

## Author comments

We want to thank the two anonymous reviewers for their comprehensive and thorough analysis of our manuscript. We carefully went through the reviewer comments and provide our answers in the text below – our response is given in italic and blue.

## Anonymous Referee #1

Received and published: 6 May 2014

### 1 General comments

This paper presents work performed on 6.3 $\mu$ m data from METEOSAT 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 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, ll. 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, indicating 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 CO<sub>2</sub> 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 meaningless. 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 (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, l. 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, l. 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.”*

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

*Done.*

ll. 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.*

ll. 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, l. 14: "main difference" of what?

*We inserted "between the comparisons performed in the FTH space and in the BT space".*

l. 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, ll. 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, l. 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, ll. 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 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.*

#### **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.

*Done.*

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

*Changed into: “-3.2%, 16.8% and 170”.*

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

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

P. 9626, l. 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.*

## **Anonymous Referee #2**

Received and published: 12 May 2014

### **General comments**

In this study a long time series of satellite retrieved relative humidity, obtained based on METEOSAT observations in the free troposphere, is presented. Water vapour is the most effective greenhouse gas and is very important for the Earth's water cycle. In addition, water vapour has probably an increasing role due to the global warming and a positive feedback loop. Thus, it is important to establish a long data record to be able to investigate these issues and therefore the present study is important. So this study is relevant for ACP, however, the current version need to be improved and a major revision is recommended.

### **Major comments**

In the beginning of the introduction a motivation for the present study is presented, thus, stating the important role of water vapour in the Earth's energy balance and for the water cycle. In the present study, merely the relative humidity has been analyzed. It is not obvious how the present results of changes in relative humidity can be link to this important greenhouse gas. A change in RH due to the global warming could be due to either a change in water vapour or temperature, depending on the region of interest. In the manuscript, at many places, it is not clear that it is actually purely RH that has been investigated here (e.g. the title of the manuscript).

*The reviewer is right in saying that FTH is a function of temperature and mixing ratio. This is mentioned in the abstract (p9694, ll 11+12.). We made this more clearly in the updated version by changing the following:*

*We added "FTH is the mean relative humidity (RH) in a broad layer in the free troposphere." in the abstract.*

*We also think that the understanding of the manuscript can be improved by removing the first two sentences in the introduction. We further slightly adapted the first sentence of the third sentence: "The importance of relative humidity (RH) in the free troposphere originates from the non-linear interaction between humidity RH and longwave radiation."*

*Also, the impact of temperature and mixing ratios on RH and FTH was further discussed on p9607, ll22. This paragraph became now the second paragraph in the introduction.*

The language is sometimes somewhat confusing and need to be improved, language is more clear in some chapters and less clear in others. Some suggestions are presented in "Specific comments" below, however, the full text needs a English proof-check.

*The manuscript has been proof-checked by a scientific editor and many changes have been implemented.*

### **Minor comments**

1. Page 9614, paragraph 2 of Section 4. I wonder how accurate it is to use ERA reanalysis data here for the cloud screening, due to the poor horizontal resolution and uncertainties in estimated cloud fraction. Since at least the SEVIRI perform observations at visible wave lengths these data could be used for cloud screening.

*The ERA-Interim cloud mask is used only to build the training set for the determination of the fitting coefficients of the retrieval and not for the generation of the FTH product. This way we have a consistency between the thermodynamic profiles and the cloud information.*



*The ISCCP-DX cloud mask is used to screen out high and mid-level cloud cases in Meteosat observations for the generation of the Meteosat FTH data record.*

*Using the visible channels on SEVIRI is not an option because SEVIRI observations do not cover the considered period nor is the visible information available during night.*

2. I do not understand equation (1). It seems that the data before and after the break should be corrected with the same factor, but then the factor “a\_before/a\_after” is applied only on “b\_after”. Is it that the latter factor should be removed?

*We added that the BTs are modified “after the breakpoint”.*

3. The treatment of the abbreviations is confusing. Since so many have been introduced please include a list, so it is easier for the readers to find what they stand for. Even so, if a "important" abbreviation is used only few times after it has been introduced, e.g. ML and PL, it is not necessary to introduce it at all. If an abbreviation has been introduced use it consequently, e.g. “RH” instead of “relative humidity”, in the remaining text. Define an abbreviation at the first place where it has been introduced, e.g. FTH and BT in the introduction, as well in the abstract. Note that the abstract is separated from the main text.

