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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of Tropospheric Emission Spectrometer (TES) CO_2 for carbon cycle science" *by* S. S. Kulawik et al.

Anonymous Referee #2

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ACPD Review ===========

Characterization of Tropospheric Emission Spectrometer (TES) CO2 for carbon cycle science

S. S. Kulawik et. al.

This is a good paper that represents a very systematic approach to retrieving CO2 using hyperspectral IR radiances. Most of the comments address particular concerns that will not change any of the major conclusions of this paper.

1 Organization ~~~~~~~~~~~~~~~

I believe Section 4 should come before Section 3. This paper is about TES retrievals,

and the details of what was done and altitude sensitivity should come before discussions of validation. Perhaps an abbreviated discussion is warranted before Section 4 in order to quickly introduce CO2 variability and how that relates to their selection for the a priori covariance.

The OSSE study seems a bit out of place. The discussion is limited, but I wonder if it would be more appropriate as a separate paper.

2 Nomenclature ~~~~~~~~~~~~~~~

AIRS CO2 is used as a generic term, which is inappropriate. A number of investigators have used AIRS to study CO2. Most of those studies have used channels peaking in the upper troposphere, but AIRS has channels similar to TES (albeit with slightly reduced sensitivity) that have kernel functions more similar to those used in this study. I would expect future work with AIRS to utilize those channels. Thus, I believe the authors should use a term like AIRS upper-tropospheric CO2 to clearly establish that the existing AIRS CO2 studies they reference in some detail were limited to channels sensitive to the upper-troposphere.

For example, page 27405, lines 5 and 6 are incorrect. Houweling *used* channels that were only sensitive to the upper troposphere, but this sentence gives the reader the impression that AIRS only has channels with upper trop sensitivity. Yes, I see the comma in that sentence, but nevertheless I think it leaves an incorrect impression. Again, later, the reference to Chevallier and AIRS should be more precise in that again, Chevallier only used AIRS channels sensitive to the upper-troposphere. In addition, Page 27405, 2nd paragraph, should not lump all AIRS studies together, since at least one study used AIRS channels peaking in the mid-troposphere.

Maybe the author's should emphasize that their study represents a much more rigorous statistical analysis for channel selection and retrievals than previous studies, which lead to their use of mid-tropospheric channels (as well as their ability to handle clouds).

3 Clouds ~~~~~~~~~

CO2 variability is far less than other minor gases retrieved using the TES retrieval algorithm. I question whether the authors have really determined the effect of uncertain cloud contamination on such small signals. I looked at their previous papers on how clouds affect minor gas retrievals and could not find information that would really be applicable to such small signals. They do discuss the effects of clouds on total column ozone, but not on tropospheric ozone (a small signal). Could they quickly, and easily, add some information on how the retrieval correlations (with maybe Carbon Tracker) vary with observed cloud amount? I would just think this is a signficant issue relative to other interferring species.

4 Detailed Comments ~~~~~~~~~~~~~~~~~~~

Page 27411, line 8: minimize(s)

Page 27415, start of Section 4.6. First sentence not well written - at least a comma after CO2. Next sentence, changing the temperature constraint from 2 to 0.6K seems like a large change. Did the authors evaluate if their temperature retrievals degraded? If so, how can they then be sure their CO2 retrievals are correct?

Section 5.5 I am very glad to see the authors accept the need for a bias correction.

On page 27422 they state they only used surface and 10 km in situ measurements in the SH. There is one active GLOBALVIEW aircraft site in the SH the authors could use for validation. In addition, there are a number of NH GLOBALVIEW aircraft sites. This would strength the validation since their existing SH CO2 intercomparisons showed more scatter than the NH intercomparisons. I'm just not sure why they did not look at the relatively numerous GLOBALVIEW NH land sites?

In section 6.4 they note lower correlations for CO2 comparisons over land (SGP). They do question the issue of variable surface emissivity. I wonder if they are unable (at some level) to distinguish between clouds and surface emissivity. If so, this would lead

to relatively large areas since clouds will "absorb" at the CO2 channels, unlike surface emissivity.

My largest concern again with this paper is the lack of discussion on how the treatment of clouds could impact their CO2 retrievals. Table 2 indicates clouds are not a large problem but the context for this table is missing as far as I can tell. Were these DOF values derived for ocean only, or land with variable emissivity as well?

5 Figures ~~~~~~~~~~

I see no reason for Fig. 1. The differences between NH and SH CO2 time series are well known.

Figure 9: It is very hard to compare the various curves. I see no need to show the TES inital or the raw data. Then the graph y-axis can be narrowed to a small range making inter-comparisons much easier. Aslo, TES-swap is not defined. I'm not sure the legend is correct. w/obs = ML w. TES obs operator? This caption needs work or more discussion in the text.

Figure 10: As in Fig. 9, I would remove the raw data so that the top panel y-axis range can be narrowed. Otherwise, it is almost impossible to intercompare the curves.

Figure 11: Same concern as Figure 10, narrow y-axis scale of top panel.

Figure 13: Same concern as Figure 10.

Figure 15: I'm a little confused about the top panel. The small circles are the TES data, and the background is interpolated from that?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 27401, 2009.