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

Anonymous Referee #1

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The authors appreciate the helpful comments from the reviewer for improving the manuscript. Note also the slight change in the title of the paper, the word "daily" being replaced by "diurnal".

GENERAL COMMENTS

The paper presents some interesting results but a few issues need to be addressed before it is publishable:
Firstly, what is the novelty of this research? page 4 lines 20-25 the authors state here that the relationship between DTE and fire emissions has already been demonstrated by Randerson et al 2006 and Chedin 2005, 2008. How then does this paper add any more information?

Answer G1:
The paper by Randerson et al. 2006 refers to the GFEDv2 fire emissions database based upon MODIS burned area land observations and the CASA biosphere model. It has nothing to see with the DTE approach which results from atmospheric observations.
The former Chedin et al. studies 2005, 2008 studies refer to the continental scale. The present paper focuses on southern Africa at a much finer regional scale. This is clarified in the abstract and at the end of the introduction.
The present paper contains new information to explain the DTE atmospheric CO2 signal resulting from 3-D simulations with the LMDz General Circulation Model, in which a pyro-thermal plume model was incorporated. The simulations confirm that a large fraction of fire products, here CO2, are directly injected in the mid-troposphere, well above the boundary layer.

To me the most interesting result is that the DTE shows more burning early in the season than any of the active fire or burned area products, and that this might actually be true (that the fire products would miss the early burns). - but you need to be able to explain why this is only apparent in some regions.

Answer G2:
The main scope of the paper is to study the correlation observed between biomass fire emissions and DTE and to propose an interpretation of these correlations based on numerical simulations. We do observe very good spatial correlations between DTE and fire emissions on an annual basis for all the regions studied. There are some phase differences on a seasonal basis for 2-3 regions out of 10 in southern Africa. We propose a tentative explanation based on the paper by Swap et al. (2003) and state that these differences "are not yet fully elucidated" (end of section 3 and conclusion). Unfortunately, we do not see how to go much further given uncertainties in both DTE and burned areas, and the different years compared for these fire-related observations… See also Answer 10.

Secondly the structure is hard to follow. The authors have a methods section (section 2) but then continue straight into reporting results without a clear results section. Similarly, at the end of the paper in the modelling section a whole lot more methods are reported. This needs to be clarified before the paper will be readable.

Answer G3
The structure of the paper has been substantially reorganised and is summarized in the last paragraph of the introduction. There are now two main sections instead of three (besides introduction and conclusion). Section 2 focuses on the description and the physical interpretation of the DTE signal using a modelling framework, while section 3 shows the quantitative correlation between the DTE signal and CO_2 emissions from biomass burning.

Finally, the manuscript needs to be carefully checked for correct English grammar. For example, Page 12 line 1: "The DTE observation periods is 10 years apart with GFEDv2" should read (I think): "The DTE observation period is 10 years earlier than GFEDv2"

Answer G4

We did our best to improve the grammar…"apart" : done

SPECIFIC COMMENTS

The abstract should be re-written to make the research sound more interesting. Start with "The usefulness of using the difference in morning and evening CO2 in the mid-troposphere as an index of fire emissions is investigated for southern Africa. This "Daily Tropospheric Excess" is compared with other datasets that have been used to estimate burned area and fire emissions (L3JRC and GFEDv2) and shows similar seasonal and annual patterns....

Answer 1

The abstract has been revised.

page 4 lines 20-25. You state here that the relationship between DTE and fire emissions has already been demonstrated by Randerson et al 2006 and Chedin 2005, 2008. How then does this paper add any more information?

Answer 2

There is nothing regarding the DTE in Randerson et al., 2006.

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

page 5 line 5 people usually burn more in the afternoon also.... see Gareth Robert's recent paper in biogeosciences.

Answer 3

We have added this interesting reference to that of Giglio et al., 2007.

methodology: be more clear about which analyses you do... this will make the results easier to follow. I would suggest a new section titled: "Analyses" containing a tabulated list of the various things investigated (eg 1: we compared DTE values with other fire products to test how good a proxy it is of fire activity. 2: we tested whether annual DTE values could pick up changes in fire emissions related to inter-annual variability in climate and fire. 3: we investigated how well DTE measurements reflect seasonal patterns of fire in southern Africa. 4. We developed a 3-D model to test whether the hypothesized mechanism of atmospheric transport can be reproduced in a general circulation model.

Answer 4

See answer G3.

It does not appear that you did any statistical tests to see whether these patterns were significant. If you did, then you should also detail them in the analysis section.

