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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Radiometric consistency assessment of hyperspectral infrared sounders" *by* L. Wang et al.

**Anonymous Referee #1**

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## 1 General Comments

This manuscript describes the comparison of observations from 4 hyperspectral infrared sounders. Very important work for the satellite calibration community, and to support the generation of Fundamental Climate Data Records, not only from these instruments, but from other satellite instruments, which can be inter-calibrated against them.

However, there seem to be some shortcomings in the methodology adopted. In particular:



1. The analysis lacks a full uncertainty analysis of the comparison methods, and their results. This makes it difficult to judge their significance.
2. The methodology is not always well justified - for example the collocation criteria.
3. More care should be taken to minimise the potential for systematic biases in the instrument comparisons. For example, if the same time period is not used for each dataset, their double differences could alias components of the seasonal cycles in instrument calibration or scene radiances if their biases are radiance-dependent.
4. The authors should be more specific when reporting their results - for example, specifying exact dataset versions, date and time periods. They often use more expressions such as "less than 0.02K in all bands", rather than specifying the exact magnitude of the differences - e.g. "with an rms difference of 0.01K". It is, however, appreciated that the relative difference between the channels can be radiance-dependent, which means that such summary statistics as "CrIS is 0.06K warmer than IASI on average" could be misleading.
   The manuscript would also benefit from more discussion on the following questions:
5. Can you make recommendations to how to address the differences in the instruments' datasets for different applications?
6. Can we distinguish between spectral and radiometric differences?
7. Are differences simple functions of radiance in each spectral band?
8. Are the differences stable in time?
9. Is there any relative scan angle dependence between the instruments?

10. The analysis of CrIS' performance is limited to the normal mode. It is now routinely operated in full resolution mode. What is the expected impact of this change?

## 2 Specific Comments

### 2.1 Introduction

Line 101: Applying strict collocation criteria will certainly reduce the uncertainty on each collocation, but will not necessarily reduce the uncertainty on their ensemble mean. It should also be pointed out that it is important to ensure the full dynamic range of radiances are covered. Fig. 9 suggests this is OK - but nonetheless, it is worth highlighting in the text.

### 2.2 Methods

Line 209: The collocation criteria in Table 2 appear to be plucked from the ether. Their choice (and the associated trade offs) should be justified in the text.
Line 221: What is the impact of relaxing the time difference threshold to 15 minutes? For example, biases could be introduced if there is a systematic time difference between the observations. This could be quantified, considering typical rates of change of scene radiances.
Line 234: Using only uniform FOVs does not avoid these uncertainties - but it may reduce them. It could, however, be better to quantify these uncertainties (e.g. using a higher resolution collocated imager) and combine all FOVs in a statistically optimal way, in a way similar to the approach adopted within GSICS for the inter-calibration of geostationary imagers using collocations with hyperspectral sounders.



Line 237: Using the high resolution imager will only be appropriate for the channels in the IR window. Channels in strongly absorbing H2O/CO2 bands will not be sensitive to the same variability - especially when caused by low clouds or surface conditions. The authors should consider using high resolution imagery from different VIIRS channels for the same purpose.
Line 249: Applying a threshold to this ratio of radiances will reject proportionally more cold collocations. The authors should consider applying a constant threshold to the standard deviation instead to allow more collocations at the low radiance end of the scale.
Line 281: It would be helpful to also know the rms difference, as this can more easily be used in an uncertainty analysis than a limit such as "less than 0.02K", which requires an assumption of the statistical distribution of the differences.
Line 294: This sentence implies the calculations are actually done in radiances - at least for CrIS vs AIRS. Is this the case for all instrument pairs? If so, it should be made more clear earlier in the article, as other sections seem to imply the comparison is done in brightness temperature space.

### 2.3 Results and Discussion

Line 333: Again, please be more specific. Instead of "less than 0.2K", please specify their mean and/or rms difference.
Line 338: It should be pointed out that, given sufficient number of collocations, the radiometric noise on each sample should cancel out - and therefore not contribute to the mean difference.
Line 349: More care should be taken to minimise the potential for systematic biases in the analysis of these double differences. In particular, exactly the same time period should be for each dataset to ensure components of the results do not include aliased signals of seasonal cycles in instrument calibration or scene radiances if their biases are radiance-dependent.

Line 364: The fact there is no strong scene dependence in the difference could imply that their spectral calibration is consistent. However, the channels in the $H_2O$ and $CO_2$ bands appear to have statistically significant slopes (at k=3 level). This may indicate a spectral calibration issue - or may be due or deficient spectral matching.
Line 368: The authors could suggest how the CrIS-IASI radiometric consistency be analysed at low latitudes, without direct collocations.
Line 373: How are the 3 datasets combined together? A simple mean? Or weighted according to the number of collocations and their geographic distribution?
Line 380: The number of collocations included in the sample should be specified to justify the statistical significance of the results.
Line 393: How are these 25 spectral regions defined? Are they the same as used by Tobin?
Line 400: What is the rationale for choosing different spectral bands for analysis in Figures 14 and 15, compared to Figures 11 and 12? The authors should follow the same approach here - and fit a trendline to the differences, giving its slope and uncertainty.
Line 412: Logically the time series analysis should precede the other analyses, as it is only because the instruments' calibrations are relatively stable that the results can be combined in this way. More statistics of the time series of the instruments' differences should be included.
Line 414: Again, please be specific about the magnitude of the seasonal variations.

