

The black color of the text in this document shows the reviewer's comments, while the green color shows the authors' responses, and the revised text is shown in italics.

This is an interesting study on a novel variation of one of the most prominent experimental techniques in atmospheric chemistry, especially aerosol-relevant chemistry. The study is written so well, that even a modeller like myself could follow the discussion, and appreciate the numerous small tweaks the authors have made to improve their setup. I can thus recommend publication essentially as is. I have some very minor comments and questions that the authors can address at their discretion, these are described below.

We thank the reviewer for his/her careful consideration of our article. We have attached a revised version of the manuscript in which we considered all the comments raised by the reviewer.

-line 230, "reagent ions and analyte ions that are very weakly bound to the reagent ions (e.g., water cluster with a binding energy of 42 kcal/mol)". 42 kcal/mol is not a weak binding energy - even the most strongly bound ELVOC*NO₃- clusters are typically bound by less than this. And certainly iodine-formic acids (mentioned as an example of moderate binding a few lines below) is bound by less than this. Perhaps the 42 kcal/mol value refers to the total binding energy of multiple water molecules? Or is there a unit conversion error somewhere? Please explain/elaborate.

The units are indeed incorrect and we apologize for this error. Caldwell et al., 1989 reported value in kJ/mol and not kcal/mol. This is now corrected in the new version of the manuscript.

Lines 233-237: *"In general, reagent ions and analyte ions that are very weakly bound to the reagent ions (e.g., water cluster with a binding energy of 10 kcal/mol) (Caldwell et al., 1989) are often observed to deviate from the thermodynamic distribution with RF amplitudes >50 Vp-p. This is not entirely problematic, as the binding energy of these complexes is usually too weak for sensitive detection even at the weakest transfer conditions."*

-line 385: "(XX%)", please update the actual number here (or explain the notation) - this looks like something that a co-author forgot to fill in during finalising of the manuscript.

This has been updated within the new version of the manuscript:

Lines 387-389: *"With acetonitrile as the dopant, the change in sensitivity across the humidity range is reduced to a deviation of <20% relative to dry conditions for all model compounds, more for formic acid and nitric acid (<10 %)."*

-Line 448: something is grammatically wrong with the formulation of this sentence: "the formation of"... "can react". Maybe reformulate to "The RO₂ radicals generated"... "can further react"?

The sentence has been modified as follows:

Lines 457-458: *"RO₂ radicals generated from the combined ozonolysis and OH radical reaction of α -pinene can further react yielding mixed oxidation products during the reaction time in the flow tube."*

-Line 511: “Differences in the contribution of these compound groups with previous work could be due to different sensitivities of the instruments...”. While certainly true, isn’t it also the case that especially the formation of ELVOC and ULVOC compounds (by a combination of autoxidation and dimer formation) is very sensitive to the experimental conditions (concentrations, residence times, NO_x levels, OH scavenger, temperature etc). So probably some of the differences between the present results and earlier studies are just because of subtle differences in the actual ozonolysis experiment, rather than differences between the instruments? Additionally, maybe explain a bit more in detail what specific “differences in contributions” are meant here?

We agree with the reviewer that product distribution can be greatly impacted by the experimental conditions. A sentence has been added to underline that experimental conditions and systems can alter the OOM distribution.

Lines 519-523: “Differences in the contribution of these compound groups (i.e., relative signal contribution to total OOMs) with previous work could be due to different sensitivities of the instruments towards organic compounds with varying oxidation extents (Riva et al., 2019). In addition, experimental conditions (e.g., RH, temperature, precursor concentration) and setup (flow tube reactor, atmospheric simulation chambers) can greatly impact the distribution of OOMs retrieved by MS techniques.”

-in figure S4 (in the supplementary), it is presumably values of the subscript x (indicating the number of O atoms in the three composition families) that are varied from 1 to 4. It took me a while to understand what the “O=1” etc texts in the figure meant. Please use “x=1” and so on instead, to be consistent with your own notation in the legend.

Figure S4 has been modified as suggested



