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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An analysis of fast photochemistry over high northern latitudes during spring and summer using in-situ observations from ARCTAS and TOPSE" *by* J. R. Olson et al.

Anonymous Referee #2

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This paper presents an analysis and modeling of measurements of OH and HO2 made during the ARCTAS-A and ARCTAS-B campaigns in 2008, and the TOPSE campaign in 2000. The authors find that their modeled OH concentrations are in good agreement with the measurements from ARCTAS-A, although the measurements are near the limit of detection of the instrument and are the same magnitude as the uncertainty in the model. For ARCTAS-B, the modeled OH concentrations are lower than the measurements, and are highly sensitive to the uncertainty associated with the NO measurements. The predicted HO2 concentrations are a factor of two greater than

the measured concentrations for both campaigns when the model is constrained to the observed concentration of H2O2 and CH2O, but in better agreement when the model is unconstrained by the measured radical precursors suggesting that there is an inconsistency with the measurements of HOx and these radical precursors.

The paper is well written and the suitable for publication in ACP after the authors have addressed the following minor comments.

1) It has recently been reported that ground based OH measurements by the Penn State LIF instrument may be subject to an unknown interference related to the presence of ozone and alkenes (Mao et al., ACPD, 12, 6715–6744, 2012). Although the conditions encountered during ARCTAS and TOPSE are likely quite different than the conditions where the interference is observed, the authors should provide a discussion on whether the measurements reported here are subject to any interference.

2) Similarly, it has recently been found that HO2 measurements using chemical conversion to OH through the addition of NO may also be subject to interference from alkene-based peroxy radicals (Fuchs et al., Atmos. Meas. Tech., 4, 1209–1225, 2011). The authors should comment on whether the ATHOS instrument is subject to this interference and whether the measurements have been corrected.

3) As discussed in the manuscript, previous modeling of the ARCTAS HOx measurements found that the measured concentrations of HOx radicals could be reproduced by including heterogeneous loss of HO2 onto aerosols (Mao et al., Atmos. Chem. Phys., 10, 5823–5838, 2010). However, including a similar loss process could not account for the model/measurement discrepancy in this manuscript. Unfortunately there is little discussion in the manuscript concerning this discrepancy. The paper would benefit from an expanded discussion regarding potential reasons for the different results from these two models. If aerosol loss does not explain the model/measurement discrepancy, can the authors suggest other possible reasons?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9377, 2012.