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ACPD

9, C12044–C12051, 2010

Interactive Comment

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

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Below contains parts of the review from Reviewer 2. Where a response is needed, a response statement is given with a description of the action taken. Thanks to this reviewer for their comments and suggestions.

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.

Response: Section 4 and 3 were swapped as suggested. The a priori covariance discussion references the new section 4.

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.

Response: We feel that this study is an important characterization of the usefulness of TES data, particularly the effects of averaging. More detailed modeling studies using TES data are being worked on for publication.

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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.

Response: The Howeling description was changed to not reference AIRS, since the upper troposphere sensitivity is the key issue: Houweling et al. (2004) showed that using upper tropospheric CO2 observations would lead to significant reductions of CO2 source and sink errors as compared to the in situ observation network.

We also changed the wording to emphasize the method used: Chevallier et al. (2005, 2009) found, however, that their inversions using AIRS radiances are sensitive to latitude-dependent biases larger than about 0.3 ppm and that while their radiance assimilation improved surface flux uncertainties, it did not perform as well as the flask network.

We did not intend to lump the AIRS studies together. We have updated this text to emphasize the wide range of approaches used to: AIRS was launched on the EOS-Aqua platform in 2002 and different techniques have been used to retrieve atmospheric CO2 abundances from AIRS radiances. These include using different spectral regions, different optimization methodologies, co-retrieving different species and direct assimilation of radiances. The different retrieval approaches have resulted in AIRS retrievals with estimated peak sensitivity ranging between 200 and 600 hPa (e.g., Chahine et al., 2005, 2008; Crevoisier et al., 2003; Engelen et al., 2004; Strow et al., 2008; Maddy et al., 2008). The AIRS data shown in this paper are taken from the AIRS standard product (Chahine et al., 2005, 2008).

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Maybe the authors 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).

Response: We believe that some previous studies have surveyed available CO2 information. We have presented the reasons for using the channels that were selected.

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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

ACPD

9, C12044–C12051, 2010

Interactive Comment

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.

Response: This is an excellent point. Our retrievals are for cloud OD < 0.5 and for these conditions, we have calculated the error propagation for clouds into CO_2 and done checks for degradation or correlations of CO_2 error with cloud properties. We added some text to describe the checks that have been done in section 5.3. In adding this additional text we realized that we had omitted that this cloud OD restriction was used, primarily because of sensitivity issues. This text was added in section 5.1 "Because of the sensitivity decrease with higher latitudes, only 40S to 45N is used from TES data. [...] For similar reasons, only targets with cloud optical depth (OD) less than 0.5 are considered to have good quality."

The new paragraph added to section 5.3: With the cloud optical depth (OD) less than 0.5, the predicted errors from clouds propagated to CO_2 are on the order of 1 ppm. We would expect this to impart a random error with some targets retrieving clouds that are too optically dense, and other targets retrieving clouds that are too optically thin. The systematic impact of cloud errors was studied by comparing TES versus Mauna Loa data for different retrieved cloud top pressures and cloud optical depths. The targets were divided into two groups, those with OD < 0.1 (162 targets per monthly average) and those with OD between 0.1 - 0.5 (101 targets per monthly average). The predicted DOF is 0.85 versus 0.67 for these targets and the RMS errors versus Mauna Loa data with the TES observation operator are 1.3 and 1.5 ppm, respectively. Although OD < 0.1 performs better, averaging all cases results in a lower RMS of 1.1 ppm due to averaging more targets. The two cloud bins show a relative bias of 0.6 ppm (higher cloud OD results in higher CO_2). Partitioning cases with clouds (OD >

ACPD
9, C12044–C12051, 2010

0.1) into those with cloud top pressures greater than and less than 500 hPa, produced 53 and 54 targets per bin, respectively. The RMS errors for these cases, based on comparison to Mauna Loa data with the TES observation operator, were 1.9 and 1.7 ppm, respectively, with a relative bias of 1.9 ppm (higher altitude clouds result in higher CO_2 retrieved). These values indicate that cloud optical depth artifacts are likely not a concern, but that cloud pressure artifacts may be impacting CO_2 retrievals and should be further studied. By itself this result should not be used to screen TES data based on cloud pressure, since cloud properties are related to convective processes and vertical transport, which can result in differences in atmospheric composition.

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Page 27411, line 8: minimize(s) Response: Corrected.

Page 27415, start of Section 4.6. First sentence not well written - at least a comma after CO_2.

Response: Reworded to: In light of the impact of CO_2 of errors in temperature and water, the retrieval strategy selected jointly retrieves atmospheric temperature, surface temperature, emissivity (over land), water vapor, and CO_2.

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 CO_2 retrievals are correct?

Response: The wording was updated to The temperature constraint was also tightened from $\sim$2K to $\sim$0.6K variability to allow more degrees of freedom for CO_2 and which matches TES-sonde temperature biases of $\sim$0.5K (Herman et al., 2008).

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

ACPD
9, C12044–C12051, 2010

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.

Response: The other aircraft land data over the US is either north of 40N or borders the ocean. Both of these will cut out a significant number of comparisons. However, we have added comparisons with other aircraft sites to gain understanding of how much CO2 should vary over distances that TES is averaging over. We feel that the TES/SGP comparisons indicate issues with the TES retrievals over land and have strengthened this language. The text now reads: Since SGP is at one location with a few measurements per month and TES is averaging over most of the United States, it is not surprising that there are some differences between these measurements, however the differences are larger than expected. Comparisons of aircraft data from Sinton, Texas (at 4.5 km) and Beaver Crossing, Nebraska (at 5 km) to the SGP aircraft data above 2 km show differences of 1-2 ppm which indicate that TES land retrievals of CO2 need improvement; one possible explanation that is now being investigated is interference from emissivity due to a prominant silicate feature in the right laser band.

Response (cont): The high TES values in the 2nd half of the year are present for 2005 and 2006 but not 2007 or 2008. The Rarotonga data is very spotty for the end of 2005 and 2006. However, the pattern of data at Rarotonga with the mid-troposphere higher than the surface values is important and is now mentioned in the text: To understand the differences between TES and validation data we also looked at data from the Rarotonga aircraft site in the southern hemisphere at 21S, 160W. Although missing data for much of 2005-2006, this aircraft data does show 1-2 ppm higher values at 4.5 km data versus the 0.5 km data indicating the southern Pacific is not as vertically homogenous

ACPD
9, C12044–C12051, 2010

as the northern Pacific.

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?

Response: As discussed above we have added additional studies of CO2 biases and variabilities with respect to cloud properties which do indicate improvements could be made. The table 2 title was updated to indicate the context: Degrees of freedom for CO2 for one ocean target in different scenarios

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

Response: Figure 1 was removed.

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.

Response: We took out TES-swap from this figure which makes the figure a lot less busy. We also tightened up the upper axis. However, we feel that showing the initial guess/prior puts these results in context and feel that the comparison between "MaunaLoa w/obs" and "TES" with the 360 ppm a priori can be easily seen in the updated figure. The caption and legend were also updated.

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.

ACPD

9, C12044–C12051, 2010

Interactive Comment

Response: These figures were re-done with a smaller maximum value of 390-395 ppm.

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?

Response: Yes, this is correct. The caption was updated to be more explicit on this point.

Thank you to reviewer 2 for the helpful suggestions.

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

ACPD

9, C12044–C12051, 2010

Interactive Comment