

Interactive comment on “First simultaneous measurements of peroxyacetyl nitrate (PAN) and ozone at Nam Co in the central Tibetan Plateau: impacts from the PBL evolution and transport processes” by Xiaobin Xu et al.

Anonymous Referee #1

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Summary: This paper presents in situ observations of PAN and ozone from Nam Co, a remote site in the Tibetan Plateau. Given relatively sparse PAN data, publication of this data is of interest. The most important conclusions of the paper are also likely sound. 1) The evolution of the boundary layer is an important driver of the diurnal cycles that are observed at this location. The discussion around this point could be substantially shortened though. 2) The site occasionally experiences air that is influenced by the upper troposphere. 3) This remote location is influenced from long-range transport of air pollutants from North India. However, I recommend substantial revisions before the

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paper is published in ACP. Major improvements are needed in the description of the methods. Several sections have significant grammar issues that cloud communication. I did not correct all the grammar issues, because there were too many to make that reasonable. The paper will need to be edited prior to publication in ACP. Finally, several of the figures need to be improved.

Major Comments: Section 2.2: The experimental details are insufficient and should be greatly expanded to include further details on the calibration technique and frequency. The detection limits and uncertainty should also be included. For PAN, please include additional information on the sampling. For example, how long was PAN was in the instrument, what was the inlet length, etc? It also appears that the instrument was calibrated with a relatively high NO reference gas. When was this NO reference gas calibrated? Also, if I understand the set-up, then that means that the calibration was done at a PAN mixing ratio substantially higher than the ambient measurements. Finally, Did the authors moisturize their carrier gas (presumably helium)? This is particularly important at the humidity levels that were experienced at this site. Without a humidified carrier gas, I would expect varying sensitivity to PAN that would not necessarily be accounted for by their indirect calibration with respect to CCl₄.

Section 3.4: There are worse grammar issues in this section compared to the other sections. The section should also provide the rationale for using the 500 hPa water vapor cut off for the analysis.

Page 13, Line 11: I do not understand this sentence. Page 13: Line 16: What is omega? How should it be interpreted in this context.

Page 14, Lines 22-32: If this section remains in the paper, I suggest that the authors reach out to someone from the MIPAS PAN community to do a proper comparison. There are many issues here. I do not believe that the sources and magnitude of uncertainty associated with the MIPAS PAN measurements are adequately or correctly summarized. It seems as though the paper is trying to compare an instantaneous in

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situ measurement with a monthly average, which is not the right approach. The authors also appear to assume that there is no PAN enhancement in the atmosphere between the UT observed by MIPAS and their ground site, which seems like a big leap. The interannual variability in PAN can be quite large in the free troposphere (e.g. Zhu et al., 2015 GRL), and so comparing different years may not be a good approach.

Zhu, L., E. V. Fischer, V. H. Payne, J. R. Worden, and Z. Jiang (2015), TES observations of the interannual variability of PAN over Northern Eurasia and the relationship to springtime fires, *Geophys. Res. Lett.*, DOI:10.1002/2015GL065328.

Section 3.5: This section again has major grammar issues and some logic issues. 1) PAN is only short-lived if it is warm. 2) Page 15, Lines 24-28 are too general to be of use, and may contain logic that does not apply to the conditions sampled at the site. 3) Page 17, Line 14: Are all the trajectories valid? Did the authors remove trajectories that had an altitude of zero agl? These trajectories are invalid. 4) The discussion of the TEMIS data seems distracting (page 16, lines 22-32). I recommend removing this.

Table 1: Please make it clear (perhaps with different sections) which are satellite or model output. Also, the observations presented in this work were not made at 350 hPa, and so it is odd to put this in the table. A better approach would be to present the mean of all the data collected, and then call out a subset of interest.

Figure 1: It is hard to read the green text on the Figure. Please use black or white. Please also add lat/lon to the map.

Figure 4: It is unclear why U and V wind speed need to be shown in addition to Wind speed.

Figure 6: Use "O3 versus PAN" rather than "O3-PAN" in the caption description of the panels. There is a typo on the caption. It should be Pa, rather than hPa, correct?

Figure 9 & 10: It is really hard to see the trajectories. Overlaying black on dark blue is not a good choice here. It is also hard to see the underlying map. Please change the

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color scale of the PV fields.

Figure 11: The font is too light and too small on the legend.

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