

Interactive comment on “The Contribution of Fires to TES Observations of Free Tropospheric PAN over North America in July” by Emily V. Fischer et al.

Anonymous Referee #2

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In this manuscript, the authors investigate the overlap of TES peroxyacyl nitrate (PAN) detections (defined as $\text{DOF} > 0.6$) with HMS smoke extent and TES CO retrievals in the western United States. The authors 1) quantify the fraction of “enhanced” TES retrievals ($\text{DOF} > 0.6$) overlapping HMS smoke extent by month and year, 2) perform two case studies using FRAPPE, HYSPLIT, MODIS and CALIPSO data and 3) evaluate the ratio of TES tropospheric average CO and PAN enhancements.

Based on feedback from preliminary reviews, the authors have removed comparison of TES PAN measurements with model simulations from the manuscript but have not substantially improved the quantitative characterization of the retrieved product. Ad-

C1

ditionally, there are editorial and typographical errors in the manuscript (e.g., Figure 6 top panel appears almost identical to Figure S3; seemingly erroneous in text figure references – Line 178).

Further characterization of TES PAN retrievals is important and the authors have worked towards that goal. However, I recommend that the authors use language that more precisely conveys the uncertainty of the product. In the introduction, for example, the authors state that “Satellite measurements [i.e., TES] are essential to understand the seasonal cycle and interannual variations of PAN (L65).” Here and similarly throughout, use of a strong word like “Essential” connotes long-standing maturity and widespread use. In this case, I would recommend using a “potential tool” instead of “essential”. While this is one specific example, edits should be made throughout the manuscript to address this concern.

The examples below indicate where the authors can add information to the manuscript from their already accomplished analyses that will be useful to the reader.

–Figure SI 4 seems to imply that there is no statistically significant difference between “In smoke” and “Not in smoke” retrievals. Is this expected? This needs to be addressed in the main text.

–One of the paper’s main findings is that $\sim 15\text{--}32\%$ of PAN detections ($\text{DOF} > 0.6$) overlap with HMS smoke extent. Is that a larger or smaller number than expected? Please report the percent of all attempted TES retrievals that overlap HMS smoke extent for context.

–The authors report that there is no statistical difference between PAN “detections” ($\text{DOF} > 0.6$) that do and do not overlap HMS smoke extent. Please discuss this finding in context of past airborne or mountain-top in situ studies: should we expect there to be a statistical difference in PAN concentrations in biomass burning influenced air and other polluted airmasses.

C2

–There is one case study involving CALIPSO data. The paper might benefit from a more statistically robust analysis of CALIPSO and TES data. The meridional offset of ~500 km between the two sensor tracks (Figure 6) could likely be overcome by the incorporation of reanalysis wind products and the potentially higher quality smoke information provided by CALIPSO.

L92 - “The TES PAN retrievals shown here were processed using a prototype algorithm for the area and time periods of interest.” Please provide more details regarding differences between this prototype and the operational retrievals. What is the source of a priori profiles in the retrieval? What is the tropospheric average PAN concentration in the a priori profiles? The prior profile shown in Figure panel 2d shows PAN concentrations between 300 ppt and 400 ppt – if so, please justify use of an assumed “background” concentration of 100 or 200 ppt in the PAN/CO enhancement analysis in Section 3.3.

L96 – “For footprints where the spectra show strong evidence of this silicate feature in the surface emissivity (this can occur over rocky or sandy surfaces) TES PAN retrievals are not attempted.” What fraction of retrievals is discarded by this requirement? Does dust aerosol have a similar silicate absorption or emission feature that should be considered in the retrievals?

L118 – What is “extremely elevated”? Please quantify.

L130 – “We only include data with DOFS > 0.6 to ensure that the retrievals are dominated by real observed information.” “Dominated” is a rather strong word to describe DOFs > 0.6. The meaning of “real observed information” is unclear.

L132 - “This conservative choice means that we are primarily basing our analysis on retrievals with high PAN” What is the mean and standard deviation of retrieved PAN concentrations?

L172 – 193 – The purpose of this paragraph is not clear to me. Furthermore the

C3

evidence is not all that convincing as the expected relationships between smoke and TES PAN detections is not consistent or as expected. I did not find Figures 3 or 5 to be particularly helpful either.

L194-210 + Figure 4 – The data in this paragraph and figure can be used to compute a PAN:CO ratio that is less dependent on assumptions of background contributions. Based on a cursory visual analysis of Figure 4, a value of 0.3% PAN:CO appears reasonable.

Figure 4 – The data that is “not in smoke” should be separated similarly to the data that is “in smoke.” Is the relationship between CO and PAN similar “not in smoke” retrievals similar to their relationship “in smoke” (see above comment)? Should that result be expected based on previous comparisons of anthropogenic (not in smoke enhancement) and biomass burning influenced (in smoke) air?

Editorial comments:

Below is a non-exhaustive list of editorial suggestions or errors. I recommend that the authors thoroughly proofread before re-submission.

L1 – Re-define PAN as peroxyacyl nitrate in the body of the manuscript.

L140 – “infrared” [imagery].

L174 “NOAA HMA smoke plume”

L178 – “Supplemental Figure 1” – Perhaps intended for Supplemental Figure 2

Figure S3 and Figure 6 top panel appear to be identical.

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C4