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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of global and regional CO burdens measured from space between 2000 and 2009 and validated by ground-based solar tracking spectrometers" *by* L. N. Yurganov et al.

Anonymous Referee #1

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Review

Title: Analysis of global and regional CO burdens measured from space between 2000 and 2009 and validated by ground-based solar tracking spectrometers.

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This paper presents an analysis of interannual variability of the global CO burden as observed by two IR satellite instruments (MOPITT, AIRS) and compares variations and

changes of the burden with emission estimates based on independent other satellite observations (GFED via burned area and fire counts). Satellite observations of CO are corrected for potential global biases by comparing with – limited - ground based FTS observations. The authors extensively describe the correspondence and differences between both satellite datasets before and after corrections and identify a number of issues in both datasets. After correction, the authors show that there is qualitative agreement between CO burden anomalies and CO emission anomalies for the period 2000-2008, and they argue that part of the 2008 reduction in observed global CO burden may be related to rather quiet tropical biomass burning, rather than due to the economic recession.

The analysis presented here provides an interesting and valuable methodology to simultaneously analyze global CO measurements and emission estimates without the additional effort of modeling (either direct or inverse). Furthermore, the use of multiple satellite records also helps in better quantifying potential biases between both and their effect on the global burdens (or lack thereof).

As a whole, I very much appreciate this paper as it continues to build on previously published work and further extends the combined use of various satellite measurements, ground based observations and emission databases.

There are a however a few questions and/or remarks that should be addressed, in particular emission amounts related to the GFEDv2 database that are quoted in the text but which appear to be wrong.

Questions and remarks

- First of all, an obvious question is why MOPITT-v3 is used when earlier this year MOPITT-v4 has become available. Non-published results suggest that MOPITT-v4 compares favorably to MOPITT-v3 (much smaller biases; http://www.acd.ucar.edu/mopitt/validation/val-mr-v4.shtml). On the other hand, I would not expect that results would significantly alter, also because biases in MOPITT (and

AIRS) are corrected in this study using some ground-based truth (FTS). Furthermore, as far as I am aware of there is currently one paper in peer-reviewed literature using MOPITT-v4 data [Pfister et al., ACPD, 2009] which is not a validation paper. Hence, it is rather difficult to assess the quality of MOPITT-v4 products, although obviously it can be expected to be better than MOPITT-v3. Summarizing, although I would be surprised in results would drastically change (which in itself would be an interesting result), it could be very helpful and interesting to include MOPITT-v4 results, also because it will become the new standard and at some point a similar analysis with MOPITT-v4 will/should be done (also by other groups for other instruments and other published analyses and results). Hence I very much encourage the authors to include MOPITT-v4, although I do not find it a crucial issue.

- Page 24877, lines 8-12. Provide a short description of how Indonesia, Siberia, Africa and South America are defined. I could not reproduce the numbers quoted in the document.

However, for the following regions I got the following range from GFED for 1997-2008:

(via http://www.falw.vu/∼gwerf/GFED/data)

100-140E, 10S-10N (Indonesia): 231 Tg (1997), 10.7 Tg (2000)

105W-35W, 60S-15N (South America); 108.6 Tg (2007), 36.0 Tg (2000)

45E-180E, 50N-80N (Siberia); 95.4 Tg (1998), 13.7 Tg (2004)

30W-60E, 40S-20N (Africa); 196 Tg (2001), 162 Tg (2003)

30W-60E, 40S-20N (Africa); 32% of total global BB (1998), 54% of global BB (2000)

Global: 590 Tg (1998) and 314 (2008).

Differences with numbers stated in the document are large, hence, it appears the numbers stated in the paper are incorrect or that I am looking at different regions. Please check the numbers. Furthermore, given the global burden of 314 Tg for 2008 it appears

that the numbers are not calculated up until 2008. Please correct if this is the case.

- Page 24877, line 14-19. It is noted that for the period 1985-1997 anthropogenic emissions are assumed to be relatively stable, but that because of the recent economic recession these emissions may have dropped from 2008 onwards. However, from 1998 to 2007 strong economic growth in especially East Asia has probably led to a considerable increase in anthropogenic CO emissions [Ohara et al., 2007, ACP] and increases in CO concentrations as well (see for example Shindell et al., 2006; JGR; Tanimoto et al., 2008; ACP). This should be noted here as well, and is also of relevance for the later analysis of the paper (why does this analysis not see an increase in NH CO burden in MOPITT for 2001-2007, for example).

- Page 24881, start of section 2.2. It is noted that no efforts were made to reconcile differences in the vertical sensitivity of both instruments, because this would require information about the "true" vertical stratification of CO. However, according to Luo et al. [2007, JGR], either MOPITT or AIRS can be adjusted without this knowledge. Fore example, MOPITT can be adjusted for differences in the averaging kernels of AIRS and MOPITT:

CO[MOPITT;SMOOTH] = AK[AIRS] X CO[MOPITT;RETRIEVED] + (I - AK[AIRS]) X CO[MOPITT;APRIORI]

After which CO[AIRS;RETRIEVED] and CO[MOPITT;SMOOTH] can be compared.

