

Interactive comment on “Regional-scale correlation between CO₂ fire emissions, burned areas, and mid-tropospheric CO₂ daily variations over southern Africa” by A. Chédin et al.

Anonymous Referee #2

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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. 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 extent. 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.

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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. 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!

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. 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.

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

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their quantitative faith in the DTE product.

Minor issues

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

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. I think that figure 5 is unnecessary.

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

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

Figure 2: Does this include the rejected values < 0.3 ppm?

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.

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