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Interactive comment on “Climatology of free tropospheric humidity: extension into the SEVIRI era, evaluation and exemplary analysis” by M. Schröder et al.

M. Schröder et al.

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Author comments We want to thank the anonymous reviewer for comprehensive and thorough analysis of our manuscript. We carefully went through the reviewer comments and provide our answers in the text below.

Anonymous Referee #1 Received and published: 6 May 2014

1 General comments This paper presents work performed on $6.3\mu\text{m}$ data from ME-TEOSAT sensors MVIRI and SEVIRI. The work contributes to the GEWEX effort on establishing a homogeneous, quality controlled data base on water vapour in the free

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troposphere, G-VAP. Such work has been described in a number of internal CM-SAF reports. However it is of great public interest and it is welcome that the authors tried to make the results given in the internal reports available to a wider public. The topic is certainly appropriate for ACP. However, in its current shape the worth of the paper for the wider public is limited and I recommend major additions to make it more useful before it is eventually published.

2 Major comments P. 9607, II. 5-7: Although the bias and std. deviation values from Brogniez et al. (2009) look pretty unsuspecting, I am questioning their meaning. For the bias it is clear, but what does the std. deviation tell us? A typical profile of relative humidity has strong variation with moist and dry layers following each other in an intermittent fashion. If one would determine the standard deviation of RH(z) (weighted with the appropriate Jacobian or not), I am sure, the standard deviation would almost always be much larger than 1.7%. Thus the question is for me whether the quoted value has any concrete meaning at all. What is its significance?

Brogniez et al. (2009) computed the difference between the FTH retrieved from METEOSAT measurements and the FTH estimated from the RH profiles of the ARSA database within each month of the period 1984-2005. The bias of -1.2 %RH is the mean difference of the monthly means over the 20 years period, and the standard deviation of -1.7 %RH is the standard deviation of the differences over the 20 years period. The standard deviation of the difference is used to describe the long term stability of the record. In no way these values give an information of the RH(z). FTH itself is a vertically weighted relative humidity and no information on RH(z) can be estimated from a value of FTH. The quoted bias and standard deviation only give an insight of the temporal stability of the 1984-2005 dataset of FTH from MVIRI observations when compared to a quality-checked set of radiosoundings. In order to clarify this point and to avoid any misunderstandings, the paragraph has been rephrased:

“The mean difference between the MVIRI FTH and the ARSA FTH over the period 1984-2005 is -1.2% RH and the standard deviation of the difference is 1.7% RH, indi-

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cating the stability of the MVIRI archive over this period.”

P. 9607, l. 22 to P. 9608, l. 14: This discussion is incomprehensible. The last two sentences seem to say that FTH data records are preliminary until the full effect of CO2 doubling becomes established in the atmosphere. Do you believe your data only when they confirm the distributions and tendencies seen from climate model simulations?

We agree that the last two sentences can easily be misinterpreted. It was not our intention to question the quality of the data nor was it our intention to propose to measure the data quality by comparison to climate model predictions. The main point is that the changes observed in models emerge when a 100 year prediction is considered while satellite data records typically cover ~30 years or less. This point has still been made even when the last two sentences are removed. Thus, we have removed the last two sentences of this paragraph.

P. 9614, Discussion on Jacobians: Unfortunately I find here the same almost meaningless discussion of the Jacobians as in the cited paper by Brogniez et al. (2009), that is, the quote of that paper is futile for the reader. Given profiles of temperature and humidity (mixing ratio or any other concentration measure), it is the solution of the radiative transfer equation that yields the brightness temperature. This solution should be more or less unique (apart from numerical issues like vertical resolution, number of angles, wavenumber resolution, etc.). I cannot see where the degree of freedom comes from that causes the existence of essentially different Jacobians for the same set of profiles (T and q). If the radiative transfer equation can be formulated with the use of a Jacobian, shouldn't that be unique as the solution itself? If different Jacobians are possible by switching between coordinate systems for instance, shouldn't they all be equivalent? Are these differences that you discuss more than simply numerical noise? The paper could gain a lot from a thorough discussion of these questions. This might be given in an Appendix.

We disagree with the statement that the discussion of Jacobians is almost meaning-

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less. Indeed, given a single set of input data (T , RH ,...) radiative transfer leads to a unique solution in radiance space when numerical noise is ignored. However, during the retrieval design several options exist on how to retrieve the information from the BTs. This is reflected in many publications since the 90s, and already in 2001 Jackson and Bates discussed the use of different weighting functions. In this manuscript, it is recalled here that various weighting functions have been utilised. Again the different definitions of Jacobians are not used for radiative transfer computations. Instead they are applied in the training of the retrieval scheme: In order to define the regression coefficients the $RH(z)$ needs to be properly weighted. P 9614, ll 15: We have changed “definition” into “retrieval”.

