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Interactive comment on "Intercalibration between HIRS/2 and HIRS/3 channel 12 based on physical considerations" *by* Klaus Gierens et al.

Anonymous Referee #1

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This paper addresses a concern about appropriateness of existing methods to construct a continuous time series from a water vapour channels of different HIRS instruments operated on different satellites. It attempts to validate a method previously published by two of the authors to inter-calibrate these data using a novel, independent approach. This is an important topic, and would be worthy of publication in AMT, some of the methodology needs substantial improvement, as described below.

Major issues:

1. Cloud - the entire analysis is based on clear sky conditions, which may be appropriate for UTH analysis. However, as many UTH analyses will rely on the HIRS data themselves to set a cloud-free flag, these will themselves be sensitive to the HIRS calibration. The method described in the manuscript could easily be extended to a much

more general application of all-sky radiances, by the addition of black cloud to layers in the radiosonde profiles layers close to saturation.

2. What about the HIRS instruments on other NOAA and Metop platforms? These each have subtly different SRFs. A simple analysis should be conducted to show whether the results for the HIRS on NOAA-14 and -15 can be generalised to all other HIRS/2 and HIRS/3 instruments, respectively, and by extension to HIRS/4.

3. Similarly, the SRF of several HIRS instruments has been suspected of changing during their lifetimes. It should be emphasised that this analysis represents a relatively short period in the lifetime of two HIRS instruments, and that their spectral and radiometric calibration was assumed to be constant over this period.

4. Uncertainties are generally neglected throughout the paper. While results are often expressed in terms of correlation coefficients, it would be more helpful to quantify the uncertainties - for example, with which HIRS/3 brightness temperatures can be predicted from HIRS/2 observations.

5. Two potentially serious deficiencies in the method related to the selection of profiles used for training and testing the regression should be addressed - see below.

Specific Points:

p.3.14: What range of angles were covered?

P.3.17: It is not generally accurate to say that "brightness temperatures from infrared sounders are only reported for cloud– free situations".

P.3.21: Why restrict the analysis to observations within 30° of nadir?

P.3.23: Are the set of profiles restricted to clear sky conditions? If so, how? Otherwise, the distribution could bias the regression results.

P3.30: It is not clear exactly which profiles were used for training and testing the regression. It is good practice to subset the dataset used for training and testing.

P.4.1: It would be worth pointing out that this corresponds approximately to the height of the tropical tropopause and that very little water vapour is expected above this level (or that it is poorly measured by radiosondes). However, it would be good to quantify its contribution to the Ch12 radiances.

P.5.15: This contradicts the values given in §5.1. Please reconcile! It would also be helpful to compare the peak of the weighting functions of Ch11 and Ch12 here - especially bearing in mind how these depend on the water vapour column amount.

P.5.28: The choice of using brightness temperatures instead of radiances should be justified briefly.

P.5.31: Is it possible that the Ch12-Ch11 difference contains additional information in off-nadir directions?

P.6.5: For completeness the covariance of the uncertainty of parameters should be included - to allow the evaluation of the uncertainty on the fitted brightness temperatures.

P.6.12: It is not sufficient to say "Obviously, the superposition methods works well for these data..." - quantitative statistical evidence is needed.

P.6.27: It would be helpful to colour cases with a significant surface contribution in Figure 7 to support this claim.

P.7.12: Why is this range of latitudes selected? (70°N is outside most definitions of "mid-latitude")

P.7.17: Introducing the symbols x and y does not aid the understanding here. Please change to follow the same convention used in the rest of the paper ($T_{12/14}$, etc).

P.7.20: Without uncertainties on the regression coefficients, their comparisons are meaningless.

P.7.29: "this problem" - which problem?

P.8.9: It is not at all obvious to me from Figure 9 what "the problems in the low tail of channel 12 brightness temperatures" are.

Fig.1: It would be helpful to include summary statistics (mean and SD) of each population.

Fig.1: It would be helpful if the upper and lower panels were presented on the same scale, or even as different colour symbols on the same plot.