution". The other comments are addressed below under "Specific Comments".

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.

The pages 10 and 11 of the ATBD are basically citing the Li et al. paper. In the configuration file of the IAPP, there are no tuning parameters, it defines mainly information on paths to the needed data. It is similar for the AAPP, with the exception that the grid reference needs to be specified, here HIRS. The grid resolution can be predefined in case of Kriging. We will add the following in the manuscript: - On page 136, line 3: after Labrot et al. "The default AAPP version was used. The HIRS pixel definition defines the "grid" for AAPP pre-processing." - On page 136, line 25 after (Li et al, 2000): "The default version of the IAPP was used, as no parameters can be tuned in the IAPP configuration file, which mostly contains path definitions for the different data needed for the retrieval." - On page 138, line 13, after Schröder et al (2013): "... , the only parameter tunable by the user in the kriging algorithm is the grid resolution."

#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

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which channels are "adjusted" using SNOs or other methods. The L1 adjustment method needs to be documented and referenced.

On page 135 lines 9 to 14 state in detail which channels are used in general for the 3 instruments (lines 9 to 10), and the particular faulty channels that have been removed from the retrieval for some given satellites (lines 10 to 14): "IAPP uses the following ATOVS channels: HIRS channels 1 to 17, AMSU-A channels 1 to 15, and AMSU-B channels 17 to 20. When an instrument channel became faulty on a specific satellite, this channel was removed from the retrieval for the entire reprocessing time period for that particular satellite. Such channels are: AMSU-A channels 11 and 14 on NOAA-15, AMSU-A channel 4 on NOAA-16 and AMSU-A channel 7 on Metop-A."

For the second part of the comment, only the AMSU-B channels are "adjusted" as it is stated in page 136.

To make it clear that the SNO are applied to the data of the 4 AMSU-B channels used for the retrieval, we will change the sentence on page 136, line 4 and 5 from : "Secondly, Simultaneous Nadir Overpasses (SNO) coefficients are applied to the AMSU B data to intercalibrate observations from the different satellites" to "Secondly, Simultaneous Nadir Overpasses (SNO) coefficients are applied to the data of the four AMSU-B channels used for the retrieval (channels 17 to 20) to intercalibrate observations from the different satellites". Furthermore, to make it clear that no SNO or any other adjustment methods have been used for the other instruments (AMSU-A and HIRS) we will add the following statement at the end of the paragraph on page 136, line 22: "...input, consequently, intercalibration coefficients have not been applied to the HIRS and AMSU-A data for the processing of the CM SAF ATOVS record."

The reference to the SNO used for the processing is the one cited page 136 line 6, John et al (2012). This paper explains how the SNO coefficients have been derived. Concerning, the way it has been implemented for the processing of the CM SAF ATOVS record, it is described on page 136, line 7 to 11. However, since the SNO coefficients

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described in John et al. (2012) are not provided in the paper but were provided by the paper's author, Viju John, we will add the following statement on page 136, line 7: "... and were provided (V. John, personal communication) as monthly mean...".

#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 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.

The bias adjustment was not activated during reprocessing and if so, it would have been valid for NOAA15 only. We will clearly state this in the updated manuscript and will include a paragraph on the consequences along the lines outlined in bullets 1-3 by the reviewer (page 137, top): "...the actual retrieval. The bias adjustment scheme is applicable to NOAA-15 data only. It has not been applied here because it has been anticipated that its application leads to a break point in the time series of the final products. The goal of a bias correction is to account for calibration uncertainties of the satellite data, radiative transfer uncertainties and uncertainties of the input to the radiative transfer. The deactivated bias correction can impact the number of convergent retrievals and the systematic and random uncertainties of the retrieved parameters. The retrieval involves...".

We will include an outlook paragraph which will include an outline of future plans on

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bias correction (see above).

#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.

As mentioned above we will stress the point that we have (only) consistently reprocessed the ATOVS data. We expand the conclusion by a series of topics for future work as outlined above. Two comments on the usage of the data are already part of the conclusions: usage of data from the first two years and over tropical land surfaces. Also as outlined above we will include a section on assumptions and limitations which also includes statements on the usability of the data record. There we comment that climate change analysis can not be done on basis of this data record.

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

We will include such a section, see above.

#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

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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.

