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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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The manuscript uses a combination of aircraft observations of CO from the NASA ARCTAS and NOAA ARCPAC campaigns, the AIRS CO retrievals and CO simulated with the GEOS-Chem chemitry transport model for April 2008 to improve CO emission estimate and to examine the sources and transport of pollution in the spring Arctic. In addition, Spring 2008 is analyzed in the context of previous years (2003-2008) to understand the interannual variability of long-range transport into the Arctic and its link with climate indices, including NAO and Ocean Nino index.

The manuscript presents new aircraft observations and explores the possibility of using

satellite retrieval, which remains an active yet challenging topic, to help understand transport of pollution into the Arctic. It is suited for publication in ACP after the following concerns are addressed.

Major comments:

1. Uncertainty in the optimized emission estimates. While aircraft-based corrections (Table 2) from ARCTAS and ARCPAC qualitatively agree with each other, quantitatively they differ quite significantly. This implies that the corrections are sensitive to the available measurements and are campaign-dependent. I am particularly baffled by the ARCPAC panel in fig 4. While the ARCTAS-based optimized results moves the mean vertical profiles of CO closer to the observations below 4 km, the two peaks ($\sim$1km and $\sim$5km) which are associated with enhanced Russian biomass burning plumes (Figure 7) completely disappeared. This clearly suggests two things: i) the lower-level bias is possibly corrected for the wrong reason, and ii) a 32% downward correction is too much and as a result the Russian biomass burning contribution are too small. In addition to derive correction factors based on ARCTAS only or ARCPAC only, have the authors try the two combined? I would think by increasing the data size, it helps to reduce the uncertainty.

2. The validity of using AIRS to understand transport of pollution to the Arctic and the associated interannual variability. I feel the discussion on this is rather weak, possibly incomplete. As pointed out by the authors that the sensitivity of AIRS CO is generally weak in the boundary layer. The 9 April 2008 transport event which occurred below 2 km is a typical example of this inadequacy. Therefore, analysis based on AIRS CO will fail to detect long-range transport of pollution, in particular European pollution and Russian biomass burning, that occur in the lower troposphere, which is of great, if not the greatest, importance in understanding Arctic composition and transport. As a result, it is not surprising to find that AIRS CO column show little correlation with NAO, which was shown to be the driving dynamic forcing of pollution transport to the Arctic (Eckhardt et al., 2003; Duncan and Bey, 2004). Is it possible to extend the model sim-

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ulation with the regional tagged tracers from a single year to 2003-2008? Whether this manuscript is written as a modeling paper focusing on understanding the interannual variability of pollution transport to the Arctic or a paper trying to test the ability of AIRS CO to observe pollution transport to the Arctic, using additional information provided by a multiyear model simulation will certainly help for an in-depth understanding of both the interannual variability and the limitation of AIRS CO. I understand this might be a lot to ask for at this stage of the publication. If this is not possible, I would encouraged the authors to expand section 5 to a more in-depth discussion on the validation and limitation of AIRS CO in the polar region, as pointed out by M. Deeter, and how this will possibly affect the detection of the interannual variability of pollution transport to the Arctic using AIRS CO.

Minor comments:

Page 19405, line 10-15. I am not clear about how you determine the correction factors. Do you regress the tagged CO tracers onto model total CO first, and onto observed CO second, and the ratio of the two slopes is the correction factor? Please clarify.

Page 19043, line 22-26. This sentence probably fits better in the last paragraph of section 2 (page 19043).

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