

Interactive comment on “Technical Note Formal blind intercomparison of OH measurements: results from the international campaign HOxComp” by E. Schlosser et al.

Anonymous Referee #1

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This technical note describes the results of a formal blind intercomparison of several OH instruments. The results are new and interesting and provide a significant advancement in the field of OH measurement. The paper itself is thorough and well written and the results are presented clearly. Overall, this is one of the better papers that I have read recently and I was very pleased to see such success by the HOxComp participants. I have a few minor comments for the authors to consider, otherwise this paper should be published as is.

Pg 14093, line 28+. The potential interference from NO₃ is mentioned here, but not later in the paper. Why is it that the MPI instrument sees this interference, but none

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of the other LIF instruments do? Is there something unique about the MPI sampling geometry or detection geometry (e.g. multipass vs single pass), or is it that the higher precision of the MPI instrument enables the identification of this interference?

Pg 14099, lines 24+. I suggest a different wording for this paragraph. The correction may have been easy, but the systematic error was not trivial. It sounds similar in scope to the calibration error that the Penn State group found in the PMT that they used in their calibration source. Also, there might be something like this causing the calibration difference between the LIF and CIMS instruments.

There is no discussion at the end of the paper of the sampling and detection biases that could not be constrained by the intercomparison and still need to be explored. The two items above fall into this category, as do unresolved differences between the measurements that might be due to weather (bottom of page 14111) or chemical contaminants that are not measured. For example, could the CIMS be measuring low because of the polluted air in Julich? A paragraph or two discussing the limitations of the HOxComp study and some of the more important remaining issues would be helpful. In particular, I think many readers will be interested about what the HOxComp results say about these instruments' ability to measure nighttime OH accurately. As written, the paper indicates to me that there is a small interference at night and that these nighttime measurements need to be considered carefully.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14081, 2009.

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