*We agree and now include a list of abbreviations in the Appendix. Abbreviations are introduced at first occurrence and are used consequently.*

Suggestion: the “FTH” should be “FTRH”, but probably better with “RHFT”. The latter abbreviation: "RH" should be in normal size and "FT" in subscript (also below).

*Indeed the term “upper tropospheric humidity”/“free tropospheric humidity” and their abbreviations UTH/FTH have been used for more than 20 years in the scientific community, be it in conferences or workshops or in scientific publications. As our FTH product is a heritage product we do not want to change this.*

4. A description of statistics used need to be included. For example, results of absolute and relative bias are obtained of the data analyzed. It is not clear how it is calculated, particularly the relative values.

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

5. End of page 9613, What is meant by "samples of the seasonal cycle with the 1<sup>st</sup> day of the months....." How representative is the obtained seasonal cycle based on a single day of the month?

*We agree that the reader can misunderstand this point. 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. We have thus rephrased the paragraph.*

6. page 9618, lines 17 and 18, How does differences in absolute BTs minimize cloud contamination?

*High level clouds strongly impact the observed BT at 6.3 microns. The observed signal will not be dominated by FTH but by the cloud. A high level cloud that has been classified as clear*

*sky will therefore result in BT difference between observed BT (affected by a cloud) and simulated BT (not affected by a cloud) of typically more than 3 K. 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.*

7. Page 9612, lines 4 – 7, The two last temperature values, 4.5 and 0.8 K, seem not agree with the results in the figure.

*We think that the definition of the “difference” was not clear. Thus we have rephrased the paragraph: ” Figure 1 shows the deseasonalized anomaly of the original and the updated BT as well as their difference. The intensity of a breakpoint is the difference between the anomaly difference (black values) prior and after the breakpoint. The breakpoints in January 2001, in July 2006 and in May 2007 have the following intensities: 0.5 K, -4.5 K, and 0.8 K, respectively.“*

8. Page 9616, lines 3 and 4, Not completely convincing concerning the outliers. “Such outliers” could be marked in the figures.”

*As suggested, we have changed the text to point explicitly at the outliers: “Exemplary outliers are observed in March 1992 (in the South Atlantic) and in April 1990 (over northeast Africa) and...”*

9. Section 6.3. Figure 6a present relative bias with negative values. Please give the expression used to calculate the relative bias.

*Following the comment #4, we now provide a definition of relative bias, bias corrected RMSD and decadal stability in the Appendix.*

Line 9, What is meant by “The temporal correlation” ? If it is “R” then write “No correlation (R = -0.01) is found between N and relative bias.”

*Done.*

Line 19, Should it be “relative RMSD” ? Check the full manuscript. Suggestion: introduce “NRMSD (Normalized root mean square deviation)” beside “RMSD”.

*We do not wish to introduce this new abbreviation and prefer to say “relative RMSD”. We thus inserted “relative” here and propagated the term throughout the manuscript.*

Line 24. The unit “%RH” is confusing and since the word “absolute” is used it is actually clear what is meant. Suggestion: “The time series averages of absolute bias (“better with” differences ?) and RMSD (Fig.6, third panel) for the RHFT are -1.2% and 5.0%, respectively.”

*The units have been defined on page 9615, l 19 and we consequently apply these definitions. Starting at line 24, we stop discussions of relative values (in %) and start the discussion of absolute values and thus think that it is helpful to emphasise this with the notation %RH to avoid confusion.*

Lines 16 and 27, What is meant by “normalisation” ?, not explained in the text.

*This is now explained in the new Appendix.*

Lines 26 and 27, The explanation sounds realistic, but the increase in FTH is it real or bias? Maybe an explanation for what “normalisation” stands for will help me here.

