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Interactive comment on “Free tropospheric peroxyacetyl nitrate (PAN) and ozone at Mount Bachelor: causes of variability and timescale for trend detection” by E. V. Fischer et al.

E. V. Fischer et al.

evfischer@gmail.com

Received and published: 9 May 2011

Response to Reviewer 3 Document

Title: Free tropospheric peroxyacetyl nitrate (PAN) and ozone at Mount Bachelor: causes of variability and timescale for trend detection Date: May 9, 2011

We thank all four anonymous reviewers for their thorough evaluation and constructive recommendations for improving this manuscript. Their comments and our responses are listed below. All authors listed on the manuscript concur with submission of the manuscript in its revised form. We have attached a pdf version of this text with our comments in *italics* as a supplement.

C3054

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In this paper the authors basically discuss interannual variability of PAN and diagnose usefulness of PAN as a proxy of photochemical ozone in conjunction with the detection of tropospheric ozone trend, particularly focusing on Asian emissions impacts. They showed year-to-year variability of PAN observed at Mt. Bachelor based on three-year dataset from 2008 to 2010, integrated other PAN measurements made in the Eastern Pacific region (though this attempt seems failed), discussed possible factors affecting interannual variability of PAN, and finally argued timescale of trend detection of PAN (and ozone) at Mt. Bachelor. I like the authors' idea and attempt. The idea to diagnose PAN as a proxy of photochemical ozone is not very original, but quantitative analysis and diagnosis associated with ozone trend is worth attempting, I believe. The data are quite new (2008- 2010!) and their focus on the trend during the last decade is timely, since Asian emissions are rapidly increasing after 2000. The paper potentially has great contribution to the scientific community dealing with long-term trends of tropospheric ozone and/or stratosphere-vs.-troposphere arguments. On the other hand, overall impression of the paper is somewhat weak. I feel that the analysis presented in this paper could be more robust if they elaborate on interpretation of interannual variability of PAN (and ozone) or on diagnosis of trend detection at some more other sites, where data are available.

We have elaborated on our interpretation of the interannual variability in response to the specific comments from this reviewer and the three others. We are not aware of any other PAN data that could be used for this type of analysis. The trend analysis of Cooper et al., [2010] uses all available O₃ data from the NE Pacific free troposphere, so we do not repeat that type of analysis here.

For example, the authors suggested biomass burning, transport efficiency, and vertical transport as three major causes for PAN interannual variability. However, their analysis seems circumstantial evidence.

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We have added an explanation of why presenting contextual information on fires, transport and temperature is important for both future data users and the calculations presented in Section 5.

It would be better if they could present some deeper analysis on this issue, for example, by using state-of art models, or anomalies in climate index and/or meteorological parameters. The interpretation could be improved if they discuss not only PAN but also ozone (or CO as a tracer) simultaneously. Since three years are obviously short, more effort to extend data period (2006 in Wolfe et al.) is highly appreciated.

We do use a climate/meteorological index LRT3 in our analysis, but we do not emphasize these results because there is not a statistically significant relationship between LRT3 and CO at MBO. LRT3 is an index designed to capture variability in the transpacific transport of CO. We have added additional details to Section 4.2, we have also added information on the interannual variability in water vapor and CO observed at MBO.

As another reviewer pointed out, there are significant uncertainties in using global chemical transport models to diagnose variability in the types of parameters that are likely to impact PAN.

Minor comments: The authors noted "ozone" in the paper title, but they did not discuss much about it. I would suggest "possible causes of variability" for the title, unless the authors analysis is going deeper.

Suggested change to title was made.

Figure 1: Can you include ozone (and more, like CO and aerosol) here?

We would prefer to keep the focus on PAN in Figure 1, but we have included additional discussion of campaign mean CO to Section 4.1. We present CO, O₃, and aerosol data for several plumes observed in 2008 in Fischer et al., (JGR 2010).

Figure 3: Data can be extended to 2010.

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In response to suggestions by this Reviewer and Reviewer 4 to shorten the manuscript, we have removed this Figure and added some additional discussion and clarification of this section.

ACPD

11, C3054–C3057, 2011

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C3054/2011/acpd-11-C3054-2011-supplement.pdf>

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 4105, 2011.

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