The number of valid observations ranges from 42 to 371. The average number of valid observations in the tropics is ~ 80 . Therefore, the figure is truly complete and contains reasonable large number of values even in tropical areas where minima in number of observations need to be present due to the polar orbiting nature of the NOAA satellites. In order to improve the readability we have changed the valid range of the color code from 0 and 400 to 40 and 375. The middle panel of figure 1 shows the day-to-day variability within a single month. Our expectation is that this variability is dominated by atmospheric dynamics on daily scales, e.g. by storms. The extra-daily standard deviation based on the HOAPS climatology is shown in Schröder et al. (2013), figure 1. As it is based on the climatology the figure appears smoothed. Though the low values in the tropics are less pronounced in Schröder et al. (2013), the overall structure is similar to our results. The day-to-day variability is not affected by the diurnal cycle within a day (except by changes of the diurnal cycle from day to day). In a separate analysis and in support to G-VAP we are analysing the diurnal cycle sampling effect using GNSS data. Indeed, tropical areas exhibit a diurnal cycle in TPW. The monthly variability is shown in Trenberth et al. (2005), figure 6. Here maxima are associated to the movement of the ITCZ and the monsoons. These regions also exhibit strongest annual cycles as discussed in response to reviewer #1. Finally, large scale dynamical affects such as El Nino strongly affect the variability in the tropics (and elsewhere). We are confident that the day-to-day variability within a month in the tropics is small. Larger variability is present on smaller (intra-daily) and larger scales (monthly and climate variability).

References: Schröder, M., Jonas, M., Lindau, R., Schulz, J., and Fennig, K. (2013):

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The CM SAF SSM/I-based total column water vapour climate data record: methods and evaluation against re-analyses and satellite. *Atmos. Meas. Tech.*, 6, 765–775, doi:10.5194/amt-6-765-2013. Trenberth, K. E., Fasullo, J., and Smith, L.: Trends and variability in column-integrated water vapor, *Clim. Dynam.*, 24, 741–758, 2005.

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

Reviewer #1 wanted to see profiles of specific humidity due to the relevance of humidity in the upper tropospheric in climate context. This is why we propose to assess the difference between ERA-Interim and the retrieval as well as ERA-Interim and the final product for profiles of specific humidity. We computed averages of specific humidity over the northern and southern hemisphere, the tropics and the warm pool. We show classically averaged profiles of specific humidity for these regions together with the spatially averaged final product and the relative difference between averages of Level 2 ATOVS data and collocated ERA-Interim data and between the final product and ERA-Interim. We propose to add the following text in section 2, page 133, after second last paragraph: “Associated level 2 data is available on request. The level 2 data contains, among others, the specific humidity profiles on at 42 pressure levels. The left panel of Fig. 3 shows exemplary profiles of specific humidity for four different regions (northern and southern hemisphere, tropics and warm pool) and for September 2007. The profiles are computed as arithmetic averages over valid observations at levels smaller or equal to the surface pressure. The specific humidity of the final product is also plotted as asterisks. The specific humidity generally decreases with height and this decrease is most pronounced strongest at 450 hPa and above. The warm pool exhibits largest specific humidity and the northern hemisphere is generally

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more humid than the southern hemisphere both between surface and . This is true also in the upper troposphere up to 20150 hPa. The final product is typically more humid than the averages based on Level 2 data in all regions and at all considered levels for reasons discussed in section 4.2.1, except at 500, 700 and 1000 hPa over the southern hemisphere. The maximum difference is 0.17 g/kg (at 850 hPa, northern hemisphere) which explains why the differences are hardly visible in Fig. 3. The average differences between the ATOVS and the ERA-Interim profiles are shown in the right panel of Fig. 3. It illustrates the adjustment made by the retrieval to the input profiles. At near surface layers the changes are minimal what is likely due to the rather low information content in the observation. Noticeable is that this extends up to 650 hPa in the southern hemisphere. Largest reductions of up to -83% are found in the upper troposphere. While moving downward this changes to local maxima in increase of up to 11%. These maximum values are found for the warm pool. Also shown is the difference between the final product and the input data. These differences generally exhibit very similar features as the difference between the averaged Level 2 data and the input. We conclude that there are substantial changes by the retrieval in the upper troposphere and, to a lower degree, also between 800 hPa and 550 hPa. Whether or not these changes led to an improvement in quality can hardly be judged because the radiosondes are assimilated in ERA-Interim and, more generally speaking, due to the lack of, because a true reference with sufficient spatio-temporal coverage is not available.”

A new figure 3 will be included. Caption of Fig. 3: “Average profiles of specific humidity from ATOVS (left panel) and mean difference (bias) between ATOVS and ERA-Interim (right panel) for June September 20072. The regions are defined as follows: northern hemisphere (NH) - within 20°N and 50°N, southern hemisphere (SH) - within -20°S and -50°S, tropics – within -20°S and 20°N and warm pool – within -15°S and 15°N and within 90°E and 150°E. Specific humidity and bias are plotted only if the number of valid observations exceeds 75% of the value in the upper troposphere (e.g., a minimum of 230,000 for the warm the pool).”

