Atmos. Chem. Phys. Discuss., 14, C13068–C13075, 2015
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Atmospheric Chemistry and Physics Discussions

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14, C13068–C13075, 2015

Interactive Comment

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.

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Received and published: 29 April 2015

First, we would like to thank the reviewer for his/her comments which have helped improving the paper and the analysis of the results.

- In general, the level of scientific writing could be improved. Please try to avoid subjective terms like 'very good' and 'extremely high' in the text.

-> We have taken care of avoiding such expressions in the revised version.

- The inventory-based comparison should be extended, since it now only focuses on GFEDv3 and MODIS BA (which is actually used in the GFEDv3 modeling framework and therefore the overlap between both datasets in not surprising at all). From a bottom-up perspective, it would be interesting to add inventories that rely on other methods as well, like e.g. the Global Fire Assimilation System (GFAS; based on fire radiative power (FRP)) and the Fire Inventory from NCAR (FINN). Extending the comparison with these inventories could give more insight in the processes that explain the discrepancy found with the diurnal cycle of IASI CO, and might bolster your hypothesis on the contribution of changes in the flaming and smoldering phase.

-> We have extended our comparisons between diurnal CO and fire products to the GFASA1.0 monthly product. In southern Africa, GFAS1.0 is lower than GFED3.1 but follows in general the same seasonal cycle (picking ~1 month earlier than the diurnal signal of CO) and agrees better with the diurnal signal of CO in terms of interannual variability. The agreement is also better in terms of spatial repartition. In the revised version, Fig.2c is plotted for both GFED3.1 and GFAS1.0: the emissions on the East of the area (35-40°E) between July and September, that are not "seen" with the diurnal signal of CO (see Section 3.2.2), are lower for GFAS1.0 than for GFED3.1. GFAS1.0 emissions are also lower in this area than the ones located between 10 and 30°E, in agreement with IASI CO signal.

This overall better agreement is illustrated by a better correlation coefficient found between GFAS1.0 and the diurnal signal of CO (R2~0.7 instead of 0.6 for GFED3.1) on all the tropical regions.

The reasons why GFAS and GFED differ are certainly complex and investigating these reasons exceeds the scope of this paper. The MODIS BA have been used here to provide a more "direct" observation of the fire location. Thus, in our view, the fact that GFED uses the MODIS BA in its framework is interesting since the agreement with the diurnal CO signal should be closer to the emissions than to the BA themselves, IASI CO providing a signature of the emissions themselves. Using the MODIS BA also permits to extend the comparison to the beginning of 2012, a period that is not available from GFED3.1.

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- How confident are the authors that the mechanisms described for the boreal forest fire case (P17L1-7) are valid for Africa savanna fires or deforestation fires in South America?

-> We do not (and cannot) state that the mechanisms are exactly the same. The results from Ferguson et al. (2003) stem from a punctual (in time and space) study, and depend on the weather in the studied days, the topography, etc. We draw a parallel between our two studies, supported by our findings and the evolution of the boundary layer height, which is described just after in the text.

- In contrast to the 9.30am measurements, I guess that the 9.30pm measurement can be affected by transport from other regions? If so, the diurnal cycle will be impacted by CO transport from other regions as well. An atmospheric transport model could be used to check whether transport does play a role in this diurnal cycle, or, at least, provide the reader with some references to convince that this is not the case. In general, more discussion on the role of transport is important, especially regarding the comparison of the different regions (vegetation types) in Africa. If transport does play a role, interpreting those results does not make sense.

-> Both the 9.30 a.m. and 9.30 p.m. measurements are affected by transport from other regions. When talking about the remaining high values of CO in southern Africa outside of the fire season (for example in Fig. 3), both "day" and "night" IASI CO mixing ratios are influenced by transport, and they are influenced the same way.

Concerning the effect of transport on the diurnal signal of CO, it is of course not excluded that both large scale circulation and local conditions have an influence. Rio et al. (2010) showed that the DTE of CO2 was not always necessarily located just above the source, because of large scale advection (" [. . .] the real DTE signal can be significant in surrounding areas, due to preferential directions of the large scale advections."). Chédin et al. (2009) showed that the DTE of CO2 could be negative on a daily scale due to particular horizontal winds. However, on a monthly basis, outside the source

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region, the DTE daily variations tend to cancel each other out.

A distinction should be made between the influence of large scale (and long distance) transport that affects day and night IASI CO for months, and the particular horizontal winds that can punctually affect the day minus night difference of CO.

On monthly averages, the diurnal signal of CO is thus mostly located in the vicinity of the sources and can be interpreted in relation to the region above which it is located.

This discussion has been added to the text.

- Instead of July 2008, how does the boundary layer behave in southern Africa in the months of August-November? How does the boundary layer behave in South America in the fire season?

-> The BL behaves the same way in August-November as in July, but the maximum of the height (reached at 12:00) increases from July ($\sim$2.0 km) to September ($\sim$2.5 km), and then decreases until November (1.5 km).

In South America, a similar behaviour is found (based on ECMWF forecasts), except that a lower height is reached in July-October (not higher than $\sim$1.8 km). This has been included in the revised text.

- What would be the impact of deforestation fires in South America on the diurnal CO signature of IASI? These fires are often started in the afternoon (>9.30am), and represent a significant part of CO emissions in the South American Continent. The same counts for Indonesia.

