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Comment

Interactive comment on “Source attribution and interannual variability of Arctic pollution in spring constrained by aircraft (ARCTAS, ARCPAC) and satellite (AIRS) observations of carbon monoxide” by J. A. Fisher et al.

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We thank the reviewer for the thoughtful and useful comments. Our responses to the comments are provided below, with the reviewer’s comments italicized.

As a key point (figuring both in abstract and conclusion), the authors highlight that “Synoptic pollution influences in the Arctic free troposphere include contributions of comparable magnitude from Russian biomass burning and from North American, European, and Asian anthropogenic sources.” (line 11 to 15 in abstract and 24 to 27 in

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Discussion Paper



conclusions). However, in my opinion, that is not supported by figure 8. Either the color scale is not appropriate (indicating a major contribution from European and Asian anthropogenic sources) or the term “comparable magnitude” is too vague to fairly describe the situation. The materialization of the Arctic Circle on the maps presented would help the reader.

The “synoptic pollution influences” referenced were meant to correspond to the CO variability shown as the horizontal bars in Figure 7, not the mean CO concentrations shown in Figure 8. We agree that this was confusing terminology and have re-phrased this result in the abstract, text, and conclusions. That sentence of the abstract has been replaced with:

“Russian biomass burning makes little contribution to mean CO (reflecting the long CO lifetime) but makes a large contribution to CO variability in the form of combustion plumes. ”

We have also added the Arctic Circle to the maps in Figure 8.

Line 16 “AIRS is capable of observing pollution. . . ” please mention “qualitatively capable”

We disagree. Figure 9 shows quantitative capability.

Based on the interannual variability deduced from AIRS, the authors suggest that in El Nino conditions the impact of Asian pollution may be particularly large. Would it be possible to assess it based on the a posteriori emissions + 2003 meteorology (or 2003 to a greater extent 1997-1998).

We have added a sensitivity simulation to the end of Section 5 at the request of the reviewer. Unfortunately, GEOS-5 meteorological fields are not available for years prior to 2004; therefore to retain consistency in the simulations, our sensitivity simulation used 2005 meteorology with a posteriori 2008 emissions. Results support the ENSO link.

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5th paragraph (“CO is emitted. . . ”): for such a general paragraph please try to extend literature references to studies from other groups (in particular non American teams, e.g. Turquety et al. ACP 2008, Yashiro et al. JGR 2009, etc.)

We have added these references.

In my opinion, the second paragraph (more precisely the text between “We use a linear CO simulation . . . with the overall CO simulation”) should be after the one describing the additional sources of CO (currently the fourth paragraph).

We have moved this paragraph as suggested.

Since the indirect CO emissions due to the NMHC oxidation is considered by increasing direct emissions, does it mean that the total emitted per regions (e.g. in the abstract and Table 2) include these indirect emissions? Please clarify.

We now state explicitly:

“These indirect emissions are not included in the regional CO emission totals given later in the paper.”

Fig. 3 and 4, the authors should remind that the optimized emissions are the ones deduced with the ARCTAS data.

This is no longer necessary in light of the next comment and associated edit.

Last sentence of page 19045, the authors state that: “The downward correction to North American emissions implied by the ARCPAC data does not seem robust in view of the limited influence of the North American source in the Alaskan Arctic. ” Either the methodology is suited to inverse emissions and the North America DOES influence the Alaskan Arctic or the methodology is biased to optimize the global emissions per regions and should not be used. Such an a-posteriori elimination of the incoherent results is not satisfying. Furthermore, at the end of page 19046, the authors choose to reject the results deduced using the ARCPAC observations due to the deliberated sampling of biomass burning plumes by this aircraft and the limited spatial coverage.

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Interactive Discussion

Discussion Paper



These arguments should have been balanced before doing this inversion. As it more discredits the methodology rather than really supports the work, this inversion could be removed from the paper. This rejection is also supported by the correlation which is not affected by the inversion (p 19047). If there was no problem of representativity of the sampling of ARCPAC (considering that the aim of this study is to invert the main global CO sources per regions), could the two datasets be merged to be used simultaneously in a single inversion?

We agree with the reviewer that a single inversion using both datasets is a more robust method to assess the source contributions. We have re-done the inversion using the combined ARCTAS and ARCPAC dataset and have changed the text, tables, and figures accordingly.

Line 15: “the five dominant sources”, could you please indicate (maybe only graphically), how much do these 5 tracers represent in term of CO concentrations with regard to the total CO signal.

We have changed the sentence referred to by the reviewer from:

“The median profiles of the five dominant sources along the flight tracks are shown in Fig. 7.” to:

“Figure 7 shows the median profiles along the flight tracks of the five dominant sources, which on average account for 67% of total CO during the campaigns.”

I do not understand why the synoptic pollution influences are better measured by the variability. Please clarify this point.

As mentioned previously, we have changed the text to instead discuss the CO variability. It now reads:

“Conversely, Russian biomass burning makes a large contribution to CO variability (horizontal bars in Fig. 7).”

Figure 9: it is really difficult to distinguish anything on the back-trajectories. Back and

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forward trajectories should be on a separated figure. Forward trajectories are almost not discussed. The text discussing them could remain without the illustration.

We have made a separate figure for each of the case studies showing the backward and forward trajectories (now Figs. 10 and 12). We have retained the forward trajectories to illustrate that the plumes studied do influence the Arctic (pg. 19051, lines 15-17 and lines 26-27).

P 19053: The link between ENSO and CO interannual variability was also explored by Szopa et al. GRL 2007, please do a link with this study.

We have added the following:

“Such an effect may be further amplified by increased biomass burning, which has been shown to play a dominant role in increasing CO concentrations over Alaska during El Niño events (Szopa et al., 2007).”

P19054: The authors state that the meteorological conditions have important implications and that, considering the same optimized emissions, the Asian anthropogenic source would have a larger influence in El-Nino conditions. Would it be possible to quantify this by doing a simulations with the meteorological fields from another year as a sensitivity study? More generally, why is the interannual variability only investigated using the satellite data and not the model (even considering the 2008 biomass burning emissions)?

We have added a sensitivity study to the end of this section using 2008 emissions and 2005 meteorology. We have added a paragraph at the end of section 5 detailing the sensitivity study and results as well as an additional figure (Figure 16).

P19055 line 2, please replace “2008” by “April 2008”. The authors should insist or at least remind that it does not necessarily point out a problem in the global annual emissions but more probably on the seasonality of such emissions.

We have clarified that the “2008” mentioned refers to the updated emission inventory

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Interactive Discussion

Discussion Paper



for Asia and have highlighted that our results are for April only. The text now reads:
“Least squares regression of the GEOS-Chem CO simulation to the ARCTAS and AR-CPAC aircraft observations suggests that, anthropogenic CO emissions in Europe in April 2008 are underestimated by 50

P 19055 line 24 to 27: I do not understand on which part of the paper it is based.

We have removed the reference to synoptic pollution influences and now simply discuss the CO variability:

“Russian biomass burning makes little contribution to mean CO but contributes substantially to CO variability.”

Fortems-Cheiney et al. is now published in ACP.

We have updated the reference accordingly.

Shindell 2006b: the list of authors is incomplete.

We have fixed the author list.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19035, 2009.

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