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Atmospheric Measurement Techniques Discussions

Interactive comment on "Technical Note: On the intercalibration of HIRS channel 12 brightness temperatures following the transition from HIRS 2 to HIRS 3/4 for ice saturation studies" *by* Klaus Gierens and Kostas Eleftheratos

Anonymous Referee #3

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Major comments

This paper demonstrates faults with a regression-based approach for intercalibrating HIRS Ch. 12 between HIRS version 2 and HIRS version 3. While it is arguably questionable to intercalibrate the 6.5 micron and and 6.7 micron channels, which sample such different layers of the atmosphere, the authors are straightforward about this, and they are not the first to attempt to do so.

The authors are interested in near- and super-saturated relative humidity with respect to ice. This is at the tail end of the distribution of brightness temperatures intercalibrated

by Shi and Bates, so it is unsurprising that their intercalibration method underperforms in this case.

The authors go on to demonstrate the failures of a linear regression approach, and instead suggest a cumulative frequency distribution (CFD)-based approach. This approach seems to make the corrections they seek, while leaving the rest of the values more-or-less alone.

I have no problems with the CDF approach the authors chose. But I would like to see it compared to something that doesn't fail as terribly as the OLS regression line. Glancing at figure 2, it can be seen that the regression curve is flat, thanks to regression dilution. (See Pitkänen et al., 2016; doi:10.1002/2016GL070852). The elimination of super-saturation upon its application is a direct result. Rather than continuing with their critique of the linear regression method - with fails almost trivially - some other standard technique ought to have been applied. May I suggest calculating instead a bivariate regression (see York reference in in Pitkänen)? Practically, this can be done by choosing a line that goes through the center of mass of the scatter plot, with the same slope as the first eigenvector of the 2x2 covariance matrix of the 2xN time series of pairs. The first eigenvector points in the direction of maximum variance, thus minimizing the residuals perpendicular to the line, rather than in an arbitrarily chosen y-direction.

Also, I think the authors should be more clear how they choose pairs of data points. For example 2 HIRS/2 points "A" and "B", and 2 HIRS/3 points "1" and "2", if all close together, can produce 4 pairs for comparison: A-vs-1, A-vs-2, B-vs-1, and B-vs-2. Do the authors avoid this sort of inflation?

Several figures are plotted with very fine points which only appear at certain zooms in Adobe Acrobat; I suggest the authors use larger points that are semi-transparent, or use a heat map. Also, the rainbow color scheme chosen is neither perceptually uniform nor is it color blind safe.

The language and writing are understandable, but not publication-ready. The paper could use some revision for language.

Some minor comments follow:

p1 7, "We present that" should be "We show that" 21, "Ticklish" is informal

p2 1-3, "Relatively few papers..." Can you cite some? It looks like you do later; Gierens 2014, Lamquin et al., 2009, Dickson et al., 2010 5, "to study" should be "with which to study"

p3 27-28 In which section?

p4 29-30, Mean value is in parenthesis with the standard deviation, contrary to the use of parenthesis beforehand.

p7 26-27, "Although it looks like an indication of climate change there is none." This statement is sweeping. (Of course there is climate change all the time. That's just probably not what we're looking at.)

Figure 9, the scales are all the same, contrary to the caption

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