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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement and interpretation of gas phase formaldehyde concentrations obtained during the CHABLIS campaign in coastal Antarctica" *by* R. A. Salmon et al.

R. A. Salmon et al.

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We thank the referee for his/her comments.

With regard to specific points, see below.

1. "The authors performed very thorough calibration and sampling efficiency tests after the field season, which is great. I wonder if there could be a temperature effect during the gas phase calibration (Method A), being carried out in England or Antarctica? Temperature gradients will be very different."

The wafer used to produce gas phase HCHO inside the instrument is temperature

controlled in order to ensure a consistent calibration gas, at the desired concentration. This parameter would therefore not vary whether in Antarctica or the UK.

2. "Page 2348, line 7: The rate constants vary a lot, why did you use 2 x 10E-12? Also this constant is for room temperature while the one cited by Enami et al. is given for 269K."

Only three measurements of the IO + CH3O2 reaction rate constant have been published in the literature, with a factor of 30 variation in the value reported. As these studies have not been evaluated by the JPL / IUPAC panels, we have to select a value to use for our analysis and prefer the measurements of Dillon et al. (2006). We comment upon the difference which using the Enami et al. (2006) values would make to the calculated steady-state HCHO in section 4.2 (p. 2349 in the ACPD manuscript). In terms of the different temperatures quoted, both Bale et al. (2005) and Dillon et al. only performed room temperature experiments while Enami et al. report a temperature-dependent results, so in the latter case the rate constant for a temperature more pertinent to the CHABLIS measurements can be quoted. We note that the temperature dependence of the rate constant, as reported by Enami et al., is not sufficient to explain the discrepancy between the studies.

refs: Bale, C. S. E., Canosa-Mas, C. E., Shallcross, D. E., and Wayne, R. P.: A discharge-flow study of the kinetics of the reactions of IO with CH3O2 and CF3O2, Phys. Chem. Chem. Phys., 7, 2164-2172, 2005.

Dillon, T. J., Tucceri, M. E., and Crowley, J. N.: Laser induced fluorescence studies of iodine oxide chemistry, Phys. Chem. Chem. Phys., 8, 5185-5198, 2006.

Enami, S., Yamanaka, T., Hashimoto, S., Kawasaki, M., Nakano, Y., and Ishiwata, T.: Kinetic Study of IO Radical with RO2 (R = CH3, C2H5, and CF3) Using Cavity Rind-Down Spectroscopy, J. Phys. Chem. A, 110, 9861-9866, 2006.

3. "In their calculations of HCHO fluxes from the snow they assume a boundary layer

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height of 100 m. Where does that value come from? If that is based on earlier acoustic radar measurements please state this."

The 100m value for boundary layer height is typical for Sodar measurements in previous years, and discussed in greater detail in the overview paper of this Special Issue. In our revisions, this is now referenced in the paper in the appropriate sentence mentioning boundary layer height.

4. "Table 1 gives potential HCHO production from VOCs, are the authors aware of the HCHO production potential as given in Wert el al., 2003, Signatures of terminal alkene oxidation in airborne formaldehyde measurements during the Texas Air Quality Study (TexAQS) 2000, J. Geophys. Res., 108(D3), 4104, doi:10.1029/2002JD002502?"

We note the reference raised by the reviewer (Wert et al., 2003), which reports analysis of HCHO and VOC observations in plumes above the Houston Ship Channel, including calculation of the "HCHO production potential"; from the different VOCs measured, using formaldehyde yields from the similar analysis of Lee et al. (1998). We have taken a more simple approach, in that rather than use the actual yield from a chemical mechanism, we simply apply the maximum possible ultimate HCHO yield, i.e. the number of carbon atoms in the parent VOC. This has the advantage of incorporating potential HCHO production from oxidation of all generations of daughter products of the parent VOC, but of course also fails to consider other possible oxidation products, so will tend to overestimate the HCHO production from NMHCs (as we state in the manuscript). In the specific case of the CHABLIS observations however, even considering this overestimate, CH4 accounts for 80 % of the total HCHO production, so the error introduced by our approach is small, and considering the large uncertainties in the halogen reactions found to be a major sink for HCHO, we did not feel that a more sophisticated analysis of the HCHO production potential was warranted. We have added a note to the manuscript to this effect, including the Wert et al. and Lee et al. references.

5. "The figures are hard to read, please could you increase font size in both figures?"

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The figures have been amended.

6. "Figure 2: I see that you want to show the variability of HCHO, but this figure is hard to read and has to be blown up a lot to be deciphered. Could you try to make is easier readable? Maybe remove lines between daily averages, or bring dark blue spots to the front, or present daily averages of the detection limit?"

Figure 2 has been amended and an additional table included listing mean monthly concentrations and variability.

7. Technical Corrections have been amended.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2337, 2008.