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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 #2

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This paper uses aircraft observations of tropospheric trace gases in the Arctic springtime and summertime troposphere to determine relative influence of different air mass origins on Arctic atmospheric composition, and the sources of NOy/NOx responsible for in-situ ozone production in the Arctic. The paper is well written and is a thorough observational study, providing a valuable addition to the literature in this area. I recommend the paper for publication in ACP. However, there are a number of concerns that require attention before publication, which may require not insignificant modifications to the manuscript.

My major comment concerns the inclusion of global CO simulations from the GEOS-5

ADAS model. It is not clear to me how these comparisons relate to the main aim of the paper. The findings from these comparisons are not discussed at all in the conclusions, and as I see it, add nothing to the weight of the main conclusions of the paper (NOy budget, ozone production rates). The conclusions regarding dominance of concentrated plumes in summer versus homogeneity in spring, and therefore representativeness of the flights can be made from the observations alone. The paper could be shortened, and its purpose made clearer to the reader by omitting these simulations and their discussion. The remainder of the analysis is far more coherent and I think is the more valuable contribution of this paper.

If it is deemed necessary to retain this part of the analysis and discussion, the following should be addressed: The discussion of idealised CO should be better integrated with the rest of the analysis. How can the comparison with idealised global model CO be used to inform conclusions regarding the NOy budget and ozone production? Far more important in this respect are the box model simulations used. What is the justification for isolating model OH fields as the cause of the CO bias in both spring and summer? This can only be adequately addressed with full chemistry simulations and comparison with e.g. HOx precursors, which seems beyond the scope of this paper. It also seems surprising to me that the coarse grid-size model CO simulation appears to shown similar variability over the flight domain as the observations (Fig. 3). This point should be considered. This is also relevant to the point made regarding the ability of the model to represent mixing of plumes with background air (Page 10730). Is it reasonable to assert that a global model would be able to represent this process realistically? Lack of lower CO peak in model CO is likely due to too diffuse exchange across tropopause in the model and inability to resolve STE filaments (Page 10729), rather than general 'bias'?

Further comments

Page 10737 - Discussion of calculations of extent of mixing between stratospheric air and background based on tracer relationships. Please add a brief outline of what was done to determine that the change in NOy partitioning can be given by mixing only.

Page 10738 - Discussion of PAN source in STE air masses. The authors state that approximately 120 pptv of PAN is unaccounted for by mixing processes only, and suggest that this could be produced as stratospheric and tropospheric air mix. However, the source of peroxyacetyl radicals for this is not clear to me. The authors suggest production from acetaldehyde. What is the evidence for this? Can DC8 CH3CHO (or precursor) observations be used to examine this hypothesis? This outcome is one of the main conclusions of the paper, and is of high significance to the community. I feel that the authors need to do a more convincing job of supporting this conclusion.

Page 10740 - Box model analysis, Equation R9. Please briefly provide more information to the reader on which terms in these model calculations are constrained and with what.

Page 10741 - P(O3) rates in STE air masses. It would be interesting to know how the balance between O(1D) + H2O loss and HO2 + NO production is balanced in these air masses as they mix with moist tropospheric air. Can the relevant terms be plotted from Equation R9?

Minor Comments

Abstract, line 4: "the GEOS-5 CO simulation" change to "a GEOS-5 global model CO simulation"

Abstract, line 9:"with CO decreases" change to "with CO decreasing"

Page 10728, line 27: "well captures" change to "captures well"

Page 10730, line 24: "define" change to "defined"

Page 10740, paragraph 1: Change two instances of "product" to "production".

Table 1 caption: Correct text "used innthis study"

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11, C4673–C4676, 2011

Interactive Comment

Table 2, footnote 'b'. Change "scattering" to "scatter"

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