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We have not added ERA-Interim to figure 4 because we want to keep the discussion of sampling and adjustments to the input separate and we want to minimise changes in topic.

#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.

We are using GUAN, not GRUAN as reference. The reviewer comment is nevertheless valid and we will remove figure 3.

#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?

We have analysed the scatter plot of bias and RMS versus TPW on basis of input data to figure 5. As it includes a mixture of climate regions it does not exhibit any correlation. We also computed the scatter plot between bias and TPW on basis of input to Figure 9. We found a correlation of 0.18 between TPW from AIRS and the bias between AIRS and ATOVS. At the small end of TPW values a slight linear dependency seems to be present. At larger values it is hard to observe such a dependency and in addition, a fractional error common to all climate regimes is hardly visible. It is clear that bias and RMS will have strong regional dependencies. We propose not to include a scatter plot but to include the following after the first paragraph in section 4.2.2: "The results shown in this section show bias and RMS based on all valid daily averages. Note that

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potential dependencies on climate regimes, TPW and other regional dependencies are not resolved here. We expect occasionally larger bias and larger RMS on regional scale." We refer here also to the discussion of the bias shown in Figure 9 which is extensively discussed in the manuscript. From this it is clear that global average values need to be interpreted with care. The reprocessed ATOVS data record has superior quality relative to the operational product. Thus, in case both products are available we recommend the utilisation of the reprocessed record. However, the reprocessed record ends in 2011 while the operational product is available to date with a delay of 2 month.

#10. Guan 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.

It is very difficult to find an independent reference. It seems that the only feasible way forward is to compare to more than one data record. Even then the intercomparison can be hampered by dependencies. As far as we know GRUAN data is not assimilated. However, GTS radiosondes are and these include GUAN data as well, if they pass the quality control. We will include a statement that the bias and RMS might be underestimated due to this dependency. We added the following text before the last paragraph of 4.2.2: "The GUAN radiosonde data and ATOVS are assimilated in the ERA-Interim reanalysis. Consequently, the bias and RMS of the comparison between the ATOVS data record and the GUAN radiosonde data might be underestimated due to this dependency.

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

On page 150 we have a discussion which goes in the requested direction. Note that on page 149 the reference to Fig. 10 should read Fig. 9 which has been corrected in the new version by also taking into account new and removed figures. We will enhance the text on page 151, line 13: "Also noticeable is the increase in bias for NOAA-16 between 2003 and 2009 and the decrease in bias after the maximum in 2009. All NOAA satellites typically have different equator crossing times and exhibit a drift in equator crossing time (see e.g. John et al., 2012, their Fig. 4). NOAA 16's orbital drift is the strongest and ranges from 14:00 local time in 2003, via 17:30 local time in 2009 to 19:30 in 2011. The AIRS orbit is stable with an equator crossing time of 13:30. Thus, at the beginning of the bias time series the difference in temporal sampling is minimal. In case the difference in equator crossing time would be the dominant contributor to the bias the maximum in bias can be expected at 19:30. It seems again that the diurnal cycle of deep convection in combination with differences in temporal sampling impacts the bias between AIRS and ATOVS. In addition to such a sampling error the retrieval uncertainty might also be affected by cloud handling which then results in a diurnal cycle of the retrieval error in presence of convective clouds. Also the bias for the data..." We think that it is sufficient to include a reference in which the equator times are given.

Discussion of Tropical Land Surfaces on page 20-21. I do not disagree with the

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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 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.

The sounding channels of AMSU-B allow the retrieval of water vapour also over land because the associated Jacobians hardly reach the surface. A major issue with the surface contribution to observations in the microwave spectral range is the unknown surface emissivity. In the microwave the surface emissivity is part of the state vector and is also retrieved. This largely relies on AMSU-A observations. Thus, microwave observations from AMSU-A and AMSU-B are used over land and ocean. The simultaneous retrieval of surface emissivity and the atmospheric state is very challenging and will lead to uncertainties. As discussed we assume that uncertainties arising from the unknown surface emissivity certainly contribute to the observed biases. It will occur that the retrieved emissivity will differ from the true emissivity.