*Following the previous comment, the term “normalization” is now explained in the Appendix. Here we discuss the fact that the absolute bias and RMSD do not exhibit an increase, only the relative RMSD does. We normalize relative to the ARSA FTH values. Thus, ARSA might exhibit an increase in FTH which has not been explicitly analysed here – we focus on the FTH from Meteosat.*

*In section 7.3 we discuss the temporal change in our FTH product.*

10. Page 9625 and line 15, Could it be worth to investigate 1 month instead of 3 months period? For this, one particularly month could be chosen in a test.

*Starting with Figure 4 the analysis consequently considers seasonal averages. In order to keep readability and in order to allow for a consistent interpretation we do not want to include Figures 8, 11 and 13 for a specific month.*

*It is however true, that though the trends have been computed on basis of seasonal averages, intra-seasonal variability contributes to overall uncertainties. We included a corresponding statement at the beginning of section 7.*

### **Specific comments**

Page 9604 Line 1, Suggestion: “A new free tropospheric relative humidity (RHFT) data...”

*As stated earlier we do not want to change the term “free tropospheric humidity (FTH)”.*

Line 2, “with” instead of “of”

*Done.*

Line 6, “data record covers the period 1983-2009, with a...”

*Done.*

Lines 12 and 13, “Under the given assumptions constant...theory it means...”

*Abstract has been shortened.*

Line 19, “The RHFT product is compared to computed relative humidity, obtained based on Analysed...”

*We changed this sentence into: “The FTH estimated from the Meteosat observations is compared to the FTH computed from ...”.*

Lines 21-22, Suggestion “and normalized root mean square deviation (NRMSD)...are obtained. The NRMSD confirms.....”. Introducing NRMSD will help when it is presented in the figures. e.g. Figure 1. Otherwise you have to write "relative NRMSD" in the figure.

*As answered earlier, we do not want to introduce this new abbreviation and prefer to use “relative RMSD” as it is already the case in figure 6.*

*We inserted “relative” also in the abstract. We do not understand the reference to Figure 1 since no normalization is performed in this Figure.*

Line 26, Suggestion: "RH10FT". The latter abbreviation: "RH10" is in normal size and "FT" in subscript .

*The term FTHp10 has already been introduced in the literature (Roca et al., 2012). Therefore, we do not want to change it.*

Page 9609

Line 8, Is it ok to write "Sects." instead of "Sections"?

*This was introduced by ACP.*

Lines 15-18, Only radiance data are presented in this section. This part may be integrated into the last paragraph of Section 1, or has to be rephrased in some way.

*We agree and have rephrased the first paragraph: “This section briefly describes the instruments and the radiance input data sets used to retrieve the FTH.”.*

Lines 20-21, “Meteosat-7, which belong to the first generation of Meteosat satellites.”

*Done.*

Line 21, Remove the second “orbit”

*Done.*

Line 26, "channels that cover the"

*Done.*

Lines 27-, “on board the geostationary satellites Meteosat-8 and Meteosat-7, which are positioned ...”

*Done.*

Page 9610 Line 1, I do not understand why “while in operational mode” is written here.

*It may happen that two satellites are available at nominal position (that is, over Africa). This is the case with Meteosat-8 and Meteosat-9. Then, one satellite needs to be slightly moved away from 0°.*

Line 2, "present day."

*Done.*

Lines 4 and 5, "at length" ?

*Has been removed.*

Line 6, "Meteosat-2 - Meteosat-5". Suggestion: "Meteosat2 - Meteosat5", and then at all other places in the text. Note that "Meteosat" is in normal size and "2" and "5" could be in subscript

*Done and we keep the terminology introduced by EUMETSAT: the number of the satellite is thus kept in normal script.*

Line 7, "Cloud"

*Done.*

Line 8, "DX level"? Rephrase the content in the paragraph.

*Text changed into: "(ISCCP, Rossow and Schiffer, 1999) at the DX pixel resolution (ISCCP-DX, see <http://isccp.giss.nasa.gov/products/products.html> for details)."*

Line 13, "ISCCP-DX" ?

*Now the abbreviation is explained at first occurrence.*

Line 14, "in sensor resolution"?

*This has been removed.*

Lines 18 – 20, A R2 value should be presented, and a suggestion: move“(not shown)” after “excellent linear behavior”. Scatter plots of what?

