

Interactive comment on “Intercomparison and evaluation of satellite peroxyacetyl nitrate observations in the upper troposphere – lower stratosphere” by R. J. Pope et al.

Anonymous Referee #2

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Overview: The paper provides a comparison of PAN measurements from the MIPAS instrument on ENVISAT produced using two different retrieval methodologies. It also includes a comparison of the retrievals (and a suite of aircraft profiles) to a TOMCAT simulation of PAN. The paper is technically strong, and well written. Unfortunately, the paper does not really further our understanding of the sources of PAN in the atmosphere in any significant way. This makes it difficult to review, because there are no substantive conclusions drawn. That being said, this paper does represent the first presentation of a new dataset, which could have significant value. Thus, I recommend publication of the paper, but recommend that the authors consider the following idea for improvement. Both datasets (Figure 2 and Figure 3) show a consistent PAN maxi-

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mum in the tropical UT in SON. Can the authors use the data to attribute this feature? Casual mention is made throughout the text that the source is biomass burning, but no evidence is provided that this is the source. What does the model attribute this feature to? Is there a way to use any other simultaneously retrieved tracers (i.e. HCN or CO) to better understand whether the presence of this feature is driven by biomass burning or by lightning NO_x forming PAN in the presence of isoprene oxidation products lifted in convection?

I have only one minor comment. On Page 4, Line 21: PAN mixing ratios are on the order of several ppbv in heavily polluted air-masses. This sentence is strange here.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-386, 2016.

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