

Referee comments for “An electrospray chemical ionization source for real-time measurement of atmospheric organic and inorganic vapors”

## I. General Comments

I recommend for this manuscript to be published in Atmospheric Measurement Techniques after revision. The manuscript is a significant contribution to the atmospheric TOF-CIMS literature providing a new method for generating a broad suite of reagent ions via electrospray. The elimination of radioactive ion sources is of great importance as these instruments are routinely deployed throughout the world. Moreover, the authors do a good job implementing the full suite of available procedures for characterizing the novel chemical ionization source (flow rate considerations, calibration with authentic standards, determination of backgrounds and LODs, high-resolution analysis of a complex mixture, voltage scanning, and multiple reagent ion comparisons). I would also like to applaud the authors for acknowledging that this is a work in progress; off-axis spray configurations, sheath gas flow, the positioning of the emitter to tune the reaction time, curtain gas (or counter) N<sub>2</sub> gas flow are all standard features on modern EIS-MS instruments and should improve this system’s performance, but these considerations are outside the scope of prototype design and proof of concept experiments.

My only general concern with this manuscript is with the introduction. The authors provide good context for why this work is important and where it fits within the development TOF-CIMS methods and applications. It has, however, been known since 1986 that the electrospray of pure solvents are capable of ionizing gas phase species (see C.M. Whitehouse, F. Levin, C.K. Meng, J.B. Fenn, Proceedings of the 34<sup>th</sup> ASMS Conference on Mass Spectrometry and Allied Topics). This technique was formalized by Herbert Hill’s group and termed secondary electrospray ionization (SESI) in 2000. Arguably, the authors have better controlled the chemistry of this ion source by isolating the electrospray plume from the incoming sample making this truly a chemical ionization source and not SESI. Even this claim is not entirely clear as the mechanism of SESI has been studied for years and no clear conclusion has been reached as to whether SESI ionization proceeds via interaction with small droplets, gas phase ions or other phenomenon. With this being the case, the extensive literature and the diversity of instrumental configurations utilizing electrosprays for the detection of gas-phase species and vapors necessitates placing this work in the broader context of the field. I highly encourage the authors to revisit the broader mass spectrometry literature and at a minimum comment on past work with electrospray plumes for detecting gas-phase species.

I have split my specific comments section into two parts: Major Specific Comments and Minor Specific Comments. There are claims in this manuscript that lack evidence, but I think some of these concerns are addressable with available data. My hope is that these Major Specific Comments will only lead to a more convincing publication as the claims about ELVOC dimers have been highly contentious to some critics.

## II. Major Specific Comments

1. **Lines 337-339:** This is not an entirely correct interpretation of clustering and high-resolution peak fitting in the HR-TOF-CIMS. It is absolutely possible to

determine whether ions observed using acetate CIMS are ion-neutral clusters or individual ions via voltage scanning techniques and/or a properly configuring the instrument. This is clearly shown in Brophy and Farmer, 2016, which the authors cite in this section. The HR-TOF-CIMS can be operated such that the probability of observing clusters is ridiculously small, but as Brophy and Farmer point out, operation in this mode may cause fragmentation of high molecular weight ELVOC-like molecules.

At sufficiently high  $m/Q$ , the ability to unambiguously distinguish iodide containing clusters from either non-iodide containing ion-neutral clusters and individual ions also becomes potentially nebulous. While the large negative mass defect of iodide certainly aids high-resolution analysis, it is not a panacea for high-resolution molecular formula assignment and the identification of clusters.

This manuscript deals with many other reagent ions that undergo clustering (meaning the instrument is operated in a cluster-transmitting mode), it would also be worthwhile to discuss the limitations and advantages of  $\text{Li}^+$ ,  $\text{Na}^+$ ,  $\text{K}^+$ , and  $\text{NH}_4^+$  for high-resolution analysis. I would imagine that confident assignment of high-resolution ions would be as problematic for this set of positive ion as running acetate in a cluster-transmitting mode with no voltage scanning experiments.

2. **Lines 423-430:** Brophy and Farmer 2016 performed voltage scanning experiments using multiple adjacent components throughout the atmospheric pressure interface. If clusters are being formed in the first or second quadrupole regions (or during the transitions from one stage to another), then it would not have been possible to eliminate the presence of acetate clusters using adjacent components in the first quadrupole. Additionally, the reagent ion-clusters are most likely ejected from a stable trajectory after an energetic collision. This makes it very unlikely that the reagent ion will 1) re-combine with a neutral species, 2) find its way back into the ion beam or remain in the ion beam and 3) be transmitted through the second segmented quadrupole with a stable enough trajectory to make it to the last portions of the atmospheric pressure interface. This argument as to why a signal increase occurs as the voltage difference is initially increased is highly unlikely if not completely incorrect. The argument presented in lines 422-433 is simple and makes much more sense: ion transmission efficiency in positive mode simply increases a bit with a slight increase in voltage difference for this specific set of components. Slight increases in voltage difference should drive ions into the second quadrupole more efficiently, and since these ion-neutral clusters are quite stable, the increased kinetic energy basically doesn't matter. The observation would probably be different for very weakly bound clusters.

3. **Lines 434-444:** "...the cluster signals for smaller dimers generally decay more slowly than those for large dimers, suggesting these positive ions can more strongly bind to the smaller dimers, likely due to the higher polarity or the smaller steric effect for smaller dimers... This behavior implies these dimers are most likely covalently-bound species... rather than noncovalently-bound species formed during alpha-pinene ozonolysis"

The fact that smaller dimers decay more slowly than large dimers could also be explained with the alternative hypothesis that two smaller monomers (or one monomer and another small oxidation product) are clustered to the reagent ion. In this scenario, the smaller neutrals would also be more strongly bound to the reagent ion for the exact same reasons (less steric effects and higher polarity) than the larger monomers (or other oxidation products) clustered as a 3-body cluster.