-> A diurnal signal of CO is found in South America too and it is in agreement with fire activity. It is located in the area of the “arc of deforestation” and its evolution follows the fire activity.

It is more difficult to see what would be the impact of Indonesian fires, as it is a smaller region that is affected by persistent cloudiness – few clear sky observations are avail-

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able.

- Besides the fact that the diurnal CO signal may reveal some interesting findings with respect to the temporal variations in fire emissions, the method could be potentially useful above regions where other sources of pollution occur, like e.g. Asia. I'm wondering why, in the global analysis (Figure 7), Asia is left out of the analysis. Indonesia and Southeast Asia are important tropical regions from a biomass burning perspective, and given the proximity to anthropogenic pollution sources the method could be specifically useful in these regions.

-> Southeast Asia and Indonesia are smaller fire sources than Africa and South America, on which we put an emphasis. And as indicated above, we are limited by the number of clear sky observations to study these regions in details.

- I miss some discussion on how to proceed with this method in future. Are there other sensors or upcoming missions that could be used? What about sensors with surface sensitivity to CO, like MOPITT? Could the work directly be combined with CO2 observations? In general, I miss a sort of guidance here.

-> We have added a paragraph about other and future spatial missions in the conclusion. Metop-B was launched in 2012 and Metop-C will be launched in 2017, so IASI will provide at least 20 years of observations, at the same equator crossing times, allowing us to study on the long term the evolution of CO, its diurnal cycle and its relation with fires. IASI-NG, on the same orbit, will cover the period 2020-2042.

Our CO retrieval method also works with Aqua/AIRS observations (see Thonat et al., 2012), whose passing times are 1.30 a.m./p.m. CrIs, with the same characteristics as AIRS, was launched in 2011 and will also be on the JPSS program planned for 2017.

Terra/MOPITT gives CO measurements at 10.30 a.m/p.m. So, with IASI and AIRS, it gives access to 6 points a day in the diurnal cycle of CO. However, our retrieval method cannot be applied to MOPITT, which is a different instrument from the AIRS and IASI

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TIR sounders, with only 8 channels located in the thermal infrared part of the spectrum, which prevents using the double spectral difference approach to derive CO columns. Having a different retrieval chain can lead to biases with our AIRS and IASI retrievals. Nonetheless, studying both TIR and TIR+NIR CO retrievals from MOPITT, which have different vertical sensitivities, can give valuable information on the vertical distribution of CO.

Since IASI enables the retrieval of other gases emitted by fires such as CO2 or CH4, the simultaneous study of the retrieved fields of CO and these other gases is clearly foreseen. In particular, the extension of the DTE of CO2 to other regions and all the IASI period is currently under investigation.

Technical corrections

All technical corrections and suggestions have been taken into account in the revised paper. We just have a few remarks on the following ones.

P5L23: From a CO perspective fire emissions are not particularly strong in southern Africa. Deforestation fires in the Amazon and South-East Asia, and boreal wildfires have in general stronger smoldering components and therefore a stronger CO signal

-> According to GFED3.1, Africa (and in particular southern Africa) is a major contributor to CO fire emissions. The tropics gather 80% of fire emissions of carbon, that's why we have focused on CO retrievals in this region. Moreover, southern Africa is a region weakly affected by cloud coverage (as opposed to Southern America and South-East Asia), yielding a higher number of day and night observations.

P8L7: Why not take the average of 2007-2012 instead of 2008 only?

-> 2008 is an average year in terms of fire activity. We have checked that the conclusions do not differ when plotting the average of 2007-2012.

P11L5-6: 'The day-night signal is observed just above fires'. Is it? I'm not sure when looking at Figure 3? -> The diurnal signal of CO can indeed be important where fires

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are not indicated by the MODIS BA, but the main signal is always located in their vicinity (for example it is not located over the oceans).

“The day-night is observed just above fires” has been replaced by “The day-night signal mostly captures CO over fires”.

P12L5: Looking at Figure 5, I’m not convinced that these defined regions actually capture a certain vegetation type. For example, H9 seems very heterogenic.

-> These regions were defined by Hoelzemann (2006) to study the seasonal cycle of fires between different emission inventories. Although it is not stated that this choice was made to fit to different vegetation types, she found different emission source distribution between these ten regions in terms of fuel types (see Fig. 3.9 in Hoelzemann (2006), where e.g. wildfires in H1 are dominated by forest fires, and wildfires in H9 are dominated by savanna and grassland fires). This has been specified in the revised version.

P13L26: So did you include these areas to derive the r2 of 0.6? If not, make clear in the text.

-> Fig. 8 has been remade to fix a problem with the average computation. The conclusions are the same except it is for the entire dataset that we have R2$\sim$0.6. We only mention R2 value for the entire dataset in the revised paper. As said above, we have also specified that R2 value is 0.7 for GFAS1.0.

P17L1-17: This is the case for a boreal fire, which is in general quite different than a savanna, cropland or tropical deforestation fire. How would the pyroconvection and natural convection work for a savanna fire in southern Africa?

-> Answering this question would require to use a pyro-thermal plume model like Rio et al. (2010) did, and it is goes beyond the subject of this paper. In the conclusion we call for such a use that could confirm our hypothesis.

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

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