Atmos. Chem. Phys. Discuss., 12, C9665–C9668, 2012
www.atmos-chem-phys-discuss.net/12/C9665/2012/

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 #1

Received and published: 21 November 2012

Summary: This paper presents TES nadir measurements of CH4 and CO with a new version of the retrieval algorithm, V005, for October 2006, a period when there was an El Niño and serious peat fires in the area of Indonesia. The absolute values and ratio of CH4 and CO measurements are compared with GEOS-Chem in order to assess if TES is able to measure the signature of methane fire emissions. The results suggest that for sufficiently strong fires that TES can measure the signal of fire sources of methane as measured against CO.

General comments: It seems to me that the underlying goal of this TES study is to provide sufficiently accurate methane measurements that they can be used for

source emission estimation/inversion and this paper is in the vein of a prelude to that. They show that TES can measure the signal of sufficiently large forest fire sources of methane emission.

One of the aspects that is not altogether clear to me is the uncertainly and the role played by the stratospheric methane mixing ratio in terms of the retrieval and it would be useful to have a slightly longer and perhaps clearer discussion of it. For example, it is interesting that the Kernal presented by Wecht et al (2012), Figure 1, shows that the stratospheric sensitivity of V004 is much less than that for V005 (certainly above 100 mb) (Also in Fig 2 of this paper). But apparently V004 has a larger bias than for V005, certainly as measured against HIPPO measurements. But for analysis of TES to compare with GEOS-Chem a truncated kernel was applied and this seems somewhat unsatisfactory. Is it not possibly to use GEOS-Chem stratospheric methane profiles or some other model or instrument etc. For example in Figure 3 the apriori profile seems to fall off with height very rapidly above the tropopause (but we aren't shown below 1.7 ppmv of methane). Also there is a large distortion of the methane profile after the truncated profile is applied.

Another interesting aspect I felt uncomfortable was the issue of aerosols over the Indonesian fires. The authors suggest that TIR instruments are generally insensitive to aerosols (Verma et al, 2009) and not optically thick in the TIR. However, I am under the impression that during the Indonesian fire that there was dense smoke so probably visible OD $\sim$ 3-5 which might translate to $\sim$ 0.1 to 0.3 at $\sim$ 10 μm. Would not scattering with such an AOD not compromise the methane accuracy which is critical for source emission inversion purposes? The team is trying to get the absolute biases and accuracy down to the few percent level and even lower which is required for inversion: would not an assessment, or at least comment be useful? It fell that in the overall trajectory of this work it would be useful to have the effects of aerosols quantified.

It would have been interesting to examine the methane/CO ratio for the Indonesian far outside of the region assessed with a view to assessing how long the fire signature is

retained and can be measured downstream from the fire.

In general, I consider that this paper is, for the most part, clearly presented. As noted above I do have a few reservations about how the stratosphere is treated and consider that the paper would be more useful if these queries were dealt with in a clearer manner.

Particulars, typos etc

P26208 L9: I would use the word “compare’ in lieu of “evaluate”. I don’t think that a model that is not driven by extensive meteorological and chemical data assimilation should be used to evaluate measurements – perhaps present sanity checks. (And even with DA in the troposphere there are issues with the PBL and convection of species).

P26208 L18 spelling “slope” instead of slop.

P26209 L14. Niggling, but I would suggest that for the Shindell et al ref an “e.g. Shindell et al” be used since this was not the first paper to discuss the idea of attacking methane and BC emissions as ones with a rapid response time, eg. Hansen et al.

P26210 L15. “Plumes . . ..WERE (was) also observed. . ...”

P26210 L26 “methane FROM the Aura TES satellite” – word left out

P26211 L2 “TES estimates ARE mostly insensitive to . . .” word omitted.

P26213 L5 “Worden et al SHOW (no s)” plural.

P26214 L4 OH density should be 10.8x105 molec cm-3 (105 left out)

P26216 L20 what is the rationale for doing a vertical mixing ratio average as opposed to a density average?

P26216 L26-27. Awkward wording. Suggest “becomes larger at latitudes south of 60°S.

P26218 L7. Awkward wording. “Unlike (with) the methane estimates, the (this) bisas in

CO . . .”

P26220 L13 Award phrase. . .suggest “This correlation due to ..”

Sub-heading Indonesia fire plumes, South American fire plumes etc. I think that using the word plume is misleading in the sub-headings. In doesn’t appear in the title and there is no associated plume associated with Indonesian, South American or Southern African fires. I would consider a plume as a feature well extended from the source as opposed to hovering over the source (see above)

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