

Interactive comment on “A flow-tube based laser-induced fluorescence instrument to measure OH reactivity in the troposphere” by T. Ingham et al.

Anonymous Referee #1

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This paper presents a detailed description of a new instrument designed to measure the total OH radical reactivity in the atmosphere. This is an important area of research, as measurements of total OH reactivity can provide constraints on models of atmospheric chemistry and can help to determine whether all there are missing sinks of this important radical not accounted for in current models. This is a difficult measurement that has only been attempted by a handful of research groups and this paper provides more details on the flow-tube LIF technique than described previously, including an expanded discussion of the quantification of the HO₂ + NO interference.

The paper is well written and appropriate for publication in AMT after the authors have

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addressed the following comments:

1) Although the technique described in this paper is similar to other flow tube techniques described previously, the paper would benefit from more details on some of the known differences between the techniques. For example, Kovacs and Brune (2001) appear to use flow conditions closer to the laminar regime, with Reynolds numbers less than 6000, while this technique tends to use turbulent flow conditions. The different flow regimes may contribute to the smaller $k_{physical}$ observed in this study. The authors touch on this issue on pages 639-640, but the paper would benefit from an expanded discussion of these and other differences.

2) On page 630, the authors state that the measured decays using humidified N₂ to produce OH in the injector were the same as using zero air. However, using zero air likely produces O₃ in the injector, which could interfere with the OH decays through the HO₂ + O₃ reaction or perhaps through the OH + O₃ reaction depending on the concentration of O₃ produced. The authors should provide more information on the amount of O₃ produced (which they measure in their flow characteristics studies) to show that these reactions are not interfering when using zero air in the injector.

3) On page 635, the authors describe the field measurements of OH decays under low NO conditions such that production of OH from HO₂ + NO is insignificant. What was the concentration of NO under these conditions? Similarly on page 646 when describing measurements during TORCH-2 under low NO conditions the concentration range of NO should be included.

4) The authors claim a systematic error of 5% in these measurements from the anemometer calibration. This is based on their measurements of the rate constant for the reactions of OH with CO and n-hexane which were within 5% of the recommended values. However, this does not appear to include uncertainties associated with the recommended rate constants, which are on the order of 10-20%. This should be clarified in the revised manuscript.

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5) It is not clear how the uncertainty associated with the determination of the HO₂ + NO correction is propagated to the overall uncertainty in the measured OH reactivity. How does this uncertainty increase as NO increases from 1, 5, 10 ppb?

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 621, 2009.

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