

## ***Interactive comment on “Impact of the deep convection of isoprene and other reactive trace species on radicals and ozone in the upper troposphere” by E. C. Apel et al.***

### **Anonymous Referee #1**

Received and published: 13 December 2011

The manuscript presents an observational and modeling based analysis of two convective events over Canada with respect to transport of boundary layer species into the upper troposphere and the impact on chemical processes, in particular ozone production. The paper is well written and the observations provide important and valuable insight into the convective transport of chemical species. The analysis also is thorough and a well established model is used. The manuscript is appropriate for ACP and I recommend publication after addressing the minor revisions outlined below.

1. In the abstract it would be helpful to make a little clearer throughout whether the effects for the enhanced isoprene that are discussed are only for presence of low NO<sub>x</sub> or

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general. e.g. “PANs persist because of the cold temperatures of the upper troposphere resulting in a large change in the NO<sub>x</sub> mixing ratios, compared to the case in which no isoprene is convected, a scenario which is also explored by the model.” Is the PAN persistence the case for high and no LNO<sub>x</sub>? Does this equate with high or low NO<sub>x</sub>?

In order to make it easy to distinguish the two types of events I recommend the authors go over the manuscript again to ensure that it is as easy as possible for readers to follow, which type of event is discussed. In most places this is already obvious, but given the importance of the observations and the analysis, which are a great addition to our understanding of this topic, it might be worth it.

2. Figure 7 and discussion. It is mentioned and discussed that observed and modeled formaldehyde agree well during the event. However, before and after the event this is not the case, with observations showing substantially higher values than model. Can the discussion be expanded? Specifically, what confidence can one have that the good model/measurement agreement is meaningful if there is large disagreement other times. Without discussing this more, it might be hard to be convincing that the agreement is not a coincidence.

3. It would be beneficial to explain more clearly why the model of event 1 (figure 7) predicts large production of MVK+MACR (up to 500 ppt as sum) but not for HCHO. Observations reflect this and I assume that low NO<sub>x</sub> conditions contributes. Specifically, it would be useful to know the high and low NO yields of MVK, MACR and HCHO (not from secondary production) in the model. In fact the paper could possibly provide a useful observational point on yields under low NO<sub>x</sub> conditions, which is of substantial interest, and perhaps this could be briefly mentioned.

4. Figure 6: Please explain more clearly what the 3.5 hours and 1.0 hours mean. The main text of the paper does this, but it is not very clear from the caption.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27243, 2011.

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