P. 9618, bottom: The paper would be much clearer to the reader if you would give mathematical definitions to all statistical quantities mentioned. This may be given in an Appendix as well.

We provide a definition of relative bias, bias corrected RMSD, decadal stability and correlation in the Appendix.

3 Minor comments P. 9610, 2nd par. of Section 2: It took me quite a while to understand (hopefully correctly) that the ISCCP dataset contains Meteosat 2-5 and 7, while the LMD dataset contains Meteosat 8 and 9. This should be written more clearly so that it can be grasped at first reading.

We have re-arranged the paragraph and think that the source of the data is described more clearly now.

Equation (1): It looks as if data before and after the break are corrected by the same factor. What do I misunderstand here? Or is the correction only applied after the break? If so, please say it.

The correction is only applied after the break. This is now mentioned in the text.

P. 9613, last line, and P. 9614 first line: a) for what do you need the seasonal cycle

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(seasonally varying regression?); b) how is it possible to represent a seasonal cycle by just the four initial days, but then, strangely, with four steps per day?

We have rephrased the paragraph. The point is to include samples of various local times, seasons and years to cover the various temporal scales. This way chances are enhanced to cover a large spectrum of different atmospheric conditions.

P. 9614, I. 6: Are there indeed cases with $RH > 100\%$ in the reanalyses? Or does this occur after application of RTTOV and application of the Soden-Bretherton formula on the resulting BT?

Supersaturation is allowed in ERA-Interim (Dee et al., 2011). Here, reanalysis is screened to exclude such cases prior to the application of RTTOV.

P. 9616, I. 11-13: What do you mean with “uncertainty varies ALONG the design of the algorithm” and what with “space/time accumulation”? Please reformulate.

Changed into “depends on details of the underlying algorithm.” Further details are given in the next paragraph. Changed into “temporal and spatial averaging.”

I. 15: You could help the reader if you quote typical values of correlation lengths.

Done.

II. 18, 19: As $d \ln(FTH) = dBT = a$, why should the relative uncertainty in the given case be b ? It should be a .

The calibration uncertainty leads to a systematic difference. Thus, the intercept (and not the derivative) is the uncertainty which needs to be transferred from absolute units into relative units.

II. 26, 27: I understand that this is error propagation of independent contributions. As we know, variances from independent contributions add to the total variance. Its square root is typically termed sigma. To give a value of sigma “at one sigma” sounds strange to me.

We are not able to understand the comment of the reviewer - “strange” is not well defined. We use a classical terminology in statistics which expresses the uncertainty at 1, 2 or 3 sigma.

P. 9617, Section 6.1: Please explain what ARSA is. Is it an archive of radiosonde data or what else? Also in line 12 add that A4 is used to compute clear-sky radiance from the profiles.

The first sentence has been changed into:” The ARSA version 2.7 is an archive of global radiosonde measurements of temperature, water vapor and ozone profiles, which have been quality controlled and combined with auxiliary observations.” We do not see the need to include the second statement because it can be misleading. Later we use RTTOV to compute clear sky radiances from the profiles.

P 9618, ll. 9-12: Since I do not know what ARSA is, I cannot understand this paragraph.

We think that this should be clear now given the above updates.

ll. 14, 15: There are more error sources in radiosonde humidity records than just the radiation error. Are these taken into account?

This is true and they are not accounted for. Note however that only night time radiosoundings are used to avoid the artificial dry bias induced by the solar radiative heating on some sensors.

ll. 17, 18: I wonder why you can throw away data pairs with a large difference in a validation exercise.

In order to have robust statistics outliers are frequently removed from the data base. Typically a value of 3-sigma is applied as threshold. Thus, throwing away data pairs is common practice. Here the main motivation to apply a threshold of 3 K is to minimise cloud detection uncertainties because it is our intention to characterise the quality of the FTH product and not of the cloud mask. Misclassified clouds will have a large impact on the statistics and will blur the “true” uncertainty of the FTH product.

P. 9619, I. 14: “main difference” of what?

We inserted “between the comparisons performed in the FTH space and in the BT space”.