*The correlations are >0.99 which is now also mentioned in the manuscript.  
Changed into “... exhibit an excellent linear behavior with correlations >0.99 (not shown).”  
These are scatter plots between the BTs from various Meteosat platforms. We have included “simulated BTs”.*

Line 24, “...homogeneous and exhibits...”

*Done.*

Line 26, "The work by Picon"

*Done.*

Page 9611 Line 1, Write out “ECMWF”

*Done.*

Line 3, “Meteosat-5-like” ?

*This term means that the BTs have been adapted to the Meteosat-5 spectral response function. As this has been mentioned in section 2, we drop “-like” here.*

Page 9612 Lines 4-5, "The difference in BT for January....."

*This paragraph has been largely rephrased.*

Line 8: “possible” instead of “potential”

*Done.*

Line 12, "The magnitude of the observing period" ? Change to “are different, but”

*A suggested, the order has been changed.*

Page 9613 Line 9, “.....BT at the 6.3  $\mu\text{m}$  to the mean RH (defined with respect to water only).....”

*Done.*

Lines 10 and 11, "Equation (2) also correct for the satellite viewing angle ....."

*Changed into “Eq. (2) also corrects for the effect of the satellite viewing angle  $\theta$  and includes...”.*

Line 12, “and” instead of “to” ?

*Changed into “ratio between ... and...”*

Line 16, Remove “(ML)”

*Done.*

Line 17, Remove “(PL)”

*Done.*

Line 20, Change “ML” to “model levels”

*Done.*

Line 22, "sampling the field of view" ?

*We keep “satellite” because the term “field of view” is frequently used for a single pixel.*

Line 25, “of clear sky profiles” ?

*The formulation may induce confusion. This is modified to “this training database is composed of (...) from ERA-Interim restricted to clear-sky profiles.”*

Lines 24-, “...covering the seasonal cycle with the 1st day (4 time steps per day) of the months January, April, July and October corresponding to the years 2001, 2006 and 2007.”

*We have rephrased this part.*

Page 9614 Line 1, “The clear sky profiles

*Changed into “The clear-sky cases are...”*

Line 11, “due to problem with cloud detection.”

*Done.*

Line 13, “700 hPa, since”

*Done.*

Line 17, “RH, for which the weights”

*Done.*

Line 27, Remove “the”

*Done.*

Page 9615 Line 1, "highlights"

*Done.*

Line 14, What is meant with "straightforward" ?

*Changed into “arithmetic”.*

Line 18, either “period March 1997 – May 1998” or “period from March 1997 to May 1998”

*Modified as suggested*

Line 19, Confusing. By introducing instead NRMSD (see above) beside RMSD for RHFT, and writing absolute and relative difference for RHFT this will be clear what is meant (see also the comment to Figure 6 below).

*Here we only introduce the units. These are valid for FTH AND statistical parameters. We think that this sentence contains a clear statement.*

Line 21, “...series of seasonal...”

*We changed into “Figure 4 illustrates the FTH seasonal averages...”*

Lines 20- 24, “Figure 3 shows examples of instantaneous and monthly average products. Strong minima in FTH over northern and southern Africa as well strong maxima in FTH at the Inter Tropical Convergence Zone (ITCZ) are evident during the boreal summer. Furthermore, Figure 4 illustrates the seasonal averages in FTH. The figure shows that the location, extent and strength of the humidified and dry areas highly depend on season.”

*Changed into: “Figure 3 shows examples of instantaneous and monthly averaged products. Figure 4 illustrates the FTH seasonal averages featuring strong FTH minima over northern and southern Africa during boreal summer and strong FTH maxima in the Inter Tropical*

*Convergence Zone (ITCZ). The location and the extent of dry and wet areas and the corresponding minimum and maximum FTH values strongly depend on the season.”*

Page 9616 Line 1, “large” instead of “strong”.

*Done.*

Line 11, “difference, while”

*Changed into “difference, whereas”.*

Lines 11 and 12, what is meant by "varies along the design of the algorithm."

*Changed into “depends on the details of the underlying algorithm.” Further details are given in the next paragraph.*

Line 12, “representativeness” ?, should it be “sampling uncertainty”?

*We mean “representativeness uncertainty” here.*

Lines 21 and 22, "tropical training a RMSD of 2% .....and an average absolute difference of 0.3% were estimated.

