

Interactive comment on “Decoupling peroxyacetyl nitrate from ozone in Chinese outflows observed at Gosan Climate Observatory” by Jihyun Han et al.

Anonymous Referee #2

Received and published: 3 April 2017

Summary: This paper presents in situ observations of PAN and supporting gas and aerosols from the Gosan Climate Observatory. The analysis focuses on attributing and differentiating four episodes of elevated PAN. Given relatively sparse PAN data, and continued interest in the role of PAN in the export of pollution from East Asia, publication of this data is of great interest. However, I recommend substantial revisions before the paper is published in ACP. I recommend shortening the paper substantially, while also increasing the methodological details. Rather than focusing on the “decoupling of ozone and PAN”, which is not actually novel, particularly in biomass burning plumes and in instances of long range transport. I think it would be better to simply present this as an attribution of four different instances of elevated PAN.

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Major Comments: Lines 89 - 114: The experimental details are insufficient and should be greatly expanded to include appropriate references for every measurement used; details on calibration technique and frequency should also be reported. The detection limits and uncertainty should also be included. More detail is particularly important for the NO and NO₂ measurements, which are used in the PAN lifetime estimates. The NO₂ measurement is likely to include other NO_y species. This should be noted and discussed. For PAN, please include additional information on how long PAN was in the instrument, what was the inlet length, etc. 100 pptv is a very high detection limit for an in situ measurement. The calibration technique is also infrequently used. Please explain why 850 m was chosen for the trajectory initialization height.

Line 129: Start a new paragraph. It is unclear what ‘haze’ means. I believe this is simply a period with elevated aerosol concentrations

Section 4.1: I recommend removing this section and Figure 3. This section and that Figure are confusing, with low science content.

Section 4.2: I recommend reordering this section to discuss clear similarities and differences between these episodes. Lines 192-193: It is odd to claim something is decoupled if it is correlated with something.

Section 4.3: I recommend reordering this section to discuss clear similarities and differences between these episodes. I am skeptical of the PAN lifetime calculations given the unknown quality of the NO₂ measurement. More discussion is required. Line 240 – 241: I find these sentences quite confusing, and I do not understand the logic.

Lines 266 – 275: Model methods belong in the methods section. This section was clearly written by a single coauthor and has a different quality of English than the rest of the paper.

Line 277-278: I think the main point of this section is to show that the model severely underestimates PAN, particularly when biomass burning is a source. Thus shorten to

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just make this point.

Figure Comments: Figure 1: The ozone line is not visible when Figure 1 is printed. The wind barbs are too small.

Figure 2: It hard to actually get hour-to-hour differences from this figure. Ideas for improvement include making log y-axes. Or a 2x2 figure, rather than 1x4. Figure 3: remove Figure 4: Remove redundant longitude line labels. "O3" and "NO2" overlap the bottom of the scale. All species need units on figure. No vertical information is shown for these trajectories. Did any intercept the ground – were they removed? Figure 5: a/b) why is the fit only shown for Episode 4? Figure 6: It is probably not necessary to show the correlation with all aerosol constituents. Again, why is only the Episode 4 fit shown. Figure 7: Add labels for events as in Figure 1.

Other Minor Comments:

Line 29: Extra E in lat/lon

Line 61: Awkward sentence

Throughout: exchange "concentration" for "mixing ratio" when referring to the abundance of gas phase species

Line 68: A few pptv of PAN is observed in the most remote locations. Change to "In the most remote. . ."

Line 71: NO_x has recently declined in China (e.g. Liu et al., 2016, Environmental Research Letters, Volume 11, Number 11)

Line 71- 81: It would be good to mention in this introduction that PAN is only sparingly soluble. Since there is an eventual discussion of biofuel/ biomass burning, more recent relevant references for elevated PAN plumes attributed to biomass burning downwind of Russia and East Asia include:

Zhu, L., V. H. Payne, T. W. Walker, J. R. Worden, Z. Jiang, S. S. Kulawik, and E. V.

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Fischer (2017), PAN in the eastern Pacific free troposphere: A satellite view of the sources, seasonality, interannual variability, and timeline for trend detection, *J. Geophys. Res. Atmos.*, 122, doi:10.1002/2016JD025868.

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.

[Interactive comment on Atmos. Chem. Phys. Discuss.](#), doi:10.5194/acp-2016-1107, 2017.

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