

***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 #1**

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**General comments:**

This manuscript by Fisher et al. examines the transport of CO pollution to the Arctic by using the global model GEOS-CHEM and the aircraft measurements of CO performed in April 2008 in the framework of the NASA ARCTAS and NOAA ARCPAC campaigns. These tools and data allow the authors to optimize the emissions of CO per regions and type of sources and thus to discriminate the role of the major sources of CO in the Arctic pollution. After this optimization of the CO emissions CO, the CO column retrieved from the AIRS satellite are qualitatively validated (by comparison with the

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CO simulated by GEOS-CHEM) and then used to discuss the interannual variability of the Arctic CO, of its sources and transport pathways. Their links with NAO and ENSO index are also investigated.

The material is of interest and is suited for publication once the concerns I have (see below) are addressed. The paper is very well-written and relies on a valid approach of the problem. The exploitation of different types of observations combined with modelling provides new insights into the Arctic pollution transport pathways. The way the paper is constructed provides an interesting story and gives an up-to-date picture of the possibility of global models when used in synergy with observations.

### **Abstract**

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 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.

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

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).

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## Introduction

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.)

## 2. Model Description

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).

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.

## 3. CO observations and constraints on sources

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

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. 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

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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?

#### 4. Sources of Arctic pollution in April 2008

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.

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

#### 5. Variability of Arctic pollution observed by AIRS

Figure 9: it is really difficult to distinguish anything on the back-trajectories. Back and forward trajectories should be on a separated figure. Forward trajectories are almost not discussed. The text discussing them could remain without the illustration.

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.

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)?

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## Conclusion

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.

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

## References:

Fortems-Cheiney et al. is now published in ACP.  
Shindell 2006b: the list of authors is incomplete.

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

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