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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.

J. Warner et al.

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We appreciate very much the comments by reviewer #1. We have answered all questions; with each answer starting with "ANSWER:" We also appreciate this reviewer for recognizing that this paper "touches on several relevant and interesting issues" and "The analysis of the effects of cloud-clearing are particular relevant for multi-platform analysis using satellite data."

—- Interactive comment on "Tropospheric carbon monoxide variability from AIRS and IASI under clear and cloudy conditions" by J. Warner et al. Anonymous Referee #1 Received and published: 25 July 2013

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

ANSWER: We have made some general changes in the manuscript based on the comments from both reviewers. 1) As Reviewer #2 pointed out that "Each of these topics could be the basis of a very rich and useful paper. . .", we have removed all discussions with IASI CO including the intercomparison with AIRS CO. We will develop the IASI CO related studies in a separate paper at a later time. 2) We have moved the PDF discussions from Sect. 3 that addresses clear vs cloud-cleared issues to Sect. 4 where we focus on the background CO vs fresh emissions. Likewise, we have limited our discussion on the trends only in Sect. 4 as suggested by Reviewer #1.

Specific Comments

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

ANSWER: The references in the abstract are removed and now are 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.

ANSWER: Good suggestion! We added "AIRS is a grading instrument on board

EOS/Aqua satellite, launched on May 4, 2002 by NASA. As a thermal hyperspectral sensor, AIRS has more than 2000 channels useful for applications including weather, climate, and air quality studies." in the text.

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?

ANSWER: The discussion of cloud detection is not really necessary here, so we removed the sentence.

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

ANSWER: We added "granules (units of data stored as files for satellite data)"

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

ANSWER: We removed this sentence and re-wrote the previous sentence as: "Note that the clear sky cases are embedded in the cloud-cleared cases under discussion."

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)

ANSWER: The clear-sky CO is a subset of the cloud-cleared data for the cloud-free cases as stated in the text. There is no difference between the clear-sky CO and the cloud-cleared CO when the same ensembles are used.

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).

ANSWER: We aim to understand the effect of cloud-clearing in terms of measurement sensitivity, but we don't intend to push users into choosing against the cloud-cleared products because the differences here in DOFS are relatively small. We added the

following sentence in the text to clearly state the conclusion of this discussion: "This comparison indicates that the cloud-clearing process may have reduced the DOFS, although not by a large amount ($\sim$0.2), in the CO retrievals."

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).

ANSWER: True! We discuss this in a general sense since we have examined more than 120 such figures. This is just one example showing the typical distribution of CO over SH Land for under clear and cloudy conditions. The large CO values over SH Land are due to biomass burning (BB) events even though we are not considering a high BB season.

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

ANSWER: Good suggestion! We have moved the discussion for the Gaussian fit and the variability discussions to Sect. 4 and we do not discuss the separation of background and fresh emissions in Sect. 3 where the focus is clear vs cloud-cleared retrievals.

ANSWER: We have also added more details in the text on the Gaussian fits and the variability from the fits: "We use the modes of the fitted Gaussian functions from each monthly PDF to represent the averaged CO values based on the fact that, for a Gaussian function, the mode is the same as the mean. The tropospheric CO histogram distributions, once fitted by two Gaussian functions, can be considered as Gaussian to a good accuracy."

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).

ANSWER: We have removed the trend discussion from this section.

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?

ANSWER: We have changed this to “more regular seasonal and interannual variability”. We have also removed the discussion on the variability differences between clear and cloud-cleared cases, and focus on the differences in the CO values.

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?

ANSWER: We have focused more on the CO differences between clear-sky and cloud-cleared by adding a difference curve in Fig. 5 to show the CO differences between the clear-sky and cloud-cleared cases more clearly. The CO values do not differ very much between the two ensembles for most of the applications. So, no, the users should not avoid using the cloud-clear products. In this study, though, we benefited from more clearly defined two-peak PDFs which apply when using the clear only datasets.

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.

ANSWER: There are two major reasons why the two datasets differ. 1). AIRS measurements are from 500hPa and the inventory data is the estimate of the net emission at the surface. Considering the CO lifetime in the troposphere of 1 to 3 months, there could be a delay from the time of emission at the surface to being observed at 500hPa and the CO can be accumulated over some time. 2) The CO sensitivity from thermal sensors depends on the surface thermal contrasts (Deeter et al., 2007). Higher surface temperature or thermal contrast in the summer months may have resulted in higher CO values, which are associated with CO at lower altitudes. This may happen more frequently in the summer months than in the spring months.

