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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Signature of tropical fires in the diurnal cycle of tropospheric CO as seen from Metop-A/IASI" *by* T. Thonat et al.

Anonymous Referee #2

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This paper by Thonat et al. relies on CO total column retrievals from the IASI instrument on MetOp to investigate the diurnal cycle of fire emissions for that species. More specifically the authors exploit here the overpasses of IASI in the morning and in the evening (the same day) to extract a daily "day-night" total column difference, which they show to be positive above the fire-affected regions in the tropics. They draw time series of the column excess over four years (July 2007 - June 2011), which they then compare to time series of burned areas from MODIS and of CO emissions from the GFED 3.1 inventory. Based on the observation that the column excess reproduces better the time evolution of the emissions (especially the time-lag between maxima of emissions and the CO daytime column is removed), they suggest that the column excess is a more direct indicator of the fire behaviour.

The paper is well organized and also generally well written. I like the idea of exploiting the day-night differences in the IASI retrieved columns to investigate diurnal variability of emissions. However, and as the author note, this is a complex problem because of the changing sensitivity of IASI to near-surface CO concentration with thermal contrast. Despite the discussion in section 4.1., I don't think this issue is sufficiently and convincingly addressed here. In other words, I don't feel that the paper in its present form achieves its goal as it does not clearly demonstrate that the day-night column excess is truly a sign of the fire diurnal cycle of emissions rather than a consequence of the increased sensitivity of IASI during its morning overpass. This is of course critical for the interpretation of the data and the discussion on the diurnal cycle of carbon emissions by fires.

In my opinion the paper is therefore potentially suitable for publication in ACP, but can only be accepted after having addressed at least the first general comment below.

GENERAL COMMENTS

1. The algorithm used here (presented in Thonat et al., 2012) consists in retrieving from IASI hyperspectral measurements a CO total column by a double difference approach (one on the measurement using appropriate channels sensitive and insensitive to CO, and the other on simulated spectra from forward simulations with a known and constant background CO column, using the same channels). The resulting difference is related to a CO excess by the Jacobians. Although very simple in comparison to iterative "physical" retrievals the approach is fast and efficient and has allowed deriving time series of CO total columns from IASI morning and evening overpasses (Thonat et al., 2012). It is clear from the present paper that in the tropics the difference is almost always positive, reflecting higher retrieved CO total columns during the morning (day) as compared to the evening (night) overpass of IASI. The critical aspect is that there are two possible interpretations for this positive signal:

- A real enhancement at day following the mechanism suggested here: CO build-up in

the boundary layer at night during the smoldering phase and subsequent uplift in the morning

- A sensitivity effect, which would be caused by the fact that the large CO columns induced by the fires at the surface –possibly similar at day and night– are just better captured by IASI during the day, when there is more favourable thermal contrasts.

The authors investigate this shortly in section 4.1 but only by looking at the impact of the increased daytime sensitivity of IASI on the background CO column (the one used in their forward model). They show that the sensitivity to the lower layers is indeed better in the morning and results in larger apparent (background) columns. They show furthermore that this apparent enhancement would have the same seasonality as the computed day-night total excess seen by IASI. They finally anticipate that the delta-qCO would be impacted in the same way but they don't quantify it. This is in my opinion a serious limitation, as it is absolutely needed for supporting their analysis of the diurnal cycle in section 4.2. For the paper to be accepted, the authors have to give convincing evidence that if the impact on delta-qCO was taken into account, they would not reproduce the measured excess or at least a big part of it. Their statement page 26017 that the 2 ppv amplitude of the seasonal cycle using the background profile is far from the 12 ppbv measured is indeed not sufficient. What would be the amplitude for a profile far from the background, with especially very high CO in the boundary layer (this would be expected above the source)? This needs to be shown (at least using one typical heavily polluted profile instead of the background one; I suppose that there are no difficulty to that in 4AOP) and discussed carefully. Similarly, the argument page 26013 that the difference vanishes above ocean is not sufficient to claim that excess above the continents are due to the fires (as this is expected also from the pretty stable thermal conditions above the oceans).

