

Interactive comment on “Characterization of a catalyst-based total nitrogen and carbon conversion technique to calibrate particle mass measurement instrumentation” by Chelsea E. Stockwell et al.

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Stockwell et al. report a thorough and satisfying performance evaluation of a catalyst-based approach to measuring particulate reactive N. Although others have explored similar approaches, the work has largely gone unpublished or lacked the thorough evaluation provided by the current authors. There is a compelling need to quantify total reactive N in airborne particles and I commend the authors for their efforts. I also commend them for the thoroughness of their evaluation and the care in which they describe limitations to their approach (e.g., the need to look at particulate OC in a

C1

CO₂-free stream, the importance of eliminating PILS-ESI-MS matrix/ion suppression effects by using single component standards, etc....). Their findings will be very useful to the broader atmospheric chemistry community, extending from those interested in source characterization to those interested in deposition and particle effects on human health and radiative scattering. I have a few suggestions for minor changes to improve the manuscript.

1. Title: I found the title confusing and somewhat misleading. The focus is primarily on N and primarily on direct measurement of particulate (or total) reactive N. The title should better reflect that.

2. Abstract: The mention of particulate organic carbon conversion in the abstract is, I suppose, appropriately brief. I do suggest that the authors here refer to "efficient" or "complete" conversion rather than simply conversion. I also suggest they point out here the important challenges of determining particulate OC by this method against a high concentration ambient background, as described in the manuscript itself.

3. Section 3.1.1. The authors refer here to experimental methods not described in the methods section of the manuscript. I suggest an Experimental Details section be added on methods for checking gas-phase conversion efficiency. This would allow the authors to clearly convey information about calibration standards and comparison gas-phase measurement methods. Section 3.1.1, for example, talks about apparent errors in the assumed ammonia absorption cross-section, but this is confusing because the reader has not been told how this is relevant to the gas-phase ammonia measurement method. The latter has not been specified.

4. p. 8, line 29: It seems a bit odd here that the authors refer just to negligible interference from N₂O conversion in biomass burning sources. Why only discuss BB and not other (e.g., auto exhaust, ag, etc...) sources. The focus makes a bit more sense given later discussion about the Missoula FIREX experiment, but since this manuscript is really addressing a more broadly applicable approach, it would be helpful to broaden

C2

the N₂O interference discussion beyond BB.

5. top of p. 9: It is my sense that it is not so uncommon for NO concentrations to be in the range of 10s of pptv in remote regions. I suggest the authors better justify or moderate their claim that an NO interference of 28 pptv is "clearly a negligible amount in almost any atmospheric context."

6. Section 3.1.4 and Fig. 6. This is an interesting timeline of deriving "excess" reactive N from the new instrument measuring a smoke plume. Do the authors have a measurement of HNO₃ in the airstream? I suggest that modified combustion efficiency (MCE) be added as a parameter in Fig. 6, if available, to help make the authors' point re: periods of smoldering vs. flaming combustion.

7. typos:

a. p.6, line 11: change "least-squared" to "least-squares" b. p. 6, line 35: change "promoted" to "promote" p. 12, line 20: change "liens" to "lines"

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