General comments

The paper reports airborne observations of OH and HO_2 that were measured together with other trace gases and atmospheric parameters up to an altitude of 12 km during the DC3 mission over the central United States. The measured radical concentrations are compared to model calculations in order to test how well a photochemical box model can explain HOx in air close to deep convective clouds. In general, there is good agreement found between model results and measurements. Within the combined uncertainties, no significant difference is seen without and with heterogeneous chemistry using uptake coefficients for cloud particles consistent with laboratory findings. The data and the conclusions are convincing. The paper is well written and thematically suitable for publication in ACP. Before publication, the following comments should be addressed.

Major comments

1. Little if any information is provided on which organic compounds were measured during the flights. Were there other VOCs besides methane that made a significant contribution to the OH reactivity? Did isoprene play a role? This question is especially relevant in the outflow of convective clouds which can transport relatively short-lived species to higher altitudes in short time.

2. In previous publications about airborne HOx studies, in which some of the authors of this paper were involved, systematic NO-dependent deviations between measured and modelled HO_2 were reported (e.g., Ren et al., JGR 113, D05310, 2008; Olson et al., JGR 111, D10301, 2006). In the present study, no such deviations are found (Fig. S3). Can the authors comment on this different findings?

3. Besides their possible role in heterogeneous chemistry, cloud particles can influence HOx through their impact on solar UV radiation that is driving photolysis. Though this may not be in the focus of the present paper, have the authors tried to quantify the impact of clouds on j values influencing HOx? It could be interesting to see a with-cloud / without-cloud (clear sky) ratio of HOx as a function of solar zenith angle extracted (if possible) from the data set.

Minor comments

page 2, line19 - 20: the production of RO_2 does not require NO; please correct.

page 2, line 22: add a half sentence, why the production of NO and $O(^3P)$ produces new ozone.

page 2, line 23: add a short explanation, why in the absence of NO the formation of OH and HO_2 destroys ozone.

page 3, line17 and page 9, line 31: : typos; Kubistin.

page 6, lines 28 -32: how much is the limit of detection for OH and HO_2 affected by scattered light from cloud particles?

page 6, lines 23-25: the principle of the k_{OH} measurement should be briefly explained in one or two sentences before equation (1) is presented. Alternatively, move equation (1) to page 7 between line 5 and 6.

page 7, line 23: the sentence is not clear. How large was the temperature difference between ambient air and air inside the flow tube?

page 8, line 6: The table showing the measurements is missing. It should be presented in the main paper. Accuracies and time resolution of the measurements should be given.

page 11, equation (3): how was k'OH calculated? Does it include VOCs (RH --> RO2 --> HO2), or only those species (e.g., CO, O_3, HCHO) that directly yield HO_2?

page 14, line 15: Fig. S8 ? References made in the main text to Figures S8, S9 and S10 should be checked and corrected.