Interactive comment on "Regional-scale correlation between CO_2 fire emissions, burned areas, and mid-tropospheric CO2 **diurnal variations over southern Africa" by A. Chédin et al.**

Anonymous Referee #2

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The authors appreciate the helpful comments from the reviewer for improving the manuscript. Following comments by referee #1, the structure of the paper has been substantially revised. Note also the slight change in the title of the paper, the word "daily" being replaced by "diurnal".

This paper compares old observations from TOVS (the so-called DTE) to two fire data sets (GFEDV2 and L3JRC). Furthermore, the paper shows simulations that are described elsewhere (Rio et al.). The main problem I have with the paper is that it does not bring a lot of new insights. For instance, the good correlation with GFED2 was presented earlier.

Answer 1

The former Chedin et al. studies refered to continental scale DTE. The present paper focuses on southern Africa at a much finer regional scale. This is clarified in the abstract and in the introduction. This study contains new information to explain the DTE signal by linking quantitatively fire emissions with mid-tropospheric CO2 diurnal variations using 3-D simulations by the LMDz General Circulation Model, in which a pyro-thermal plume model was incorporated. These simulations confirm that a large fraction of fire products are directly injected in the mid-troposphere, well above the boundary layer. This is still a much debated question and these results bring new insights.

A second worrying point is that there remains a strong belief in the DTE product, despite the fact that the model can only reproduce its occurrence of Africa to some extend. The authors even claim that the DTE signal can be used to extend existing fire products in the past. This claim is not substantiated in the paper.

Answer 2

This application to Africa has been chosen for reasons explained in section 2.1, the essential point being that it is probably the continent for which fire activity has been studied in depth and for long periods. The "*claim that the DTE signal can be used to extend existing fire products in the past"* has been moderated (see, in particular, the abstract and the conclusion); a new research is planned to be launched soon to make this statement more quantitative.

The results are presented in a slightly biased way that seems to hide the difference and to highlight the correspondences. For instance, the histograms in figures 9 and 10 use different x-axis. The modeled DTE is presented in classes, while the observed DTE is presented in ppm.

Answer 3

This figure (now fig. 5) has been revised in consequence.

Observed DTE thus peaks at 2 − 4 ppm, while modeled DTE, after some recalculation, seems to maximize at only 0.2 − 0.4. The claim of a monthly mean modelled DTE of 1 ppm does not follow from this figure. On page 18636, line 13, even a DTE of 2 ppm is quoted!

Answer4

Figure 9 (now fig. 5) has been revised. First, over sea data included by error in the statistics have been removed. Second, as for the observed DTE (or GFED, or L3JRC statistics) a significance minimum value has been introduced. Now, modelled DTE less than 0.08 ppm (absolute value) are removed.
On figure 10 (now fig. 6), the observed peaks are for class 0-1 ppm (15S-25S) and for class 2-3 ppm (0S-15S). "*The claim of a monthly mean modelled DTE of 1 ppm"* applies to daily results over the fire region, as said in section 2.3.2 (part of the former section 4).

Another problematic part of the paper is found on page 18631-2, where the seasonal cycles are discussed. According to the authors these cycles start "too early" or "too rapidly" and shows an "early bias". The most plausible conclusion is thought in "limits of the burned area detection methods, ..., during the early season". This is again quite a large claim that is not substantiated and comes from a prior assumption that the patterns should be the same. I miss a critical evaluation of the DTE product. For instance, the venting of the biomass burning emissions to the upper free troposphere might be different in the early fire season due to different atmospheric stratification.

Answer 5

Seasonal cycles start earlier and faster in DTE than in burned area observations for 2 regions out of the 10 studied. The venting of the biomass burning emissions is one assumption among others. To explain the earlier phase of DTE, we need a source of atmospheric compound having a diurnal cycle in coherence with the satellite passes, and capable of inducing a signal for the infrared channels used in the retrieval. On the other hand, early season fires may not be detected properly in burned area retrieval algorithms. This hypothesis is supported by the study of Swap et al. 2003 (…"These results indicate that the fire season peaks earlier than presumed (…) and contradicts the assumptions of several other investigators"…) analysing the results of the SAFARI experiment. We obviously cannot conclude in presence of limited information to falsify either burned areas or DTE seasonal phase in regions 6, 7. We have modified the conclusion, from :
"This earlier DTE bias is not yet elucidated although a tentative explanation has been proposed based on the limits of the burned area detection methods for detecting small burnt scars during the early season."
to:
"This earlier DTE bias is not yet elucidated although a tentative explanation has been proposed based on the SAFARI 2000 experiment results presented by Swap et al. (2003) showing that fire scar estimates reveal contradictory information on the timing of the peak and extent of the biomass-burning season; this could possibly be due to the limits of the burned area detection methods for detecting small burnt scars during the early season."

