Here we copy over the comments of Reviewer 2 for our preprint, with our responses to each in blue bullet points. We greatly appreciate the discussion and points raised.

Major Comments

1. Since the t-IMR represents an improvement over traditional IMR designs, it would be valuable to provide additional quantitative data. For example, the authors could briefly summarize the limits of detection, sensitivity, and wall effects for representative species under conventional configurations. This would allow readers to more directly appreciate the extent of the improvements achieved with the new IMR.

- we have added a table summarizing LODs and sensitivities for key compounds, in addition to adding results of an HNO3 wall effects experiment to the appendix

2. The authors acknowledge that one limitation of the t-IMR is the inability to control humidity. This raises the question of how the reported concentration data were calculated during field observations. Was it assumed that humidity has no effect on sensitivity? Given that specific concentration values are presented and that these species are known to be influenced by humidity in traditional IMRs, a detailed description of how humidity effects on sensitivity were accounted for is essential. Furthermore, was any on-site calibration performed during the observation period? If so, these data could be used to further analyze and discuss the role of humidity. Since the treatment of humidity directly determines whether the instrument is truly field-deployable, and given that the authors have already applied the t-IMR in field studies, it would be highly valuable to provide recommendations regarding the handling of humidity in such applications.

- We have added and referred to a new section in the appendix outlining the water vapor dependence of Br2 and HNO3 sensitivity as we were able to test it in the lab. Time series figures now reflect a dynamic humidity-dependent sensitivity used to convert signals to concentrations.

3. Overly descriptive text – Several sections (e.g., Sect. 2, description of dimensions and housing) read like a technical manual. While detail is important for reproducibility, the writing could be more concise and structured (perhaps moving some details to the Appendix).

- We've removed some tertiary dimensions and technicalities that are not necessary for the demonstrations and conclusions of the manuscript

4. Axial vs. transverse geometry (discussion point) - Many atmospheric-pressure CIMS instruments (e.g., HOx-CIMS) employ an axial geometry in which the sampled flow is directed straight into the pinhole. By contrast, this study adopts transverse geometry. It

would be useful for the authors to briefly comment on the rationale behind this choice and how it may compare in terms of sensitivity, turbulence, or wall effects. This would help contextualize the work for readers familiar with axial designs.

- A few sentences have been added in the IMR description section about the choice of transverse geometry preventing neutral compounds from diluting and contaminating analyte flows, as well as reducing potential for turbulence and eddy formation at the capillary MS entrance. We also report our reynolds number and compare wall effects directly with Palm et al's coaxial design.

Minor Comments

L216: I understand that it may be challenging to evaluate the dependence of sensitivity on humidity in the laboratory given the high sampling flow rate. However, was the potential variation of sensitivity under different humidity conditions examined during field calibrations? In traditional IMRs, for example, Br2 is known to be significantly affected by humidity within certain ranges. How was the influence of humidity accounted for during the BLEACH observations?

- We now include and reference an appendix section on water vapor dependent sensitivities calculated and applied to bromine and nitric acid measurements. Corresponding figures have been updated

L306: The authors should report explicit limits of detection. Although the calculation formula may be cited, the authors should provide a worked example using Br2 that lists all parameter values used in the computation. Because the LOD is a key criterion for assessing whether the t-IMR outperforms traditional IMRs, presenting the full set of inputs and the resulting LOD for Br2 is essential for a transparent and fair comparison.

- We have added the parameters for Br2 in a statement where we reference Bertram's equation.

L328: The reported Br2 sensitivity of 1.5 counts ppt-1 per 1e6 TRIC is substantially lower than the value shown in Figure 2. Please clarify the source of this discrepancy. If the value of 1.5 counts ppt-1 per 1e6 TRIC is correct, it appears lower than that of traditional IMRs. Please discuss whether the t-IMR design prioritizes a lower LOD at the expense of sensitivity, and explain the underlying factors that could account for this trade-off.

- The experiment performed for figure 2 was in a laboratory environment while the 1.5 is reported for the in situ measurements at THMAO, which is much more humid and has higher temperatures, affecting sensitivity. Different CIMS tuning voltages also likely contribute to this discrepancy, as a significant amount of time

exists between this experiment and BLEACH, with tuning adjustments performed in the field.
- It does seem that compared to low pressure designs, at least at such high humidity, the t-IMR sacrifices sensitivity for lower LOD, likely stemming from ion-molecule interaction time, large sample flows, and reduced wall effects/background signals

L341: The specific sensitivity of HNO3 should be reported. If the background concentration of HNO3 cannot be subtracted, it is unclear why this compound was nevertheless selected for demonstration. I suggest that the authors briefly explain the rationale for choosing HOBr and HNO3 as representative species in their analysis.

- HNO3 was selected because it is well known to be less volatile and deposit on IMR surfaces contributing to wall effects. The inability to be background subtracted is only due to one fitting in the N2 flow delivery apparatus for zeroing, likely contaminated from an experiment with HNO3 performed before BLEACH. HNO3 is shown because, despite not being background subtracted, the lowest concentrations recorded (assumed to be larger than or equivalent to instrument background or noise) are shown to be relatively small, with a minimum of 0.3 ppt and a 10th percentile value of 1 ppt. HNO3 sensitivity is now reported in a table with that of the other key species

Other Comments

The authors are advised to carefully check the manuscript for grammatical issues as well as the correct formatting of superscripts and subscripts. Below, I list only the instances I have identified.

- **The following formatting and grammar issues have all been resolved in the manuscript**

L3: “shows significant improvements in potential measurement interference” it is awkward to say improvements in interference. Mitigation or reduction sounds better.

L4: “reduce”, without “s”.

L7:“… exhibited by alpha-pinene ozonolysis” sounds ungrammatical. “…as demonstrated in α-pinene ozonolysis experiments” sounds better.

L16: “affects” instead of “effects”.

L38:” However, by reducing the pressure of the IMR dilutes sample molecular number concentration by up to several orders of magnitude.” Delete “by”.

L41: Change ”For already low concentration specie” to “For species present at very low concentrations”.

L82: “time series” instead of “timeseries”, in here and the rest of the manuscript.

L159: “its” instead of “it’s”

L200: The “2” in N2 should be formatted as a subscript (N2).

L231: “their” instead of “them”

L238: “factor of 2” instead of “factor 2”.

L254 “from” instead of “off of”.

L293: “… located on the southwestern shores of Bermuda operated by the Bermuda Institute of Ocean Sciences (BIOS).” lack of conjunction “and”.