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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reactive nitrogen, ozone and ozone production in the Arctic troposphere and the impact of stratosphere-troposphere exchange" *by* Q. Liang et al.

Anonymous Referee #1

Received and published: 1 June 2011

This paper shows a nice classification of source contributions to the ARCTAS observations based purely on the observations using tracer correlations. Airmasses of primarily anthropogenic, biomass burning, stratosphere and strat-trop exchange origins are identified and characterized. I believe this sort of analysis has not been previously published for ARCTAS, and is well worth publishing.

However, the other sections of the paper do not relate well to this, or each other. Section 3 shows a comparison of the CO observations to a CO-only simulation with GEOS-5, along with "tagged" CO to show source contributions. Section 5 discusses reactive nitrogen and its sources and budget, while section 6 discusses ozone production rates

as determined from a box model. These topics are certainly all inter-related in the study of understanding ozone distributions in the Arctic troposphere, but this paper does not show very well these connections. My specific concerns about Section 5 are listed below and I am not sure they can be addressed without more comprehensive modeling. Finally, the Conclusions include comparisons to TOPSE and ABLE-3 measurements; I think this discussion could warrant its own section prior to the Conclusions. Otherwise, I think it would fit better in Section 4 where the ARCTAS average concentrations are discussed. It seems like new material suddenly appearing in the Conclusions in the current version.

I feel a fairly significant revision of this paper is required to make the sections connected for a more coherent and focussed paper, which would then be acceptable for publication.

Specific comments

Section 3. The under-estimation of the modeled CO is attributed primarily to OH values being too high. Why are not an underestimation of sources considered as a possible cause? Also, the factors accounting for secondary production from HCs could be too low.

Fig 3 & 4. It would be nice to have more of a justification given for uniformly adding +25 ppb to the model CO. Is this adjustment used only for visually comparing the distributions, or are the modified values used elsewhere?

p.10731, l.5-22: Give more justification for looking at C2H2/CO, and/or at the end of the paragraph explain how this ratio will be used later in the paper. I think including the Supplement figures here instead of Fig 5 would be more appropriate. Does C2H2/CO really show you something more than CO in Fig. 6? If so, make that clearer. If not, it could be left out.

p.10731, l.20: I think the long lifetime of CO in the winter Arctic is equally, or more, the cause of the well-mixed troposphere than slow transport times.

p.10737, l.3: You state "we calculate the extent of mixing . . .". Say more about how that calculation is done and how you reach the conclusions in the following sentence.

p.10738: Much more needs to be shown and explained to convince me that stratospheric HNO3 is a significant source of NOx in the upper troposphere. In addition a 1983 reference is not sufficient to convince me that C2H6 -> CH3CHO -> PAN is the main source of PAN in the UT. This whole paragraph is just too vague and lacks the illustration of evidence for your conclusions.

Minor Comments

Table 2: for STE, do you mean 80<CO<160 and 50<CO<120 ? (missing first "<")

p. 10740, below (R8) in 2 places, should be "production and loss rates" (not "product").

A number of minor grammar errors (singular/plural, missing articles, etc.) that I have not bothered to note.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10721, 2011.