

Interactive comment on “Measurements of tropospheric HO₂ and RO₂ by oxygen dilution modulation and chemical ionization mass spectrometry” by R. S. Hornbrook et al.

Anonymous Referee #1

Received and published: 22 November 2010

The manuscript by Hornbrook and co-workers describes the development, test, and field application of a new method to measure HO₂ and RO₂. Since most of the manuscript (90%) deals with the development and tests of the method, I do not think that ACP/ACPD is the appropriate forum for this kind of research. Therefore, I strongly suggest that this "technical" paper is published in AMT/AMTD or another journal focussing on technical aspects or instrument developments. The paper itself provides information on many aspects of the method, especially the determination of conversion efficiency of RO₂ to HO₂ for different conditions and different precursor species.

In general, the paper is well written, but it can be shortened at several points (see sug-

C10116

gestions below). It represents a valuable, extensive set of work which is of importance for the community of scientists who perform radical measurements in the troposphere. Even though I think the paper should go to a different journal, I would like to see the following specific points to be addressed before further consideration.

- Abstract: The statement "PERCIMS is able under the majority of tropospheric conditions" is not justified in this manuscript.
- The first paragraph is more-or-less textbook knowledge and should be cited from a book, if necessary.
- The abstract should provide more information about other instruments measuring HO₂ and RO₂. Are there reviews of the current instruments available? If yes, they should be cited. If not, the author might add a few lines about other instruments which use the same kind of conversion, e.g. FAGE-LIF.
- The reason for the H₂ addition is the quenching of all OH to HO₂. That should be stated in line 23 page 22229. I also miss a number for the cleanliness of the used H₂.
- Section 4 on ambient measurements should be re-organized. The comparison of measurements with the model is the weakest part of this manuscript. For a technical paper like this manuscript the authors should select ONE day (i.e. 10-Mar-2006) as an example for a successful measurement using the newly proposed method. In my opinion the Figures 10a, 11, and 12 can merge into one figure with same time axis (19:30 – 22:00 might be sufficient). The discussion of the ambient measurement should then focus on the effect of the different RO₂ not on the agreement between a model and the measurements.
- In the discussion and conclusion the authors should explain how the detection of RO₂ in the HO₂ mode would influence their and other previously measured HO₂ data.

C10117

Technical corrections:

- The reactions R1 – R9 are part of Scheme 1 and should not be repeated in the text.
- The statements in lines 8-12 page 22223 need references.
- Eq. 3 can be deleted since it is repeated in Eq. 4.
- Figure 9 and Table 5 provide identical information. Please delete Figure 9.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22219, 2010.

C10118