

***Interactive comment on “Nitrogen oxides and PAN in plumes from boreal fires during ARCTAS-B and their impact on ozone: an integrated analysis of aircraft and satellite observations” by M. J. Alvarado et al.***

**Anonymous Referee #2**

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General comments:

This paper attempts to improve understanding of chemistry within fresh, aged and old biomass burning plumes using aircraft and satellite observations made during the ARCTAS-B campaign. It provides the first observational confirmation of rapid PAN formation in a boreal smoke plume but there was little evidence for ozone formation within the boreal smoke plumes measured during the campaign period. I find the paper very clearly written and should be accepted by ACP once the authors address the specific points listed below.

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Specific comments:

Page 15330, line 17: How did the authors arrive at the assumptions made to identify the plumes? The choice of 20 ppb seems rather arbitrary to me. Is this a standard technique to identify boreal plumes? Also, why was  $r^2 > 0.3$  chosen as the filter for a significant correlation?

Page 15332, line 1: In section 2.2, the authors discuss the use of TES observations to look at ozone and CO enhancement in boreal smoke plumes. Median vertical profiles from DC-8 aircraft data are shown in Figure 5. Could the authors please comment on the thickness of individual plume “layers”. To what extent does the vertical thickness vary? This is a very important observation and has a direct bearing in section 3.3.3 which uses the TES satellite instrument to detect CO and ozone in plumes.

Page 15334, line 1: The authors refer to the Val Martin et al., 2010, paper which derived that only 4-12% of North American fire plumes are injected above the boundary layer. It is my understanding that this study excluded pyro-cumulus clouds intermingled with smoke, so was possibly biased low. The authors comment on page 15337, line 14, that they find a value higher than this from the DC-8 measurements. One point is included (labelled “A”) which was one such cloud. Were the authors sure that none of the fires labelled in section B and C were associated with pyro-cumulus cloud, leading to a higher bias?

Page 15338, line 14: In the section on enhancement ratios the authors contrast their findings with aged plumes over the Azores. Could the authors please comment as to how significant the vegetation type is (i.e. the type of boreal biomass burned) to the difference in results?

Page 15339, line 1: As the error bars overlap, these two measurements are the same in my opinion.

Page 15345, line 25: In section 3.3.3 the authors compare GEOS-chem results with

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TES special observations. Given that TES is a nadir viewing satellite instrument, how sensitive is TES to tropospheric ozone and CO? It is mentioned that the TES averaging kernels are applied to the GEOS-CHEM data, which is a generally accepted approach when comparing atmospheric measurements to a model, but no averaging kernels are shown in the paper. Can the authors please include a “representative” averaging kernel which provides the reader with more information on the vertical sensitivity of the measurement and comment on the tropospheric sensitivity of TES to CO and ozone. Can they be sure that the retrievals are not just reproducing the a priori information?

Technical corrections:

Figures 10, 11, 12, 13, 14, 15: what does the solid blue line represent?

Figures 5, 6, 7, 8, 10, 12, 13: could the authors please replace “O3” with “O<sub>3</sub>”.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 15325, 2010.