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?

ANSWER: The actual near real-time fire detection will be the subject of a future study. The study under discussion builds the foundation for these further studies by providing a way to separate the fresh emissions from the background CO. The CO climatology based on the background CO will provide references for the real-time fire detection. The background CO would include fossil fuel emissions as continuous emission sources, unlike the fresh 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?

ANSWER: IASI discussions have been removed.

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?

ANSWER: We originally only included AIRS data to the end of 2010 to prepare for an AGU talk when we first introduced our CO trend study using multi-year AIRS CO datasets. We have removed IASI related discussions, but extended AIRS data to the end of 2012. The AIRS team is no longer distributing V5 products beyond the end of Feb. 2013 since they are replaced by V6 products.

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?

ANSWER: This discussion has been removed in this paper.

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.

ANSWER: No longer relevant.

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).

ANSWER: The IASI discussions are no longer relevant to this paper. True, a number of studies have documented the fact that AIRS V5 CO tends to overestimate the CO field in the SH. AIRS team has made major improvements in this matter in their V6 version.

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.

ANSWER: This is a great suggestion. We have expanded the material related to AIRS CO short-term trends in the text. We have removed the IASI discussions to a separate paper for future publication because more detailed analysis is needed to support the answers for questions such as the reasons for the positive trends for IASI CO in the NH. Also, with 4 years available IASI data, it is premature to discuss trends in a meaningful way.

ANSWER: The trends are computed using a running average. Additionally, we use only full years so the trend estimates not affected by seasons. The trend is significant at greater than 2 sigma everywhere except the background fit over SH Land (1 sigma) and the fresh emissions over SH Land, where the CO emissions are due to large and somewhat irregular biomass burning events. We have also added discussions to compare our results with the literatures.

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.

ANSWER: We have updated this figure and added details in the caption.

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.

ANSWER: We have redone this figure with more details and updated the text as follows: "AIRS clear coverage defined by the MODIS cloud mask for March 4 2006 is shown in blue in Fig. 2 top panel and the total clear sky pixel ratio is approximately 14.9%. If we choose to define a clear AIRS pixel when all MODIS pixel are flagged clear, there would be only 13.3% clear AIRS pixels per day. AIRS clear coverage is also defined by AIRS measured radiances, instead of by the MODIS cloud mask, as part of the L2 products. The blue pixels in Fig. 2 middle panel show AIRS L2 clear sky cases (when CloudFraction=0 in the L2 product) and the total clear sky pixel ratio is ∼24.3%, which tends to overestimate the amount of clear coverage compared to Fig. 2 top panel using the MODIS cloud mask. AIRS L2 cloud ratio products can be compared to those defined by the MODIS cloud mask only under clear sky conditions because the MODIS sub-pixel (1x1 km2) cloudiness is unknown. The clear sky coverage differences between MODIS and AIRS L2 are shown in Fig. 2 bottom panel, where the blue pixels represent the cases when both MODIS and AIRS L2 detect clear sky (∼9.5% of total daily pixels). The green pixels are when MODIS detects clear sky, but AIRS L2 failed to identify clear sky cases (∼5.4%), whereas the magenta pixels are clear sky detected by AIRS L2, but not by MODIS (∼14.8%)."

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.

ANSWER: The panel swapping was a mistake during the type-setting for the publication on ACPD. Our apologies for not recognize the mistake! We have improved the figure titles and captions. Also we added legends and removed the irrelevant numbers on the panels.

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).

ANSWER: We have added another panel for this to Fig. 7 (as new Fig. 8).

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"

ANSWER: Corrected.

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

ANSWER: Corrected.

16339, 25: this sentence would be clearer if "from 2002 through the lifetime of AIRS" were replaced with "since 2002" as AIRS is currently still operational

ANSWER: Corrected.

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

ANSWER: Corrected.

16345, 24: degree symbol missing from end of line

ANSWER: Corrected.

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

ANSWER: We have made necessary changes to the titles, captions, and legends to make the figures easier to understand.

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