

Atmos. Chem. Phys. Discuss., author comment AC1  
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## Author's Reply to CC1, RC1 and RC2

James Roberts

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Author comment on "Comment on "Isotopic evidence for dominant secondary production of HONO in near-ground wildfire plumes" by Chai et al. (2021)" by James M. Roberts, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-715-AC1>, 2021

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Response to Comment and Reviews:

The response to comments and reviews are given below, with the proposed additions to the manuscript in quotes.

Response to CC1

This comment from the authors of the original paper acknowledges the interference of PAN in the NO<sub>x</sub> sampling method. They point out that the <sup>15</sup>N isotopic signature in their collected "NO<sub>x</sub>" in aged air did not change as much as they would have expected based on theoretical considerations. This is indeed an unresolved question given that we know it was extremely likely that substantial amounts of PAN were present in the aged air masses, and other NO<sub>x</sub> photochemistry happening that needs to be considered. The resolution of this conundrum is well beyond the scope of this Comment. Instead, I have added the following concluding sentences:

"The community engaged in <sup>15</sup>N isotopic analysis appears to be left with a conundrum: why don't <sup>15</sup>N signatures, from NO<sub>x</sub> + PAN and other reactions of NO<sub>x</sub>, match their current understanding of the effects of photochemistry? As with many such situations, this is an opportunity to learn and refine our understanding of <sup>15</sup>N cycling in atmospheric photochemistry."

Response to RC1

I thank Dr. Jaffe for his supportive comments and for detailing his group's measurements near Boise, ID during the 2017 fire season. I have now noted those measurements in the revised Comment:

" There are ground-level measurements of PAN and oxides of nitrogen in the Boise, Idaho urban area during the 2017 WF season (McClure, and Jaffe, 2018), and they show that PAN levels are a substantial fraction of odd-nitrogen, and are certainly significant relative to NO<sub>x</sub>."

There do not appear to be any other aspects of Dr. Jaffe's review that require a

response on my part.

Response to RC2

I thank the reviewer for these supportive comments. As noted above, I have added concluding remarks concerning the lack of agreement of the  $^{15}\text{N}$  measurements with expectations given the importance of other  $\text{NO}_x$  photochemistry in this environment.