I. 16: Note that the word “minima” applied to negative quantities can be misleading. While you mean minima of the absolute values, “minimum” usually would imply the most negative (or least positive) value.

We mean “most negative” in this context. We added “with values down to almost -15%”.

P. 9620, II. 10-15: I cannot follow your explanations and would like to have a better and more detailed explanation. Part of the problem is that “decadal stability” is not defined (cf. major comment of missing mathematical definitions of statistical notions). I have no idea, for instance, what % per month means here.

As mentioned above we will define the statistical parameters. The bias is given in relative units and may change over time. When this change is computed using linear regression based on the results given in Figure 6, top panel, the change in bias with time (decadal stability) will have units of %/month. By simply applying a factor of 12 this is transferred into units of %/year.

P. 9621, I. 20: What is a “confidence probability”? Do you mean a confidence level or a confidence interval? This strange notion appears often in the paper and should either be defined or replaced.

We meant “coverage probability”. At first appearance, we now say “coverage probability or level of confidence” and will then consistently speak of “coverage probability”.

P. 9622, II. 15-17: the two statements “dry composite has its main origin in the tropics” and “wet air mainly originates in the tropics” seem to be inconsistent. Also, it is not clear what you mean with “dry composite”.

Text changed into: “Brogniez et al. (2009) analyzed the FTH from MVIRI over northeast

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Africa over the period 1983-2004 for the months of July/August and separated the analysis into dry and wet years. The air masses of the driest years have been shown to...”

P. 9623, ll. 2 and 10: The correlation values look quite small and thus either irrelevant or statistically insignificant. Be careful not to interpret statistical noise.

We agree. This is why we conclude that El Nino and QBO do not significantly contribute to the variability.

ll. 23-25: Can you please say which kind of statistical test you are describing here?

We tested if the signal, that is, the difference between FTH from 1990s and the 2000s, is larger than the noise, that is, the square root of the sum of the standard deviations of FTH from the 1990s and 2000s. This is simply done by considering the ratio of the difference to the noise.

P. 9625, l. 8,9: Which oversimplifications?

In the extra-tropical environment, the assumption that a constant lapse rate can be used in deriving the equations is no longer valid. Such an assumption can be seen as an oversimplification of the retrieval of FTH in a midlatitude environment. Results should hence be interpreted with care. We have adapted the text accordingly.

ll. 19,20: I agree that many years of data are needed to detect trends in noisy time series with statistical significance. But that is all! The part of the sentence “allow for a verification of climate model output” should be deleted. First, your data base has a merit on its own and it is not necessary to mention climate models at all in this respect (cf. 2nd major comment from above). Second, a climate model cannot be verified, as a matter of principle!

It is true that the last sentence of this paragraph may unnecessarily question the value of the data. Therefore, the sentence has been removed.

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4 Technical comments

P. 9605, l. 18: although it might be clear, complete the statement by saying “the full probability distribution of ...” (of what?).

“of RH ” added.

l. 25: broad range of scales (plural).

Done.

P. 9606, l. 21: replace “adjusted” with “applied”.

Done.

P. 9607, l. 6: expand ARSA.

Done.

P. 9608, l. 4: explain FTHp10.

Done.

P. 9609, 1st par. of Section 2: You say that you will describe radiance data, reanalysis, and RTTOV in THIS section, but evidently only the radiance data are presented. Please rephrase.

We have changed this paragraph into: “This section briefly describes the instruments and the radiance input data sets used to retrieve the FTH.”

P. 9610, l. 19: Add BTs after Meteosat-9 (or is the satellite itself simulated?).

Done.

P. 9614, l. 23: adapted appropriate.

Done.

P. 9615, l. 1: highlights.

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Done.

P. 9619, I. 4: Rephrase: as it stands, the number of observations are 170%.

Changed into: “-3.2%, 16.8% and 170”.

I. 6: Give the value of the GCOS requirement.

We added “for FTH (5%, verify GCOS-154)”.

P. 9626, I. 16: extent.

Done.

P. 9632: reference Engelen et al. is at the wrong place here.

The reference has been removed.

Figures: could be larger, in particular Figure 6 is hard to read.

Figure 6 and figure 8 have larger font size now.

Figures 4, 7-13: It will be easier for the reader if the season triplets (“DJF” etc.) would be printed in each panel. In particular, as there seems to be an inconsistency between Fig. 4 (not clockwise) and Fig. 8 (you say clockwise, but I doubt whether it is correct). Please check and order it in the same way in all figures.

We included the season in figures 4, 7-13.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9603, 2014.

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