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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CH_4 and CO distributions over tropical fires as observed by the Aura TES satellite instrument and modeled by GEOS-Chem" *by* J. Worden et al.

Anonymous Referee #2

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This paper addresses the comparison, on a global scale, of CH4 and CO retrievals from TES (version 5) with simulations from the GEOS-Chem model for one month (October 2006), and the study of the correlation between CH4 and CO for fire situations, mostly over Indonesia.

It seems to me that the title of the paper is misleading since one would expect that most of the results would address the question of CH4 vs. CO emissions over the tropical region using both observations from TES and simulations from GEOS-Chem. However, about half of the paper deals with the comparison between TES and GEOS-Chem CH4, on a global scale, giving quite reduced a part on CH4 and CO for fire situations.

Moreover, the authors mostly have a look at peat fires over Indonesia while fires over Africa (that represent the majority of fire emissions in the tropical region) are not even really addressed (extremely short Section 5.3). I would therefore suggest the authors to change their title to highlight that their paper addresses the global comparison of CH4 and CO retrieved from TES and simulated by GEOS-Chem and the study of their correlation for specific tropical fires.

I agree with reviewer 1 that this paper seems to be the very first step before a more detailed study. If so, it should be clearly stated by the authors. But overall, the actual study on the correlations seems very preliminary to me and does need to be quite strengthened before being suitable for publication. In particular, the study of the correlation during a month not affected by fires, or during other months of the fire season should be performed to confirm the results (see also comment on Section5). Sections 2 and 3 deal with the presentation of TES CH4 retrievals and with the comparison to GEOS-Chem simulations. I agree with reviewer 1 that these sections should be longer. I find them very hard to follow for non-specialists (at least define what DOFS is, how are computed the various errors, etc). The definition of 'surface-to-tropopause CH4' should also be stated quite explicitly. What is the justification for averaging the mixing ratios and not computing a kind of 'tropospheric column'?

The consequences of using truncated averaging kernels need to be better explained. First, a new validation of TES CH4 (version 5 with truncated kernels) with HIPPO measurements should be made to obtain the bias corresponding to the retrievals actually used here. Since the authors are mostly interested in the tropical region, they should also use measurements from CARIBIC flights: a dozen of them, measuring both CO and CH4 (see for instance Schuck et al. ACP 2009 and AMT 2010) are located over Africa. Then, the role played by the stratosphere is quite vague to me and I agree with reviewer 1 that it should be analyzed in more details. In particular, one sub-section should be devoted to this, while the results are currently distributed in several subsections.

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Something that is generally missing in the paper is the study of uncertainties. What are the typical uncertainties associated to TES retrievals for both CO and CH4? How do they vary spatially and temporally, and with the thermal contrast (difference between surface and upper-air temperatures)? Thermal contrast has been shown to play a key role in both the uncertainties and DOFs that characterize the retrievals, and fire situations are particularly concerned. Also, how does it affect the averaging of TES retrievals in a GEOS-Chem grid box (for instance, are all the averaged TES retrievals in one box sensitive to the same part of the atmosphere?).

Concerning Fig. 4, while the authors mostly study the bias, there is no discussion of the very large spread of the retrievals which does seem to vary with latitude as opposed to the bias. What is due to natural variation of methane and what is due to the instrument noise and retrieval uncertainty? Moreover, does is vary regionally (especially over South America, Africa and Indonesia, which are the 3 regions of interest of section 5)? Section 5 finally deals with the study of the correlation between CH4 and CO for air parcels affected by fires. In particular, some simulations where CH4 from fires has been turned off are used to confirm that the slope obtained in the full comparison is characteristic of fire plumes. Here, I am wondering why the authors did not use simulations where only CH4 and CO from fires are active: this would give the value expected for fire situations, to which the retrieved slope could be compared. It would seem a more direct way than turning-off CH4 and finding a reduced slope.

Finally, the authors state that these results 'provide confidence in the total methane emissions from Indonesia'. I find this comment overly optimistic since it is based on one single month. Moreover, is this confidence at the level required to actually improve our understanding and estimates of the emissions themselves?

Specific comments

p 26210, l. 12-14: the maps in Fig. 1 should be described to explain the general features of CO. Otherwise, it could be removed.

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p 26211, l. 3: are TES estimates insensitive to any kind of aerosols? In the thermal infrared, it is true that fine particles like smoke don't have a strong impact on the observations. However, dust aerosols have been shown to strongly impact the observations and are even retrieved by observations from AIRS and IASI instruments.

p 26212, l. 5-9: '. . . vertical distribution of the sensitivities. . .': is it true whatever the thermal contrast is?

p 26213, l. 13-14: the last sentence should be given earlier (Fig. 2) since it explains what TES retrievals actually are.

p 26215, l. 22: could the authors give the averaged values of noise and temperature errors?

p 26215, l. 23-26: why is the sensitivity different between CO and CH4? It should at least be illustrated.

p 26216, l. 18-19: could the authors give some details on how errors in the stratosphere and the tropopause height can affect the comparisons?

p 26216, l. 24 and 27: is it 50 or 60° S ?

p 26217, l. 6-15: here the authors discuss the influence of stratosphere on CH4 TES retrievals. This paragraph should be aggregated with previous sections (4.1) where it is also discussed. Are CO retrievals also affected?

p 26217, l. 20: could the authors explain how the averaging is performed?

p 26217, l. 25: could the authors give some information on the spatial resolution of TES retrievals and the co-location criteria (averaged time/space distance between TES and model)?

p 26218, l. 22-25: the authors now consider only some part of the retrievals. How does it affect the previous comparisons? I strongly suggest them to homogenize the retrievals used in the various sections.

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p 26219, l. 27: '. . . are not just from peat fires': what are the other sources?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26207, 2012.

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