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The authors describe analysis of a complex data set from a field campaign in the boreal forest in Finland from 2010. They use measurements of CH3C(O)OOH (PAA) and CH3C(O)O2NO2 (PAN, commonly peroxyacetyl nitrate) for a rough estimate on the HO2 radical concentration assuming steady-state behaviour for PAA and PAN within an assumed reaction scheme. For comparison, LIF_HO2 data were available in a limited time range. The agreement is reasonable with exception of episodes where HO2 measurements were strongly influenced by RO2 radicals. Results of a box modelling study support the derived HO2 levels and seem to be in excellent agreement with the measured OH radical concentrations. As a result of this study it can be concluded that the HOx chemistry is well understood.
This manuscript provides a couple of interesting things and meets the criteria for ACP. Some minor points should be considered before final acceptance is recommended:

1. Nothing is said regarding the uncertainty of the PAA and PAN measurements and the consequence for the derived HO2 concentrations. A statement on that is needed. Calculation of the consequential error in HO2 would be fine.
2. There is only very sparse information on the OH measurements, I think it was done by nitrate CIMS with a UHEL instrument. What has been done to "adjust" the ground level OH measurements to canopy level. Please explain. Original data compared with those after adjustment, as shown in fig.7, should be given in SI.
3. Expected RO2* radicals from OH+OVOC/terpenes show concentrations of up to 10(9) cm(-3) and surpass CH3O2 concentrations significantly. That´s a bit surprising for me. Are similar RO2* concentrations known from other forest sites?
4. There are a couple of typos, careful proof-reading is needed.

This manuscript describes an extensive set of measurements made in the boreal forest in 2010. The data set includes photochemically active molecules, in addition to OH and HO2 radicals, so that a detailed balance of radical sources and sinks can be assessed. Modifications to a standard model are discussed stepwise, so the reader can follow the logic involved in building the model.
The final agreement between measurements and model is good, with the exception that the HO2 measurements are impacted by other, BVOC-derived peroxy radicals, RO2*, that are partially detected by the instrument. Also, additional photolytic sources C1
of radicals need to be invoked, e.g., glyoxal, methyl glyoxal and biacetyl, to account for the radical production rates.
Overall, this is a good paper, which is clearly written and explained, and provides a good data set for future comparisons. It can be published after consideration of some minor comments, below. Additionally, there are quite a few grammatical errors (mis-match between subject and verb; misuse of commas) that could be cleaned up by the authors.