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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Closing the peroxy acetyl (PA) radical budget: Observations of acyl peroxy nitrates (PAN, PPN, and MPAN) during BEARPEX 2007" *by* B. W. LaFranchi et al.

Anonymous Referee #2

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The manuscript presents a detailed analysis of acyl peroxy nitrates (APN) measurements made during the BEARPEX 2007 campaign, aiming at obtaining closure on the budget of the corresponding peroxyacyl radicals. The paper is certainly of interest to the atmospheric chemistry community and should be published. However, I found the manuscript difficult to follow and was not convinced at the end if the conclusions drawn by the authors are fully supported by the data. An important point in this respect is the very long discussion on the applicability of steady state versus time dependent models. I found this part hard to follow and would thus suggest to (i) streamline the text and (ii) include at the end of this section a Table which summarises the salient conclusions. Likewise difficult to follow is the analysis of overall uncertainty, mainly because

the authors mix throughoutt the text vague statements and assumptions with quantitative information. The analysis seems to ignore the potential influence of transport and mixing. This should be discussed more explicitely.

Too little attention is paid in the text to the different data used and the respective uncertainties. For example, NO was not measured during the campaign. Instead, it was estimated from the photostationary state of NOx (PSS). This could constitute a significant sourece of error, as reaction with NO is one of the important loss processes for the peroxy radicals, and thus also determines the effective lifetime of the APNs. Several publications (e.g., Volz-Thomas et al., 2003) have discussed the fact that significant differences exist between observed NO/NO2 reatios and their estimation from PSS. Again, a table summarising the individual contributions to the overall uncertainty of the radical budget etc. would be extzremely helpfull for the reader. It would also be helpfull if more emphasis was put on the different data used, for eaxmple by including them in a figure. From the information provided in the manuscript, I cannot verify the uncertainty of 40 percent stated for the measurement of MPAN. The authors refer to Wolfe et al., 2007; 2009. According to these papers, the sensitivity of the method to MPAN is 3-8 times lower than that for PAN, which seems in contradiction to a 40 percen uncertainty.. As the APN ratios play an important role in the paper, the quantitative assessment of the errors is of paramount importance and must be clarified.

Specific comments: The title should be changed in order to something more appropriate (see comment above).

The abstract contains very little quantitative information. The first seven lines read like an introduction and should be moved to this section. The conclusions contain some of the information missing in the abstract. This should be rearranged.

The section entitled "Measurements" starts with a general review of the prevailing meteorological conditions mainly reviewing literature from prior campaigns. I would recommend moving this material into a section entitled meteorological situation, which

should then put more emphasis on the conditions encountered in 2007 in the context of what had been observed in prior years.

Literature: Volz-Thomas, A., H.-W. Paetz, N. Houben, S. Konrad, D. Mihelcic, T. Klüpfel, and D. Perner (2003): Inorganic trace gases and peroxy radicals during BERLIOZ at Pabstthum: An investigation of the photostationary state of NOx and O3, J. Geophys. Res., 108, doi:10.1029/2001JD001255.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9879, 2009.