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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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1 General comments

This paper presents work performed on 6.3μm data from METEOSAT sensors MVIRI and SEVIRI. The work contributes to the GEWEX effort on establishing a homogeneous, qualitiy 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 unsuspicious, 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?

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?

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.

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.

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.

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.

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?

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?

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.

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

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

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 σ. To give a value of σ "at one sigma" sounds strange to me.

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.

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

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

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

P. 9619, l. 14: "main difference" of what?

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.

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.

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.

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”.

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

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

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

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!

4 Technical comments

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

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

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

P. 9607, l. 6: expand ARSA.

P. 9608, l. 4: explain FTHp10.

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.

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

P. 9614, l. 23: adapted appropriate.

P. 9615, l. 1: highlights.

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

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

P. 9626, l. 16: extent.

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

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

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.

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