Effects of aerosols on the DTE product are mentioned but quite easily put aside. Only high altitude aerosols (above 4 km) could contribute to the enhancement of the DTE signal by 1 ppm according to the authors. This is larger than the modeled DTE! And I do not see why only the CO2 would be transported to the free troposphere and not the aerosols of biomass burning.

Answer 6

Nothing is said in the paper contradicting the Reviewer's comment. In some extreme cases, the DTE signal can be enhanced by some 1 ppm due to smoke aerosols, as shown by Chedin et al, 2005. In the conclusion, a sentence has been added: "Also, one must keep in mind that, for particularly intense fires, high altitude smoke aerosols can contribute to the enhancement of the DTE by about 1 ppm."

In conclusion, I find the paper not very strong in showing the added value of the DTE product. After careful reading I am left with the feeling that we do not understand the DTE observations in a qualitative way. The simulations may look qualitative similar, but the effects are much smaller, even when the diurnal variations of convection and emissions are maximized in the afternoon (Gaussian time profile centred around 15:45 LST, with a width of 1 hour only). The conclusions formulated by the authors is far more positive. The authors should at least notice more clearly that there is still a large discrepancy between simulations and DTE observations. This includes slimming down their quantitative faith in the DTE product.

Answer 7

We respectfully disagree with the reviewer. The model simulations confirm for the first time that pyro-convection can induce a significant CO2 diurnal excess in the mid-troposphere. Such a link between CO2 emissions and DTE was not established quantitatively before. On the other hand, we agree with the reviewer that more work remains to be done for using DTE as a direct local proxy of fire emissions : the difference between simulations and observations has been outlined in the conclusion. One sentence added in section 2.3.2.: "However, the observed DTE signal remains larger on average than the model simulations.", and in the conclusion: "Although the simulations show that pyro-convection can transport CO2 from fires in the mid-troposphere to produce a daily variation in CO2, the observed DTE signal remains larger on average than the model simulations."

Minor issues

DTE values smaller than 0.3 ppm are discarded. Why is this? (page 18629)

Answer 8

Statistical analysis of the DTE signal was presented by Chedin et al., 2008 and its results are recalled section 2.2. We have added "(see details in this reference)" after "…mean accuracy of 0.4-0.5 ppm". DTE absolute values less than (at least) 0.3 cannot be considered as reliable as well as too small values of the other fire products (see section 3).

There is a funny way of dealing with the different time periods of the compared products. Whenever it fits there is no problem. When the correspondence is poor, the reason is the different time period.

Answer 9

We actually try to verify an assumption and, when it fits, there is indeed no problem. When it does not fit, we try to propose an explanation or we recognize that the difference is not fully elucidated yet (see section 3.2.3 and the conclusion). We have however modified one sentence end of section 3.2.1.

I think that figure 5 is unnecessary.

Answer 10

Figure 8 (former fig. 5) addresses an important aspect of fire emission inter annual variability with ENSO. By chance, the two time periods analysed show similarities in the occurrence of El Nino and La Nina events which directly impact precipitations and, by consequence, vegetation and fire occurrence and intensity. We have added, section 3.2.2 after "…La Nina episode (1988 and 1999)." : "These pairs also show large similarities (not shown) in the precipitation patterns for the two-year periods preceding and including the peak fire month used to estimate

precipitation levels during the period when herbaceous fuels typically accumulate (van der Werf et al., 2008; precipitations fields from Mitchell et al., 2005, http://www.cru.uea.ac.uk/cru/data/hrg.htm). However, exceptions are clearly seen for regions 8 and 10 and could explain why DTE and GFED disagree. Note also that regions 8 and 9 are regions where precipitation has greatest interannual variability in southern Africa, and thus vegetation also has greatest variability (Weber et al., 2008)."
We would prefer keeping Figure 5.

Caption figure 8: "as seen by a satellite". I would change in: "as would be seen by a satellite" to avoid confusion.
Answer 11
Done

Figure 9: Change to 'real' DTE values on the x-axis.
Answer 12
Done

Figure 2: Does this include the rejected values < 0.3 ppm?
Answer 13
No, as said in the text. See also Answer 8.

The final conclusion that the DTE observations can be very useful to reconstruct fire emission patterns should be substantiated by actually present such a reconstruction.
Answer 14
This statement has been moderated.