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Atmospheric Chemistry and Physics Discussions

Interactive comment on "OH in the coastal boundary layer of Crete during MINOS: Measurements and relationship with ozone photolysis" *by* H. Berresheim et al.

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1. The new value was adopted, the reference has been added, and new calculations were made (including Figure 5). The overall difference relative to the previous model results was -7% for [OH].

2. The wording has been changed to address all three radical self-reactions as one group (using the expression "dominant"). As an individual reaction, OH+HO2 may not be dominant but may certainly play an important role in NOx-poor environments. For example, for noontime conditions during MINOS we calculated that its contribution was 25% of the total contribution from all three reactions in this group.

3. The study cited by the referee was conducted in a forested region in the northern mountains of Greece. Therefore, in our opinion the corresponding OH and J(O1D)

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measurements cannot be compared with corresponding values measured over Crete. We have changed the wording in the text emphasizing the marine environment conditions under which the MINOS and PAUR campaigns took place.

4. Done.

5. The chemical correction mainly affects the calibration of the instrument which introduced a correction by 14% constantly applied to all measurements. In addition, on average 4 % reduction are due to chemical changes during ambient air measurements, resulting in a total factor of 0.82 +/- 4% (or +/- 0.03, 1-sigma). These corrections were not significantly different in the 3 periods discussed in this paper. We have added the 1-sigma uncertainty in the text.

6. This value of 8% is indeed realistic. The UV photon flux was measured using calibrated phototubes for which the calibration uncertainty was specified to be 4%. However, during MINOS a systematic uncertainty due to changing sensitivity of the phototubes of 8% ("calibration uncertainty") is estimated in our paper. The measurements of the electrical current from the phototubes was complicated by sea salt effects, owing to this an uncertainty of 8% was estimated which is higher than the typical 2% obtained from our measurements in a continental environment (Hohenpeissenberg). The flow velocity in the inlet region, which includes the illuminated zone during calibration measurements, is determined by a frequency-controlled blower. The rotational frequency is constant and independent of variations in the electrical power voltage. Since the blower controls the volume flow, this results in well-defined residence times of the sample air in the illuminated zone. Flow velocities were determined by independent techniques, e.g., pitot tube and heat anemometer measurements, which resulted in a determination of the flow velocity of 5 m/s with a systematic uncertainty of 8%. Additionally, cross winds of several m/s induce turbulence in the inlet of the instrument. This, however, may lead to enhanced or shortened residence times in the illuminated zone during calibration measurements. But such variations are expected to be randomly distributed around the average value and thus have no systematic effect on the calibration factor,

ACPD

3, S600–S603, 2003

Interactive Comment

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which was determined from the mean of all calibration measurements during MINOS.

7. Done.

8. We comment on these high values in section 3.2 stating that these were a few outliers probably caused by NO emissions from cars passing by the measurement site. This agrees with the same argument made in the companion paper by Salisbury et al. (2003).

9. See our comment to point 3 which also applies here.

10. No, the summit was east of the DWD site and since wind direction was always from the west it did not block the air flow to the DWD site. We added a sentence at the end of section 2.2 to clarify this point.

11. The OH loss due to reactions with NMHC was less than 10% according to the detailed box model by Baboukas et al. (2003). Therefore it has been neglected in our simple CH4-CO-box model. We have added this information in the text in section 3.2 on page 7 (bottom) where we introduce our box model.

12. The referee may have missed that for HCHO we adopted a measured value of 1 ppbv (average mixing ratio measured from the Falcon aircraft during fly-bys near Finokalia by Korrmann et al., 2003). This has clearly been pointed out in the text. With respect to our calculations of H2O2 and HONO mixing ratios we now have added to the text on page 8 the maximum values obtained from our model (1 ppbv and 5 pptv, respectively). With 1 ppbv H2O2 the H2O2 photolysis contribution to the HOx budget is negligible (3%). The contribution by HONO photolysis is also negligible assuming max. 5 pptv. However, it is also obvious from the description of our model that we only considered homogeneous gas phase formation of HONO by NO+OH reaction (as does the model by Baboukas et al., 2003). Should heterogeneous formation of HONO have been important during MINOS then corresponding mixing ratios may have been significantly higher. Based on our model the contribution by HONO photolysis to the

ACPD

3, S600–S603, 2003

Interactive Comment

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OH budget would have become significant at mixing ratios of 50 pptv or higher. At 50 pptv this process would enhance the HOx production by about 20%. However, since HONO was not measured during MINOS we avoid to further speculate on the HONO contribution.

13. This has been reworded on page 9 to make our approach more transparent and clear to the reader.

14. As suggested by the referee we have added some more information about the detailed box model study by Baboukas et al. (2003) and its agreement with the measured OH values in the Conclusions section.

15. This has been reworded.

16. On page 5, section 3.1, we have added that [H2O] and rH were highly correlated (R2=0.90), i.e., temperature was relatively constant. We decided to show rH values in Figure 2b (H2O values in Table 1) since it was much easier to scale with the other parameters shown in the Figure.

17. A more explicit description concerning the calculation and interpretation of Figure 5 has been added on page 8.

18. See our comment to point 8.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1183, 2003.

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