

Interactive comment on “Long-term stability of TES satellite radiance measurements” by T. C. Connor et al.

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The Connor et al. paper is thorough and should be highly useful to the TES user community. I recommend acceptance subject to the authors addressing the following general and specific points (which should be easy to do). The technical points/typos indicated at the end should also be addressed.

On behalf of the authors, I thank you very much for the helpful comments and thorough review. We appreciate your hard work and effort. Below, we have addressed each of your comments. In addition, we have modified the text of the paper to reflect your helpful insights, where needed.

General points:

C812

The main point is that the authors should provide details of the motivation for this work in the Introduction (e.g. before paragraph starting L. 20) and Conclusions. Why should one be interested in TES data? Who would use it?

The high spectral resolution of the instrument makes it very useful for identifying and quantifying trace atmospheric gases, among which are ozone, carbon monoxide, methane, and ammonia. The user community consists of researchers interested in global air quality and climate change. A sample of published retrieval results for ammonia may be found here: [Clarisse, L., M. W. Shephard, F. Dentener, D. Hurtmans, K. Cady[©], F. Karagulian, M. Van Damme, C. Clerbaux, and P.[©]. Coheur (2010), Satellite monitoring of ammonia: A case study of the San Joaquin Valley, *J. Geophys. Res.*, 115, D13302, doi:10.1029/2009JD013291.]

A number of statements need to be quantified, clarified. See the specific points below for details.

Specific points:

P. 1726

L. 2: Please quantify the agreement “with each other”.

The 1B2 and the 2B1 filters show differences with TES and either SHIS or AIRS that are x , while filter 2A1 is $x+\delta$ (Shephard et al., 2008). So, that is what is meant by “with each other.” This was a poor phrase to use as it is ambiguous. Thanks for drawing our attention to it, we will change the text to state this in a less confusing manner.

P. 1727

L. 12: Why will much less error be shown for the case mentioned?

There will be less error due to the fact that the windows are free of spectroscopic lines (at least strong lines). This means that the spectrum over these micro-windows will be essentially flat, thus the reference calculation and the OSS fit will be numerically the

C813

same.

P. 1729

L. 16: Do you refer to a micro-window here or to a window?

This is referring to the window over the specified interval of 900.48 to 901.28 cm^{-1} , which is a micro-window.

P. 1730

L. 1: To help the reader, identify the window regions.

We will make a reference to Table 1 at this place in the text, there the coverage of the windows in each band is listed.

End of Section 2: There is no reference to Fig. 3 in the text, and the next figure to be discussed is Fig. 4 (beginning of Section 3).

Figure 3 is an example of the raw residuals that were used in constructing the Histograms (Figure 4). Thank you for pointing this out, we will certainly make reference to it in the text.

P. 1731

L. 16: Why is the crucial value of the wind speed 6 ms^{-1} ?

Good question, this number comes out of the literature (Donlon et al., 2002). Turbulent mixing of the upper water column (down to about 1m) takes place at wind speeds greater than or equal to 6 m/s.

P. 1733

L. 2: What is expected?

The expected double difference (see page 1732 L. 15) is about -0.33 K

L. 9: Does the value of -0.16 come from a Figure?

C814

No, it is from the identical analysis shown in Figure 2, but for the night time cases, where Figure 2 is showing the day time cases. Good point, we will adjust the text to make it known.

P. 1734

L. 1: The lags mentioned indicate a semi-annual cycle – could you comment on this?

This is a great question. It is our assumption that it has to do with the seasonal variability in clouds, which was pointed out in: [Aumann, H. H., S. Broberg, D. Elliott, S. Gaiser, and D. Gregorich (2006), Three years of Atmospheric Infrared Sounder radiometric calibration validation using sea surface temperatures, *J. Geophys. Res.*, 111, D16S90, doi:10.1029/2005JD006822.]. Although there are tight constraints placed on which data we select, there is always the chance that there is some cloud in the scene. There are seasonal signals in the formation of clouds in the tropics and this is the most likely candidate for the observed pattern in the differences between measured and calculated.

L. 20 and Fig. 6: Could you explain the large negative spikes?

Again, clouds contaminating the scene are the most likely explanation.

P. 1735

L. 8: Why should the bias not have a deleterious effect?

The constant bias is simply removed (i.e. mean centered) and the trend analysis performed. The point here is that it has no impact on our ability to analyze for trend, so it will not have a deleterious effect on the analysis.

P. 1736

L. 15: What is n in Eq. (4)?

The variable n is the number of observations in the time series. Thanks for pointing

C815

this out. We will update the manuscript.

P. 1740

Table 1: Is it microwindows what is being shown? (Numbers are different from those mentioned in p. 1725.) If so, please indicate. Also, the frequency ranges for 1B2 and 2A1 are the same. Is this correct?

Yes, these are the statistics for the micro-windows. The numbers on p. 1725 indicate the spectral coverage of the individual TES filters (i.e. the total spectral coverage of the TES instrument).

Yes, the same micro-window was used for filters 1B2 and 2A1, as these filters have some overlap in their spectral coverage. This was a convenient choice as it will totally remove any spectral considerations when comparing radiances measured by the two distinct filters.

Technical points/typos:

P. 1724

L. 12: Replace “;” with “:”.

L. 13: I think it should be “ocean;”.

L. 22: “(L2),”.

P. 1728

L. 2: Introduce acronym for SST (even though you have done so for RTGSST). Make sure all acronyms are introduced (in abstract and main body of the text).

P. 1729

L. 27: I suggest “provide evidence that”.

P. 1730

C816

L. 15: Would “longer tails” be better?

P. 1731

L. 7: I suggest “-0.07 K, respectively, for TES minus AIRS differences” – am I correct in this assumption?

L. 26-27: I think you mean “the expected daytime TES measurement”.

P. 1732

L. 5: I think you mean “The expected nighttime TES measurement”.

L. 7, Eq. (2): Should this have -0.13 K as the last term?

L. 20: “Equator”.

L. 25-26: Do you mean the difference has the correct sign? I suggest “reduced” instead of “muted”.

P. 1733

L. 28: I suggest “plot demonstrates”.

P. 1734

L. 18: I suggest “Figure 7 shows”.

P. 1736

L. 4: “test chosen is the”.

L. 7: “normally distributed”.

L. 10: “data. The Mann-Kendall”.

P. 1737

L. 10: “and demonstrate”.

C817

L. 23: "period can be assumed".

Thanks very much for the typo corrections! I have included them in the revised document. Again, the thorough review is very much appreciated!

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