2. If the demonstration can be made that the measured excess is for sure not due to the better sensitivity of IASI to boundary layer CO during daytime, the authors need also to better explain why the excess is in many months not seen right above the fires but

pretty far away (Figure2, for example in September). This is puzzling. In the author's hypothesis, the excess day-night CO is linked to the build-up of high concentration at night, followed by uplift in the morning but wouldn't this occur in the near vicinity of the source? In other words are the timescales of horizontal transport that the author point to explain the spatial and temporal (2 month lag page 26011) mismatch between emissions and CO columns vs. vertical transport consistent with the proposed emission mechanism and diurnal cycle? This should all be better supported.

SPECIFIC COMMENTS.

1. Page 26009 and Figure 1. If I understand it correctly, you use model temperatures from ECMWF but retrieve the surface temperature. I would like to see show maps of thermal contrasts (for day and night) in parallel to the CO distributions in Figure 1. This would be helpful also for analysing the results.

2. Section 3.2.1. and Figure 2 (also for the other Figures): Please specify how the averages have been performed, both for the total columns and for the difference: what is gridding? Do you consider per grid cell daily means from which you compute the difference (in each cell)? It is for these reasons unclear why the difference (Figure2d) contains so many gaps. That is important also for the discussion per region in section 3.2.3. Note that Figure 2 is too small and blurry. Lat/long (mentioned in the text) are unreadable.

3. In section 3.3 (page 26016, line 1) you mention a correlation coefficient of 0.6. Do I get it right that this is by excluding the two regions AfsE and AmC? If yes it should be made clearer in the abstract and in the conclusion (26020, line 13) that the 0.6 correlation coefficient is not considering the entire dataset (and it would in fact be good to give the value for the entire dataset as well).

4. Section 4.2. The fact that the CO2 and CO day-night differences exhibit opposite signs is surprising The proposed mechanisms could indeed lead to this but even if the smoldering phase emits more CO than CO2, the flaming phase is still expected to

release significant amounts of CO, which would follow the same uplift mechanism as that proposed for CO2. Why are these enhancements not better seen in the IASI CO data? Or would this mean (again supposing that the proposed diurnal cycle is real) that the CO excess from the mid-troposphere is underestimated (as a significant fraction of CO in the upper troposphere would have been subtracted)?

MINOR COMMENTS AND TECHNICAL CORRECTIONS

1. The last sentence of the abstract is unclear as the objective is not stated. "The need to develop complementary approaches"; but to achieve what? Please rewrite this sentence.

2. Page 26005, line 10. "..the components of radiative forcing" is too vague. Please be more specific.

3. Page 26006, line 24: ". . . A new approach". I suggest to remove "new" as these paper date from 2005 and 2008.

4. Page 26008, line 8. Specify that the 2200 km refers to the swath on the surface

5. Page 26008, line 10. Be more specific to the pixel area. 12 km at nadir may be a bit misleading (at least say it is circular).

6. Page 26009, line 16. What is meant with "A negative thermal contrast has symmetric effects"? Temperature inversions also increase sensitivity significantly.

7. Page 26010, line 5: Is it sound to use the term mid-tropospheric CO considering the possible impact of the sensitivity to the lowest layers? Furthermore, "tropospheric CO" is used in other occasions (e.g. in the abstract). I would suggest being homogeneous in the notations throughout, to define clearly these terms and verify that they are consistent with what is actually measured.

8. Page 26011, line 4. ". . .enhance the links between fire activity and tropospheric CO" is not appropriate; The attempt is just to make it more visible, no?

9. Page 26013, line 5-6. "The daytime signal is observed just above the fire". Are you referring here to the spatial location (in which case this is optimistic –see general comment 2-) or to the better match is the maxima of the CO excess as compared to the emissions (From Figure 4)?

10. Page 26013, line 10: Where is transport from the NH seen? Is it not too far South to be affected by NH transport?

11; Page 26013, line 17: "some discrepancies can be found between the two of them" does not sound right. Please rephrase.

12. Page 26014, line 23. Suggestion to replace "Like" by "As for"

13. Page 26019, line 6 vey » very

14. Page 26019, lines 11-12 "Suggestion to replace "CO emissions happening at night are" by "CO emitted by night is"

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26003, 2014.