*We changed into: “Based on the tropical training a RMSD of 2% RH (8% when assuming an average FTH of 25%) and an average difference of 0.3% RH were estimated.”*

Line 23, “20% yield a 10% relative” ?

*Yes-done.*

Line 24, “uncertainty in the estimation of mean FTH is”

*Done.*

Line 25, Remove “so” ?

*Done.*

Page 9617 Line 14, “with as much as” ?

*Typically more than 10,000 observations per month are available in ARSA.*

Line 17, “...2009), showing.”

*The paragraph has been removed.*

Lines 21-23, “...Meteosat-5 observations the Radiative Transfer for the TIROS Operational Vertical Sounder (RTTOV) 9.3 model has been applied to ARSA (Matricardi et al., 2004). The RTTOV uses...”

Page 9618 Lines 4 and 5, “...as the calibration uncertainty of ~2K (e.g. Stephens..”

Line 6, “(2003), which is....user guide, the uncertainty”



Line 12, “are considered in the comparison”

Line 13, “applied for the validation:”

*The paragraph has been largely rephrased.*

Line 14, It is not clear what is meant with “remaining”. Suggestion: “night time only to avoid possible problem with radiosonde quality...”

*Change into “Night time only”.*

Line 21, Suggestion: “contains radiosonde measurements from ships..” otherwise “...contains measurements from radiosondes launched on ships and at small islands...”

*The second suggestion has been implemented.*

Lines 23-27, Not clear written, rephrase.

*We have included more details in the appendix.*

Page 9619, "are -3.2%, 16.8% and 170, respectively."

*Done.*

Page 9621 Line 3, “PDF” ?

*We changed into “Probability Density Function (PDF)”.*

Line 10, “more than 70%”, Is this the cases really for the months September-May?

*It means that more than 70% of the monthly means have valid FTHp10 values in the tropical South Atlantic, as defined in Figure 4. We dropped the term “all year through”.*

Line 24, “for each season”

*Done.*

Line 21 and 22, This sentence is confusing.

*We want to analyse variability on time scales ranging from interannual to decadal scales.*

Page 9622 Line 28, R of +/-0.15 is not much of correlations. Suggestion: “of only around”

*Now we provide averages for positive and negative values.*

Page 9624 Lines 1 and 2, “We tested two different methods for the analysis of linear trends: median of pair wise slopes regression (named “Theil–Sen slope estimator”, Theil, 1950) and linear regression.”

*Changed into: “Two methods to analyze linear trends are tested: the “median of pair wise slopes regression” method (named “Theil-Sen slope estimator”, Theil, 1950) and the linear regression method.”*

Line 11, remove “only” ? otherwise rewrite the sentence.

*“only” has been removed.*

Figure 2, What is "RMS" and "RS" ? The first sentence is hard to understand, particularly when "and the "observed" FTH." at the end is included.

*Figure 2 has been updated and “...and the observed FTH” has been removed.*

Figures 3-5, Suggestion: "RHFT (%)" as y-label. Change to "considered periods"(Fig.4).

*As mentioned earlier we will continue to speak of FTH.*

Figure 5. “Time series of regional RHFT averages”

*Change into “regional FTH averaged over...”.*

Figure 6. Increase the size of the figures and fonts. The name of the three y-labels used in Figures 6a - 6c are not coherent. Suggestion: Fig. 6a "Relative diff. (%)", Fig. 6b "NRMSD (%)", Fig. 6c "Absolute diff. & RMSD (%)" and then explain in figure caption what the labels stand for.

*We enhanced the font size of figures 6 and 8. We harmonised the labeling and consistently speak of bias/RMSD and relative bias/RMSD .*

Figure 8, “Relative standard deviation in FTH and FTHp10 for each season (top four panels and bottom four panels, respectively) of the period 1984–2009.”

*Changed into “Relative standard deviation in FTH for each season (top four panels) and in FTHp10 (bottom four panels) over the period 1984-2009.”*

Figure 9, “Difference in decadal averages of FTHp10 between the periods 1990–1999 and 2000–2009.”

*Changed into „Difference in decadal averages of FTHp10 between the period 1990-1999 and the period 2000-2009.“*