Some discussion of this issue is required. Results from Luo et al. [2007] suggest that a considerable improvement in total columns can be derived by applying these corrections (see their table 1): differences between MOPITT and TES can be reduced from 11% to about 5%, but most of this improvement is related to the different a priori in MOPITT and TES retrievals. This is not an issue for MOPITT and AIRS, as they use the same a priori. According to Luo et al. [2007], adjusting for the different averaging kernels only improves the differences between MOPITT and TES by an additional 1%.

Arguments for not applying such a correction might be that on the one hand both instruments are validated independently with FTS measurements, that corrections are applied based on this validation, indirectly taking potential differences due to differences in averaging kernel and a priori into account, and that in the later analysis of interannual variability the anomalies in the burdens are compared, not the actual CO burdens (which also indirectly takes any differences due to different averaging kernels into account).

Although it appears unlikely that this issue would change the results of this paper, some discussion would nevertheless be very beneficial.

- Page 24885, lines 6-9. It is noted that in absence of TC validation in the tropics validation results for the NH and SH are averaged for the tropics. However, this may not be valid. For example, Emmons et al. [2009] presents a validation of MOPITT with MOZAIC data for various locations, among them a few sites in (sub)tropical South America which show the largest negative biases of all validation results. Hence, averaging NH and SH biases may not be justified, although at the moment it is unclear how this can be determined and it appears that the comparison between MOPITT and AIRS burdens improves in the tropics. Some words of caution could be added. Furthermore, specifically state that corrections based on the FTS comparison are applied for both MOPITT and AIRS.

- Page 24888, line 8. See earlier discussion about accounting for the different averaging kernels of MOPITT and AIRS, this could be accounted for by the method described in Luo et al. [2007]. Also, how could the different vertical sensitivities be the cause of the discrepancies? Is this because most CO in these regions is located in the lowest troposphere, where the largest differences between AIRS and MOPITT may occur due to somewhat different vertical sensitivities? Please clarify.

- Page 24890, second paragraph. This effect, i.e. the AIRS bias for low CO columns which is absent in MOPITT, could be simply tested by performing some retrievals on

artificial spectra that either AIRS or MOPITT would measure, which should be relatively straightforward as it is also stated that some tests were already performed.

- Page 24904, figure 4. Discussion about the trend. I wonder whether or not a linear trend is the best fit to the data. It could be argued that the deviation from ground truth in MOPITT behaves as a step function (before and after 2003).

Furthermore, it also appears that the AIRS deviation also shows a trend, contrary to what the authors claim, at least in the NH (SH is less clear for AIRS, but as explained, there are issues with AIRS CO measurements in clean SH air masses with low CO content). The 12-month sliding average does appear to also show an increase similar to the MOPITT trend. This is rather important, as the standing hypothesis for the trend in MOPITT is gas-cell leakage but a similar trend for a different instrument would suggest something real rather than an instrument-related feature. Please clarify.

- Page 24905, figure 5. It appears that the wrong MOPITT bias for the MOPITT-MOZAIC comparison is taken from Emmons et al. [2008] for the year 2002. According to table 2 in Emmons et al. [2008] the mean column bias for MOZAIC for 2002 should be -6.7%. However, the number plotted in this figure looks like +6.7%. A negative number would actually result in a better agreement for the 5–year trend for MOZAIC and NOAA trends.

- Page 24906, figure 6. This figure shows the burdens for several latitude regions before and after correction. In the paper (page 24885, line 11) it is stated that AIRS and MOPITT show a better agreement after the corrections have been applied. This statement/finding could be quantified in terms of trends, biases and correlations before and after the corrections for all four latitude regions, and could be presented in a small table.

Typos, grammar.

Page 24887, lines 5-6. Change sentence to “Interannual variations of this source are minor due to small (<5%) changes in concentrations of . . .”

Page 24887, line 7-8. Change “Contributions . . . regionally” to “The Global Fire Emission Database [GFED; van der Werf et al., 2006] provides satellite estimates of monthly geographical CO emission from wild fires for the years 1997-2008.”

Page 24887, line 10-11. Change to “ . . . 25% of the total source.”

Page 24887, line 10. Change to “. . . large variability of CO emissions from wild fires is . . .”

Page 24887, line 23-24. Change to “. . . have been investigated previously (. . .)

Page 24887, line 26. Change “inverse modeling” to “inverse model calculations”

Page 24887, line 26-27. Between brackets, change to “(Turquety et al., 2008 and references therein).

