Review for AMT-2025-3709, “Laboratory and field Characterization of an Atmospheric Pressure Transverse Chemical Ionization Ion-Molecule reaction Region” by Rund et al.

The authors describe an updated design of a transverse atmospheric pressure Ion Molecule reaction Region (t-IMR) for use with chemical ionization mass spectrometers (CIMS). The authors describe in detail the details of operating and optimizing this t-IMR and demonstrate the capability of measuring trace gases at a ground site in the marine boundary layer. The data presented in this manuscript is quite impressive for difficult to measure sticky trace gases such as HOBr and HNO_3, and should be published. I have concerns about how the instrument could be calibrated for analytes difficult to deliver in the field, as the IMR conditions are not held constant (i.e. water vapor, pressure) during deployment and would require chamber experiments at field conditions in order to measure sensitivity as a function of these conditions. I would like to see the data for IH_2O/I included in this manuscript to make the operation conditions of this system clearer to other users.

Detailed comments:

Line 11, “Finally, we demonstrate the capability of the t-IMR…”, it would be useful to report sensitivity and LODs for HOBr, Br_2 and HNO_3 in the abstract. It would be even better to compare these LODs and sensitivities to the standard ‘low-pressure’ IMR design.

Line 25, add CF_3O^-, to this list, one of the major adduct ion chemistries.

Line 31, maybe add the design impacts backgrounds as well, which doesn’t impact sensitivity or selectivity directly, but is important in instrument performance, and one of the benefits of the t-IMR design.

Line 46, “And of equal importance…”, I think the authors a referring to wall effects and effectively the importance of quantifying backgrounds in the instrument accurately? As worded it is a little unclear with the authors mean.

Line 88, “Teflon™…” is this FEP or PFA?

Line 100, “via a gaseous mix…” Please specify mixing ratios and flowrates of MeI and toluene

Line 112, “The orifice, highlighted in purple in the inset, is designed specifically to maintain the pressure difference between the IMR at a somewhat dynamic atmospheric pressure on the order of ~1000 mbar and SSQ operating at 2 mbar.” Does the orifice change diameter? How does it maintain a pressure difference between the IMR and SSQ? Is the pressure in the tIMR monitored continuously?

Line 116, “…to provide a small concentric flow of UHP nitrogen…” how much flow is this, how is it controlled?

Line 166, “*d* is the diameter of the IMR tube (m).” One could consider the characteristic length in the Re calculation as the IMR pipe length rather than the diameter. The authors should also calculate Re this way to ensure the system is laminar.

Line 197, “Here, between 12 and 14 LPM of nitrogen is introduced…” so does this mean backgrounds are determined dry? How do the authors correct for analyte sensitivity differences between dry and wet IMR conditions? How long and often are backgrounds determined?

Line 203, “Low pressures…” This sentence and the following seem out of place in the paragraph.

Line 223, “Br_2I^- cluster peak…”, which one?

Line 229, “However, maximizing kinetic energy…” There is this effect but could the increase in field strength also simply be de-clustering the product clusters?

Line 258, “To characterize our t-IMR…” this section should be moved to a separate section in the Methods.

Line 285, “…especially after accounting for the t-IMR residence time of 0.75s” Could you include the Palm et al. low pressure IMR residence time as a comparison point here? My understanding is this was on the order of 50 - 100 ms?

Line 286, “This result is also comparable…” did the authors also do this experiment for HNO_3? It would a nice comparison to the Zhao et al. 2017 paper result.

Line 290, “The t-IMR was deployed…” much of this section could be moved to the Methods section as a “Field Deployment” section.

Line 306, “LOD is also calculated…” Please include a table of these LODs and for what integration time.

Line 315, “binding enthalpies…” Please move these DFT methods to a separate methods section.

Line 326, where did this equation come from? Was a water dependence of the Br2 or HOBr sensitivity considered?

Line 330, “The daily maximum levels of HOBr…” It would be useful to include Br2 and BrO data in this figure and analysis to confirm this measurement. Additionally, isotopologue ratios is normal practice when reporting halogens and should be included in the SI.

Line 336, “However, in this case, the signals are converted to atmospheric concentrations using a direct calibration of the t-IMR after the campaign.” It is not clear why this was done rather than using the same approach as HOBr. Please show the equivalent result with the BE scaling approach. What was the sensitivity for HNO3? How was the water dependence of HNO3 sensitivity determined and applied?

Line 351, “However, there are periods during the night…” The iodide CIMS measures N2O5 and HPMTF very sensitively, if either of these chemistries are at play the authors have data to prove this. Please include this or remove the speculative statement.