### 2.4 Conclusions

Line 433: It would be interesting to speculate on whether these differences could be related to difficulties in calibrating this band when CrIS is processed in “normal” spectral resolution mode.



### 2.5 Tables

Table 3
I found this type of table very useful, as it contains quantitative information and would also like to see counterpart tables for CrIS-IASI-A and -IASI-B.
The specific period of data analysed should also be mentioned.
This table seems to have too many decimal places. The STDEV suggest that two decimal places should be sufficient.
However, the number of samples should also be specified to allow the statistical significance of these values to be established.
It may be helpful to summarise the mean rms difference of all spectral ranges.

### 2.6 Figures

Figure 1 is hardly referred to in the discussion, yet it provides a useful overview of the spectral coverage of the sensors. It would be helpful to add the wavelength on a second x-axis for readers without extensive experience of spectroscopy.
Figure 7: Again, it is difficult to judge from this plot the typical magnitude of the errors introduced by the spectral conversion process, as the line density saturates. It would be helpful to specify the rms difference in each band.
Figure 8: It would be helpful to mention that the blue dots show the mean difference in each of the 25 spectral regions. And even more helpful if error bars were also plotted to show the uncertainties of these differences.
Figure 9 and 10: I would find it helpful to indicate the standard errors on the biases. By my calculations these would be  15mK (SD 0.5K, n 1000). But I'm sure you can do a better calculation. The scale on the middle panels could then be zoomed to show the significance of these differences.
Figures 11 and 12: Although the values of the slopes look very small, it still represents

-0.12K and -0.34K over the range shown for the window channel. For completeness, it would be good to include the fitted intercept (and its uncertainty).
Figure 15: The summary statistics should be shown, including rms difference, slope, intercept and their uncertainties.

## 3 Technical Corrections

### 3.1 Abstract

The authors should specify the period of data used in the comparison in the abstract.

### 3.2 Introduction

Line 86-87: This sentence is not sufficiently unambiguous. How are they averaged?
Line 66: I suggest removing "newly-launched", as that will soon be out-of-date.
Line 85: The text should be reworded to make it clear that there is not only one 300 km x 300 km area which contains all orbit crossing points.
Line 86-87: This sentence is not sufficiently unambiguous. How are they averaged?
Line 99: Typo: hypespectral

### 3.3 Instruments and Datasets

Lines 114-119: This paragraph repeats much of the material from the introduction and should be rationalised.
Line 123: It does not seem to be relevant to discuss this requirement here. It could be removed.
Lines 129-133: This is probably more detail than is necessary and could be reduced.



Line 140: Typo: "Fields of Regard" not "Fields of Regards".
Line 154: If only the apodized spectra are used in this study, then the spectral resolution before appodization is not relevant.
Line 157: This sentence implies there was a change in the CrIS hardware or control software to switch to FSR mode. If it was just a change in the processing software, it should be made clear here.

### 3.4 Methods

Line 181: The fact that the satellites are at different altitudes is not really relevant to this discussion.
Line 184: Figures 9 and 10 show the SNOs do cover a wide BT dynamic range. So it would be more accurate to say here that they cannot be compared over their full dynamic range.
Line 190: Similarly, the word "relatively" could be omitted here.
Line 208: It would be helpful to add "(not shown)" in the discussion of the CrIS-IASI swath overlaps.
Line 250: It should be noted that the use of brightness temperature difference amplifies the apparent differences for cold scenes.
Line 260: It should be noted that these plots nicely how CrIS' lower spectral resolution smooths the line features and limits the observed range of radiances.
Line 312: This sentence is not clear and should be re-written. Alo typo: "happed"

### 3.5 Results and Discussion

Line 334: I do not see the relevance of this statement. NWP centres typically include their own bias correction schemes.
Line 343: It should be made clear that this discussion on the large spectral resolution

in the SWIR band refers to CrIS.
Line 365: typo: “implies” = “applies”.
Line 407: The cause could be described simply as “differences on the instruments’ non-linearity corrections”.

### 3.6 References

Line 493: The article cited by Jouglet et at. does not appear in the proceedings of this conference.

### 3.7 Figures

Figure 2: Typos: ARIS=AIRS, 1 March = 1-3 March.
Figure 3 and 4: A color scale should be included, if only in the caption (e.g. blue=200K, red=300K).
Figures 6, 9 and 13: Spectral gaps should not be interpolated by continuous lines!
Figure 9 and 10: These figures appears to be fuzzy - both when the PDF is viewed online and printed.
Figures 11 and 12: Again, the y-scales could be zoomed on the top and bottom panels.
Figure 13: The y-scale on the lower panel could be zoomed.
Figure 15: The y-scale could be zoomed.

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