

We thank reviewer #1 for having carefully read our manuscript and for making very valuable comments and suggestions. Here is how these were addressed:

***The only major issue is stated in line 100 - in field calibrations were inconsistent. It's not clear how much of an issue this is, but anytime a calibration fails in situ one is concerned about the quality of the data. The authors need to provide more detail so this concern can be alleviated. How confident are they that the measurements are accurate based on the laboratory calibration.***

This should indeed have been explained better. We now provide more details and the modified text reads as follows:

“We performed three types of calibrations in the field: i) periodic in-flight calibrations using a dynamically diluted calibration standard in a pressurized cylinder (10 ppm NH<sub>3</sub> in N<sub>2</sub>; Praxair Distribution Inc., Lancaster, CA, U.S.A.), ii) a ground-based calibration using a dynamically diluted calibration standard in a pressurized cylinder (2.7 ppm NH<sub>3</sub> in N<sub>2</sub>; provided by NOAA's Chemical Sciences Laboratory), iii) a ground-based calibration using an NH<sub>3</sub> permeation source (provided by NOAA's Chemical Sciences Laboratory). While results from the cylinder-based calibrations were in good agreement, the permeation tube based calibration yielded an a factor of two higher instrumental response factor. For resolving this inconsistency, we carried out an extensive post-mission NH<sub>3</sub> calibration in the laboratory. For that purpose, an artificial atmosphere (NH<sub>3</sub> in air) was generated in a 250 L environmental (“smog”) chamber equipped with a Fourier Transform Infrared (FT-IR) spectrometer (Bruker IFS 66v/S). The concentrations of NH<sub>3</sub> (accuracy: ±5 %) were determined from the FT-IR spectra (120 m path length, 0.125 cm<sup>-1</sup> spectral resolution) in a global non-linear least squares spectral fitting procedure (Griffith, 1996) employing the absolute cross sections of NH<sub>3</sub> (Gordon et al., 2017). The instrumental response factor derived from the post-mission calibration agreed to within ±15 % with the response factors obtained during the in-flight calibrations. We concluded that the permeation rate determined in pre-campaign laboratory measurements had changed in the field. The estimated accuracy of the reported NH<sub>3</sub> mixing ratios is ±15 %. We note that this accuracy estimate is not valid when NH<sub>3</sub> mixing ratios abruptly changed and inlet/instrument surfaces were not equilibrated.”

***In the introduction, line 59- it would be useful to provide more information to the reader on the differences of the Innsbruck instrument to the other instruments discussed in the paragraph.***

No other instruments are actually discussed in this paragraph. For making this clear, we now refer to the “conventional PTR-ToF-MS instrument” used in the cited previous studies and the “modified PTR-ToF-MS instrument “used during FIREX-AQ. The modifications are described in section 2.2.

***Section 2.2 - The authors should provide some evidence to support the statement on line 87 that the instrumental background was reduced to single digit ppb levels.***

This is actually not easy to show in a simple plot. The instrumental background consisted of a “chemical” background (*i.e.*, NH<sub>3</sub> formed in the plasma source) and a “mass spectral” background (*i.e.*, a peak feature in that region of the mass spectrum that caused an enhanced baseline at  $m/z$  18.034). The latter was peculiar to the instrument/operational settings used during FIREX-AQ. We did not deconvolute the two contributions in our automated data analysis. A manual reanalysis of selected mass spectra shows that the chemical background ranged from sub-ppb levels (observed under optimized conditions in the laboratory) to single and sometimes even double-digit-ppb levels (observed in the field). We feel that a measurement report does not need to go into such fine instrumental details. We have thus removed the sentence in

which we make a quantitative statement. In our opinion it is sufficient to qualitatively describe that He addition to the source drift region effectively suppresses  $\text{NH}_3$  formation in the plasma. It will anyway vary from instrument to instrument how low the chemical background will get.

***Line 111 - what are the implications of the 34% uncertainty in the  $\text{NH}_4^+$  data on the final calculated emissions factors? Can that uncertainty somehow be propagated through?***

Fire emission factors have a high natural variability and we feel it is important to show the extent of this variability, without confounding it with the measurement uncertainty (and the uncertainty in the carbon fraction). We do, however, explicitly mention the uncertainties of the underlying measurement variables ( $\text{NH}_3$ ,  $\text{NH}_4^+$ ,  $\text{CO}_2$ ,  $\text{CO}$ ,  $\text{CH}_4$ ), providing the reader with all measurement uncertainty information. The measurement uncertainty in  $\text{NH}_4^+$  actually has only a small impact on the uncertainty of  $\text{NH}_x$ : an average  $\text{NH}_4^+$  fraction of 29% converts into a 15% uncertainty for  $\text{NH}_x$ , the maximum  $\text{NH}_4^+$  fraction of 52% converts into a 19% uncertainty for  $\text{NH}_x$ .

***Line 114 - There is potentially a lot of organic N compounds in burning emissions (e.g., Mace et al., 2003 Water-soluble organic nitrogen in Amazon Basin aerosols during the dry (biomass burning) and wet seasons). This could be contributing to the large amount of  $\text{NH}_4^+$  measured early on in the fires. This needs to be stated very clearly throughout the paper as the authors have no way of knowing if this is an issue or not.***

This is clearly stated in section 2.2 of the original manuscript: “We note that, based on the current state of knowledge, the AMS  $\text{NH}_4^+$  data collected in fresh smoke plumes suffer from a minor ( $\leq 20\%$ ) positive interference from reduced organic nitrogen compounds. A general correction is still under development based on positive matrix factorization (PMF) analysis.” In the revised manuscript, we have added a subsentence and the given reference: “...reduced organic nitrogen compounds, which are known to be abundant constituents of biomass burning particles (e.g., Mace et al., 2003).” We also reiterate our comment in the Results section (3.1): “As stated in section 2.2, the  $\text{NH}_4^+$  measurement suffer from a minor ( $\leq 20\%$ ) positive interference from reduced organic nitrogen compounds.”

***Section 2.3 - is this the most commonly used method for calculating  $EF_{\text{NH}_3}$ ? Also, why was the carbon fraction assumed to be 0.5? This should be justified.***

Yes, this is how emission factors are usually calculated, although we are using a simplified formula that only applies to  $\text{NH}_3$ .

We have added a brief explanation on the carbon fraction:

“ $F_c$  is the fraction of carbon in the fuel, which is typically in the 0.45-to-0.55 range (Akagi et al., 2011 and references therein). We assumed  $F_c$  to be 0.50 and note here that the resulting 10% uncertainty in  $EF_{\text{NH}_3}$  is small compared to the natural variability of  $EF_{\text{NH}_3}$ .”

***Conclusions on line 215 - If the discrepancy is based on the tailing and downwind data, then the authors could prove that by doing the calculation on their data excluding tailing and the downwind data. This would provide confidence in the conclusions.***

The Gkatzelis et al. manuscript is still in preparation. We have deleted this paragraph and reference, as suggested by the other reviewer.