Review of '*An analysis of fast photochemistry over high northern latitudes during spring and summer using in-situ observations from ARCTAS and TOPSE*' by Olson et al.

The manuscript provides a thorough and detailed study of measurements and model calculations made during the ARCTAS aircraft campaign. The methods and tools used in this work are appropriate and clearly described. The topic is certainly within the scope of ACP, and is a significant contribution to the field. I recommend publication in ACP after the comments listed below have been addressed.

Minor comments

Page 9383, line 8: Please provide a brief description of the diurnal steady state approach.

Page 9384, line 14: Please state the tolerance used in this work.

Page 9386, lines 1-4: Please provide further details regarding the TOGA dataset and the correlations of available data with CO/acetone/methanol.

Page 9392, line 13: Please correct to 'ozonolysis'.

Page 9393, lines 18-26: In the description of the model-measurement comparisons for OH and HO_2 it would be useful to provide a point by point comparison (i.e. modelled mixing ratio vs observed mixing ratio for all points) in addition to the plots shown in Figure 8.

Page 9394, lines 4-6 and lines 18-20: How are the R-Obs/Calc values impacted by the inclusion of points at or below the LOD which are given R-Obs/Calc = 1? Perhaps the points below the LOD should not be included in R-Obs/Calc, it is not clear how much the R-Obs/Calc below 0.04 pptv is skewed by the inclusion of points below the LOD.

Page 9394, line 19 and Figure 8c: 'ppt' is used instead of 'pptv'. Please check that there are not other instances of this in the manuscript.

Page 9395, lines 14-18: There has been some debate regarding the atmospheric relevance of the reaction of electronically excited NO_2 with water vapour, with subsequent experiments under more appropriate conditions to the atmosphere suggesting that the reaction of NO_2* with H_2O is unlikely to occur in the atmosphere (S. Carr, D.E. Heard and M.A. Blitz, Science, 324, 5925, 336, 2009). Please include some mention of this work in your discussion.

Page 9397, line 7: What was the limit of detection for BrO? Where there any measurements of IO? How would the presence of iodine radicals impact the conclusions in this work?

Page 9402, line 26-28: Is there any suggestion as to the source of the remaining model discrepancy? Is the discrepancy consistent with results from other field campaigns in similar regions?

Page 9405, lines 15-18: What is the change in the branching ratio for CH_2O production from the surface to the upper troposphere?

Table 1: Please change 'Rate' to 'Rate coefficient' in the table heading, and include appropriate units. Please give the parameters used to calculate the rate coefficient for R13. For reactions 20-23 it looks as though the reactions are with 'hγ' and not 'hν'.

Table 7: Please define the term 'Pct'.

Table A1: Please also provide references to instrument descriptions if available.

Figure 13a: It would be helpful if the *y*-axis were presented on a log scale.

Figures 17, 18 and 20: Please see above comment regarding use of 'ppt'. These figures are rather small in the ACPD format, please check that they will be clearly legible in the ACP format.

General comment

Recent work by Fuchs et al. (Fuchs et al., AMT, 4, 1209, 2011) has indicated potential interferences from alkene-derived RO_2 radicals in measurements of HO_2 by the FAGE technique. Please provide some indication as to the expected levels of interference for the ATHOS-FAGE instrument used in this work, and whether the interference has been considered in the model comparisons.