

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

Anonymous Referee #2

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Overall, I found the Berresheim et al. paper to be interesting and a useful addition to the current literature on OH measurements. The Mediterranean region is a particularly interesting place to carry out such a study owing to the high levels of photolysis and correspondingly high OH concentrations.

Specific comments: 1. In section 2.1, the authors describe how the CIMS measurements need to be corrected for ambient concentrations of various species such as NO_x, O₃ etc., using average mixing ratios for these species. Given that the ambient concentrations of these species varied quite significantly throughout the campaign (figure 2b), what magnitude of error will be introduced into the corrected measurements? In other words, over what range can these ambient measurements vary without affect-

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ing the final results?

2. In section 2.2. in a similar vein, how good is the assumption that $j(\text{O1D})$ from the ground is 10% of that measured upwardly, given that only a few tests were performed? What difference could this assumption make to the final results?

3. In section 3.1, the check carried out for local impacts of biogenic emissions is hardly conclusive. Are the authors sure that enough biogenic species were being emitted to really measure the effect on OH signals?

4. In section 3.2, the authors state that secondary HOX can be neglected. I doubt very much that this is true, particularly for regimes 2 and 3. Is there any way that the authors can account for secondary HOX to make their calculation of the parameters in (E3) more accurate?

5. Can the authors account for why in Table 1, the value of $j(\text{O1D})$ decreases between periods II and III, but that of $j(\text{NO2})$ increases?

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