Page 24878, line 3. “columns”

Page 24878, line 8. Add reference to Kopacz et al. [2009]

Page 24878, line 9-10. Change to “. . . interannual variations. Yurganov et al. [2008] demonstrated . . .”

Page 24878, line 15. “... these estimates . . .” Clarify which estimates are referred to.

Page 24878, line 15. Change to “CO emissions”

Page 24878, line 24. Double semicolon “;;” after Clerbaux reference.

Page 24878, line 18-19. Number disagreement (two plus four FTIR stations). On page 24880 (line 25) it is noted that CO TC measurements at seven observatories are used, not six. Six of them have provided data to the NDACC database, the seventh is Zvenigorod.

Page 24882, line 12. Change to “For a DOF approaching zero of a TCQP approachting

100% the . . .”

Page 24882, line 16. Change to “In the NH, the CO TC is generally higher . . .”

Page 24882, line 18-20. Why do AIRS-FTS differences for Ny-Alesund not approach the a priori for low information content?

Page 24883, line 3-4. Please provide some additional motivation/justification for chosing data with TCQP between 0 and 60% (MOPITT) and DOF > 0.7 (AIRS).

Page 24883, line 14-16. Change to “ . . . with differences between the FTS and satellite measurements of 60-80%.”

Page 24883, line 16. Change to “Meanwhile, the CO TC measured by the ground-based was larger than . . .”

Page 24883, line 20. Change to “. . . CO TC biases are illustrated . . .”

Page 24883, line 21. It is noted that all the points . . . are inside the +/- 10% corridor. This is technically incorrect as some points for SH MOPITT (month 7, 8 and especially 10) and NH AIRS (month 2) fall outside the 10% corridor. Better would be to state that “Nearly all the points – except for SH AIRS - . . .”

Page 24883, line 21-23. It is stated that MOPITT NH/SH and AIRS NH biases relative to the ground truth behave almost the same way. I tend to disagree, they behave in a similar way (positive change in base for months 7-12 compared to 1-6), but to behave “almost” the same way I would expect a very close correspondence, which is not the case. I would recommend to remove the “almost”, as it is anyhow explained what is meant with the similarity between these time series in the next sentence.

Page 24884, line 3-4. Change to “. . . but depend linearly . . .”

Page 24884, lines 5-7. Rephrase: “Conversely, AIRS data (right graph) are overestimating CO for small CO columns (i.e. during austral summer) and underestimating CO for large CO columns at night.

Page 24884, line 16. A trend/slope is calculated. What type of fitting is applied (ordinary linear regression?).

Page 24884, line 16. Change “is obvious” to “is present” or “is visible”

Page 24884, line 20. Delete space in “12- month”

Page 24886, line 14-16. Change to “. . . measured by AIRS and MOPITT whose measurements were corrected based on validation with ground-based spectrometers.”

Page 24886, line 19. Change “highlighting” to “highlights”

Page 24886, line 20. Delete “a” in “. . . are in a reasonable . . .”

Page 24886, line 23. Remove “only”

Page 24886, line 25. Delete the question “What is the cause of this effect?”, as the next paragraph immediately addresses the observed diminution of CO in 2008.

Page 24887, line 2. Remove the brackets around “(OH)”

Page 24887, line 5. Remove the brackets around “[OH]”

Page 24887, line 14. Remove “As a matter of fact,”

Page 24887, line 16-17. Put “bottom panel of Fig. 8” between brackets ()

Page 24887, line 17-18. Remove the brackets around “(and sinks)”

Page 24888, line 2. Change to “The CO burden . . .”

Page 24888, line 3. Change to “compared to”

Page 24888, line 22. Change to “The CO TC . . .”

Page 24888, lines 26-27. Change to “However, large CO TC’s were observed . . .”

Page 24888, line 29. Delete the period after “2008.)”

Page 24889, lines 3-5. Change to “shows that Indonesia and South America are the regions . . . . . .decrease, which is confirmed by the GFED2 emissions.”

Page 24889, section 5.1, lines 8-17. This paragraph appears to be redundant and could be deleted.

Page 24889, line 26. Change “shape” to “dependence”

Page 24890, line 1. Change “shape” to “dependency”

Page 24890, lines 14. Change to “At low total column values the absorption lines become . . .”

Page 24891, line 4-5. Change to “. . . data sets show that global and NH CO burdens are 10% lower . . .”.

Page 24891, line 27. Change to “ . . . would be the leading causes.”

Page 24899, table 2 caption, line 4: Change to “ . . . due to the long solar day.”

Page 24900, caption figure 1a. Explain that what is shown here is satellites minus FTIR. It is not immediately clear from the figure caption and thus a bit confusing.

Page 24908, caption figure 8, line 4. Change to “. . . assuming the reaction with . . .”

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24875, 2009.