This argument can be made more convincingly by showing the monomer declustering scans. Does this same trend hold true for the C-8 through C-10 monomers (which are most like the >C-10 dimers)? Does this same behavior hold true for dimers detected with nitrate CI and iodide CI?

The authors may already have the data to further understand the behavior of single vs multiple neutral-ion clusters. They note earlier in the manuscript (line 349) that they observe  $\text{NO}_3(\text{NaNO}_3)_n^-$ . While this is substantially different from ELVOC/HOM type hydrocarbons, this does present an interesting example of 3-body and higher-order clusters. What do the voltage scans of these clusters look like? What is the  $dV_{50}$  as a function of the number of  $\text{NaNO}_3$  neutrals? Should we expect 3-body clusters consisting of two neutrals to be more or less stable than 2-body ion-neutral clusters?

This is a contentious area of HR-TOF-CIMS and nucleation/cluster chemistry. This is not "direct" evidence that these potential-dimers are strongly bound as we don't really know they are, in fact, covalently bonded molecules given the data presented.

I recommend either substantially reworking this section or providing additional evidence via the largest monomers,  $\text{NO}_3(\text{NaNO}_3)_n^-$  clusters, and behavior of the dimers in iodide and nitrate experiments. Given the amount of additional data I am suggesting here, it may be better to actually remove this claim from this paper as it is outside the scope of characterizing and showing the promise of ESCI. This could be an impactful standalone paper if the additional data are consistent. Further study is likely warranted.

### III. Minor Specific Comments

1. Lines 126-128: Here the author's present "preliminary fluid dynamic simulations" that suggest the flow rates in which the flow profiles remain laminar. Previous work with the Aerodyne/Tofwerk HR-TOF-CIMS have evaluated this problem in the IMR by simply introducing a calibration gas flow until a stable signal is

reached. Then, the calibration gas is shut off and the decay monitored to determine an e-folding time (or similar metric). This is an easy and direct method for understanding the mixing of gases in these reactors. If this source improves the e-folding time over the conventional Aerodyne/Tofwerk IMR design, this ion source could be quite an improvement!

2. Lines 138-146: This section could use some clarification as to how the electrospray solvent is being pushed through the electrospray emitter. Conventional electrospray configurations use syringe pumps or HPLC/UPLC style solvent pumps with online feedback control making the use of a pressure driven solvent flow a bit different. This could, perhaps, be addressed in Figure 1 by showing the pressure controller, electrospray liquid reservoir, and high voltage connections. Additionally, have the authors measured the solvent flow rate? This is commonly used to distinguish between nano-ESI and standard ESI sources.
3. Lines 138-146: I am also curious why the electrospray voltage to reagent ion signal relationship was not investigated? The electrospray voltage is typically a key parameter and is more complex in this source; the authors point this out by addressing that the ion drift/reaction time depends on the voltage difference between the emitter and the inlet capillary.
4. Lines 165-170: The use of methanol and salts is an interesting choice of reagent ion precursors. Have the authors considered the possibility or looked for any evidence of methanol/protonated-methanol clustering? Additionally, other solvents could be used such as acetonitrile, which typically exhibits less clustering effects in standard electrospray ion sources.
5. Lines 268-273: Here the authors address the differences in calibrations between atmospheric pressure ESCIMS and low-pressure CIMS. They attribute the differences to ion source configuration, pressure and ion-optics voltage settings. Tofwerk's Thuner voltage optimization tool would be a great way to ensure that the voltage configurations are optimized across sources. This also appears later in the manuscript where the authors comment on the differences between positive and negative ion modes.
6. Line 328-329: why was the 5 cps criteria chosen for the high-resolution fitting cutoff? Does this correspond to some significant signal-to-noise ratio? Eyeball test? Some justification of this number is required as various other recommendations have been suggested in the HR-TOF-CIMS literature for what constitutes a significant signal.
7. Lines 348-350: The presence of  $I(NaI)^-$  and  $NO_3(NaNO_3)_n^-$  suggests that the reagent ions generated from this ESCI source are not identical to the reagent ions generated from conventional Po-210 based methods. This may explain why the mass spectrum obtained for alpha-pinene ozonolysis are similar but not identical to Lopez-Hilfiker et al., 2015 results.
8. Lines 368-371: It might be worth adding a figure to better illustrate the difference between these various reagent ions. For example, a histogram of O:C or oxidation state by high-resolution assignment could help to highlight these differences. This

provides a great opportunity to further show that reagent ions are compound-class specific and parallels the previous work of Aljawhary et al., 2013. I think that bulk metrics (i.e. average oxygen:carbon ratio) would be less informative.

#### IV. Technical Corrections

1. Line 85: remove double “..”
2. Line 116: please specify the manufacturer/model if this is a commercial silica spray needle or simply a piece of fused silica tubing
3. Line 126: what type of pump was used to back the IMR? Was the flow controlled via mass flow controller or some other device?
4. Line 139: the electrospray solvent reservoir is biased between 3-5 kV. Should this read something like “+/- 3-5 kV depending on the ion mode”? Typical electrospray systems require different voltage magnitudes depending on the ion mode (positive vs negative) with negative mode typically requiring higher negative potentials. This would be useful information.
5. Similar to item 2 in the specific comments section: It would be useful to show the electrospray solvent reservoir, pressure controller, and HV connections in Figure 1 as the experimental configuration is not totally clear.
6. Figure 2a appears to have error bars while figure 2b does not.
7. Figure 3’s caption reads “normalized sensitivity” while the y-axis reads signal in counts per second. Should the units be normalized counts per second (ncps) and/or “normalized signal”?