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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric carbon monoxide variability from AIRS and IASI under clear and cloudy conditions" *by* J. Warner et al.

Anonymous Referee #1

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General Comments

This manuscript uses 8 years of CO observations from AIRS and 3 years from IASI to evaluate the interannual variability and trends in CO columns in both the northern and southern hemispheres. The authors provide a new analysis of the effect of cloud-clearing on the retrieved CO columns from AIRS and introduce a new method to distinguish between background concentrations and fresh emissions with satellite data.

In general, the paper touches on several relevant and interesting issues, and in terms of content the paper is well-suited to ACP. The analysis of the effects of cloud-clearing are particular relevant for multi-platform analysis using satellite data. However, the

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manuscript lacks depth in the discussion of many of the results, and there are a number of minor issues detailed below that require modification.

Specific Comments

Abstract: The references in the abstract distract and seem unnecessary – these can be given in the main text.

16339, 27-28: It would be useful to have a similar sentence describing AIRS here, especially since this is the main instrument used for the paper.

16340, 10-11: MOPITT isn't particularly relevant to this discussion – is there a reference for cloud detection for AIRS or IASI that could be cited instead?

16341, 13: Please define "granules" in this context

16343, 5-6: "we equate the total pixels as the cloud-cleared cases" – I don't understand what this means. Can this be rephrased?

16343, 8-9: It would be much easier to make this point if Fig. 3 included difference maps between the cloud-cleared and cloud-free cases (even just for the clear-sky boxes)

16343, 19-25: There is not much of a punchline here – it would be good if this discussion included an idea of whether the DOFS differences between the two cases were large (or worth considering when choosing a dataset).

16344, 15-16: March-May isn't typically a major biomass burning season for most of the Southern Hemisphere, and Fig. 3 doesn't show SH burning (except a bit of an extended plume from NH burning in Africa).

16344, 18-19: "using the modes... to represent biases." More explanation is needed for this process. This seems to be a crucial component of the new methodology to separate fresh emissions from background air, but it's completely unclear what was actually done. Also, the explanation of this process would fit better in the next section,

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which from its title is focused specifically on distinguishing between these populations.

16344, 20: “Decreasing trends” On first read, I thought this needed more discussion, but now I realize trends are shown and discussed later in the paper. I would suggest either removing here or putting in a note that these will be discussed later. If this discussion is maintained, it would be good to show trend lines earlier, and also to be more quantitative (but again, both of these are done later).

16344, 25-26: “more regular variability” – what does this mean? Does this refer to interannual variability? Seasonal? Does “regular” refer to a repeatable cycle without as much variability, or does it mean there are more frequent excursions from the mean? Can any of this be quantified?

16344, 28: “without causing large biases” – this is one of the discussions I think needs to be expanded. This is a very important issue (potential biases arising from cloud-clearing) that hasn’t previously been addressed. It would be nice if there were some more quantitative conclusions from this analysis. How large are the biases? How are they affected by averaging timescale? Should users of these data avoid cloud-cleared products for short-term variability studies, as done here?

16346, 8-9: This argument doesn’t seem particularly cogent to me. This may have played a role, but it’s a little misleading, as these years also show offset between the unsmoothed peaks.

Section 4: The correlations between AIRS-derived “new emissions” and the emissions inventories are quite convincing. Can this go a step further? If the goal (as stated in the introduction) is to provide near real-time fire detection, is there a way to show that can be done with these methods? Or at the very least that it is possible to distinguish between fossil fuel and fire emissions?

16347, 12: “IASI L2 cloud information” – there was a lot of discussion of AIRS cloud information, but no previous detail for IASI. Is that a cloud fraction?

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16347, 18: The two sensors are compared for 2002-2011, but IASI has been in orbit for less than half of that time. Why not compare over their periods of overlap for this figure? There would be less sensitivity to any anomalously large fire emissions in early years of the record. Also, Fig. 11 shows that IASI was analyzed through 2012. Could the AIRS record also be extended to 2012 here to have a longer overlap period?

16347, 28: "AIRS (09:30 LT) and IASI (13:30 LT)" – I think these two are swapped. Also, can the overpass time difference really explain most of the difference? I wouldn't expect a large diurnal cycle in CO – or is this related to mixing between the boundary layer emissions and the free troposphere where the instruments are sensitive?

16348, 2-3: "one global a priori" – what does IASI use? The choice of a priori for each instrument wasn't previously discussed, but would be useful background information.

16348, 6-7: "bias ... due to IASI CO being too low" – that might be reasonable for most of the regions discussed, but for Antarctica, AIRS looks unrealistically high. This shouldn't be lumped into the same statement, and should perhaps be commented on. Alternatively, it should be noted that Worden et al. (2013) show that AIRS is higher than all other TIR satellites for the southern hemisphere (which would include most of these regions).

16348, 24 – 16349, 17: This is another section that needs more detail. First, how are the trends computed? Are the data deseasonalized? Are the trends significant? Second, some discussion of the possible drivers of these trends is needed. Third, some context from the literature is required. For example, why are the IASI NH trends computed here positive, when Worden et al. (2013) show decreasing (but not significant) trends? In general, this should be more closely related to the Worden et al. (2013) analysis.

Figure 1: The caption needs much more information – I don't understand the figure currently. Which parts are the granules? Which are the pixels? Are the colors relevant? What do the numbers mean? Also, the numbers in green are not readable.

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Figure 2: What is the time frame for these maps (1 day?)? It would be good to state this in the text or caption. Also, it is unclear what is meant by "AIRS cloud ratio" – this should be defined. From the figure, it appears that every pixel is either 0, 1, or undefined. If that is the case, it would be much clearer to have a legend rather than a colorbar. As it stands, it appears there are many pixels with cloud fraction <0 or >1, which doesn't make sense to me. Are these just unobserved pixels? If so, please clarify on the figure and fix the colorbar. Finally, as written, the legend is confusing. It states that the "cloud coverage" is "defined by . . . where CloudFraction=0." But that is just where it is clear, not where there is cloud coverage. Again, this would be clarified with a legend rather than a colorbar, but either way the caption needs to be re-worked.

Figure 5: The upper two panels are swapped (according to the text, NH ocean should be on the left and NH land on the right). This really confused me when reading the text. The figure caption needs more information – what are all the numbers? Also, it would be clearer if the two (solid/dotted) lines on the legend were right under the words "clear" and "cloudy", and maybe also listed in the caption, as it took me a long time to figure out what the different lines meant. Finally, the difference between blue and red lines needs to be given somewhere on the figure or in the caption.

Figure 7: It would be helpful for the discussion (and for the attribution of the offset) if the separate GFED3 and MACC inventories were also plotted somewhere (potentially below the current plots).

Technical Comments

There are some grammatical errors throughout (that I have not detailed). It would be good to have the manuscript copy-edited, or at least closely edited by the authors.

16339, 10: replace "yr" with "years"

16340, 9-10: "referred as" should be "referred to as"

16339, 25: this sentence would be clearer if "from 2002 through the lifetime of AIRS"

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were replaced with “since 2002” as AIRS is currently still operational

16344, 9-10: specify that this is for NH land, not all NH scenes

16345, 24: degree symbol missing from end of line

Figures: for most figures, especially multiple panel maps, it would be useful to remove all the redundant information from the figure titles and replace with a clear statement of what is unique for each panel. For example, the top panels of Fig. 2 would just say “Daytime, clear only” and “Daytime, all data” so the reader can quickly distinguish the relevant characteristics.

Figure 6: needs a legend

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16337, 2013.