

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.

Anonymous Referee #4

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Review of "Free tropospheric peroxyacetyl nitrate (PAN) and ozone at Mount Bachelor: causes of variability and timescale for trend detection, by Fischer, Jaffe, and Weatherhead

The authors report springtime PAN measurements from 3 consecutive years made at an elevated ground site in Oregon (MBO) and attempt to relate PAN and its variability to (a) Russian fires, (b) transport and its variability, (c) vertical transport and its variability. The arguments are all quite plausible and common-sensical, but really not overwhelmingly convincing from a quantitative point of view. The data are valuable and merit publication. I suggest minimizing much of the discussion unless a more rigorous

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attempt is made at quantitative analysis. For example, how is the variability in Russian fires, plotted in Fig.3, related to PAN at MBO? I see little in the way of quantitative argument for this connection. Why is this figure in the paper? Perhaps this can be removed, and the paper can more purely focus on the data itself as part of long-term record that will become much more valuable if it can be continued for a decade more. Also, the connections with transport are not thoroughly demonstrated. Overall I find the data to be extremely valuable but the analysis of variability rather cursory. I recommend publication with either (1) a significant reduction in length, or (2) a more rigorous and thorough analysis of PAN variability and its causes.

Specific comments:

p.4109, line 3: "The lifetime of PAN" – need to clarify which lifetime this is. I think it's merely dissociative lifetime. As made clear later, this is not "real" lifetime, as dissociation is reversible.

p.4111, line 14: Seems an unsafe assumption to assume 93% conversion to PAN as this can depend on intensity of light source, and this can vary from lamp to lamp, and with lamp age, and with lamp operating temperature.

p.4111, line 18: Calibrations only every two weeks seems too infrequent. However, if calcs shows stability, then may be fine. Is this dominant contributor to the quoted uncertainty?

p.4114, line 14: The value of aircraft measurements is modestly marginalized with the comment that aircraft sampling is not random. What measurements are? Making measurements as single ground site does not provide a random sample of the atmosphere.

p.4115, line 3: INTEX-B/C. Was there an INTEX-C?

p.4115, line 19: I don't understand "average number of fires in each grid cell in the region". Assuming there are a number of cells in the region, there should be multiple averages at each point in time, one for each grid cell. I'm missing something.

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p.4115-6: It is plausible that fires are connected to PAN variability at MBO, but I don't see a connection made in section 4.1.

pp.4116: In reading section 4.2 I'm puzzled by the fact that the point is being made that 2008 and 2009 had weak long-range transport from Asia, while 2010 had strong transport. In my mind I contrast this with the mean data in Fig. 1 which show high PAN in 2009 and 2010, and low PAN in 2008. Why is PAN high in 2009 when transport is weak? However, the authors do not explicitly address this unexpected result. This reader was left wanting some discussion of this point.

P.4118: I now see my quandary (just above) is addressed. Would have useful to acknowledge it in section where first arose.

p.4118, line 15: I doubt that mean temperature is really a good measure of PAN loss. Two means could be the same, but one path might see greater extremes (high and low) and so could experience far greater PAN dissociation. Using the mean is a crude simplification.

p.4118, line 19: What is being called a "correction"? If it's the use of NO/NO₂ ratio in calculating PAN lifetime, I would not call that a correction. Rather it is simply the proper way to calculate the meaningful lifetime.

p.4120, line 7: The authors rely on a published quote to argue for lack of variability in transport out of Asia: "pool of Asian pollution in the western Pacific" is constantly replenished. But is it replenished to the same degree? Is it the claim that there is not, say, factor-of-2 variability in the replenishment, thereby leading to significant variability in PAN seen at MBO? This seems a very weak and non-quantitative argument – more just a plausibility argument.

p.4134, fig.1: Can barely see the x's or the small squares in my printed copy.

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