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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric nitric oxide and ozone at the WAIS Divide deep coring site: a discussion of local sources and transport in West Antarctica" *by* S. Masclin et al.

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This paper provides a new dataset to further investigate ozone, NOx, HOx cycling in the polar regions. Given that this is a topic of great interest for those aimed at understanding atmospheric chemistry and snow-air cycling in both the Antarctic and Arctic, I recommend this paper be published in ACP after addressing the reviewers comments.

In general, the detailed 1D modeling for this dataset (similar to that presented in Thomas et al. (2011) and Thomas et al. (2012)) would be a good addition to our

scientific understanding of these measurements, but goes beyond the scope of this present study.

Major comments:

I echo the second reviewers concerns about the simplified model framework used to estimate NOx fluxes. However, I think the approach still has some value even with the noted shortcomings. I recommend that the authors revise section 4.1.4 to clarify that this approach results in NO2 production – which represents the maximum NO2 released to the interstitial air. Then the authors assume all of this is mixed from the interstitial air and evenly distributed in the atmospheric boundary layer. This should be presented as the maximum emission flux that can result from the conditions near the WAIS Divide drilling camp.

There is also a major difference between the WAIS Divide and Summit, as pointed out by the authors, in that at Summit, it has been established that there is low levels of bromine oxide (BrO) in the boundary layer that counteracts the effects of ozone production from NOx released from the snowpack. Prior 1D modeling work has focused on the combined role NOx and bromine released from the snowpack at Summit have on ozone. Given that it has not been established if there is low levels of halogens present inland within the boundary layer in the Antarctic, I believe this should be mentioned as a difference between ozone production at Summit and in the present study. It may also be added at as a caveat that if it is established that any halogen chemistry is occurring in the WAIS Divide snowpack, this may act to counteract ozone production from NOx release.

The value of 2 pptv for NO to trigger O3 production is very interesting and should be included in the final version of the paper, within the context of the previous comment.

More details of the NASA Goddard Flight Center (GSFC) model should be included. What is meant by “that included physical sources of H2O2 and CH2O”? What other species were constrained using measurements?

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13, C2163–C2165, 2013

I'm a bit confused about the discussion of HOx. The authors state: "The lifetime of NO2 was estimated with measurements of HOx at Halley by Bloss et al. (2007) ([OH]=3.9×10ˆ5 molecule/cmˆ3,[HO2]=0.76pptv)". Then the authors use these HOx values later in the paper to estimate ozone production. Why not take the estimated HO2 and OH from the box model run (I assume constrained to measurements of NO and ozone) to estimate the OH and HO2 and use these values later in the paper. There is not a good reason to assume the HOx measured at Halley can be used as an estimate of HOx at the WAIS divide.

Minor comments:

In places I find this manuscript a bit difficult to read. It may help to review the text for clarity during the review process.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6807, 2013.