Answer 5

Statistical analysis of the DTE signal from Chedin et al., 2008 are given in section 2.2. We have added "(see details in this reference)" after "…mean accuracy of 0.4-0.5 ppm". Such a mean accuracy ensures significance to the patterns presented.

The authors mention in page 12 line 8 that the burned area data used to compare with the DTE are themselves prone to error (in fact, accuracy assessment show that the best of them (the modis burned area product, which the authors did NOT use) only identified 75 % of the fires. Can they think of any other, less error-prone, way of testing the usefulness of the DTE?

Answer 6

Unfortunately no. Regarding the Modis product, we agree with the Reviewer's comment. At the time of these comparisons, only one year of the MODIS burned area product was available (July 2001 to June 2002; see Roy et al., RSE, 2008).

page 12 lines 15-20: I dont think many fire ecologists or managers would dispute that most of the burning in Southern Africa occurs in June and July.

Answer 7

We agree with the Reviewer. However, we observe that L3JRC peaks in August and GFEDv2 peaks in September. We had to discuss this difference and used the paper by Swap et al., 2003 on Safari 2000 supporting the reviewer's remark.

Section 4.1: This explanation should be in the methods surely ??. Also, it does not provide all the information required. Where do you get your fire data from? Are these active fire data? In which case, perhaps it is justifiable. If the fires are also modelled then it is highly suspect to make them all the same size

Answer 8

The structure of the paper has been revised accordingly and the former section 4 no longer exists. The new structure is summarized at the end of the Introduction (see also answer G3).

The data, as said in the text, are from Liousse et al. (2009) and were derived from observed burned areas (ATSR and SPOT).

The goal of the modelling study is to show that an average fire size (here, $2km^2$, see section 2.3.1) can produce a non-zero DTE signal. Moreover, at the coarse spatial resolution of the transport model, variability in the size of each fire cannot be prescribed easily.

The discussion only focuses on the results of the modelling exercise. Why did you report all the information on annual and seasonal patterns if you do not discuss them?

Answer 9

The "Discussion" section mentioned by the Reviewer, labelled 4.3, refers to the former section 4: "3-D Simulation of the DTE by the general circulation model LMDz". As reorganized, the discussion section now labelled 2.3.3, is more intuitively interpreted as referring to section 2.3 (2.3 Mechanisms responsible for this tropospheric excess: a numerical study). The general discussion and, in particular, on annual and seasonal patterns, is in the "Conclusion".

Conclusions - if your hypothesis that the burned area data do not catch the early fires is true, then why would the DTE only show an earlier start of the fire season in two of the 10 regions? which regions are these? What is the major vegetation in these regions?

Answer 10

Regions 6, 7, and, to a lesser extent, 8 where DTE shows an earlier start are regions for which the forest fractional cover is the smallest (in particular, closed deciduous forests and mosaic forest/savanna) but with more wooded grasslands and shrublands (see for example, Fig. 2 of Mayaux et al., 2004, quoted in the paper). We added (section 3.2.3): "These three study regions correspond to areas in which forests (in particular, closed deciduous forests and mosaic forest/savanna) contribute the least (see for example, Fig. 2 of Mayaux et al. (2004)) compared to the other regions."

I do not find figure 4 very informative

Answer 11

It has the merit of showing that both DTE and GFED display similar interannual variability when L3JRC is much less variable. Fig. 4c (now 8c) also present mean GFED fire emissions region by region. We would prefer keeping it.

Figure 5. Report mean annual rainfall for region for each year to give an indication of how comparable the two year-pairs are.

Answer 12

The matter is relatively complex as explained, e.g., by van der Werf et al., Glob. Biogeo. Cycl. (2008) or by Archibald et al., GBC, 2008. They show that a 13-month to 20-month period preceding and including the peak fire month could be used to estimate precipitation levels during the period when herbaceous fuels typically accumulate. We have verified that on such time periods the two year-pairs show precipitation patterns largely similar. We have added, section 3.2.2: "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)"

In figure 9 label the classes with the actual DTE intervals.... using a class is unnecessary and makes it difficult to interpret.

Answer 13

This figure (now fig. 5) has been redrawn. The difference between this new figure and the former one comes from the elimination of data points either over sea (a few remained), or for which the modelled DTE was smaller than a significance test (here: 0.08 ppm for the absolute value). Such a "significance test" already applied to figure 6 (former figure 10).

Why report both figure 9 and figure 10 ? Surely just one of these graphs will suffice?

Answer 14

We think that showing the great similarity between Fig. 9 (now fig. 5), resulting from numerical simulations, and Fig. 10 (now fig. 6), resulting from observations, is meaningful. Also, for reasons discussed in the text, they show different DTE dynamics. We would prefer keeping them.