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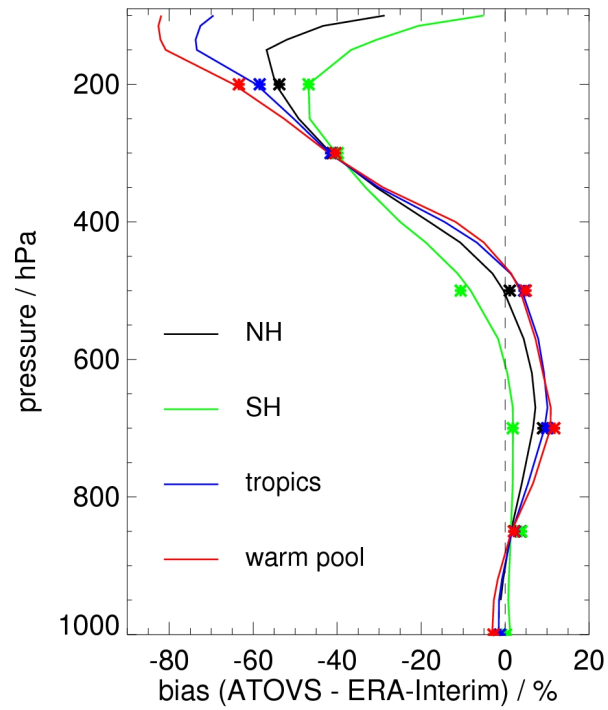


Fig. 1. Average profiles of specific humidity from ATOVS (left panel) and mean difference (bias) between ATOVS and ERA-Interim (right panel) for June September 20072. The regions are defined as follows: north

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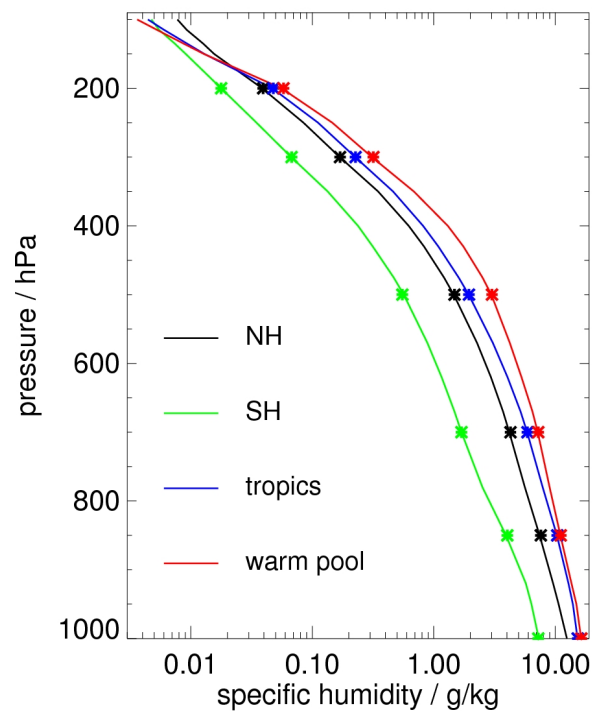


Fig. 2. Average profiles of specific humidity from ATOVS (left panel) and mean difference (bias) between ATOVS and ERA-Interim (right panel) for June September 20072. The regions are defined as follows: north

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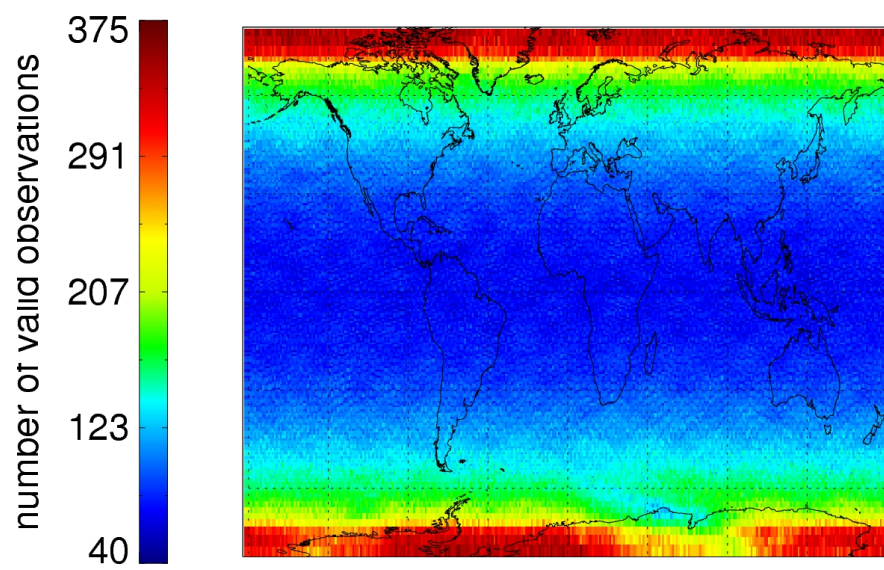


Fig. 3. #6 Comment on figure 1