

# ***Interactive comment on “Evaluation of the new capture vaporizer for Aerosol Mass Spectrometers (AMS) through laboratory studies of inorganic species” by Weiwei Hu et al.***

**Anonymous Referee #1**

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In their manuscript “Evaluation of the new capture vaporizer for Aerosol Mass Spectrometers (AMS) through laboratory studies of inorganic species”, Weiwei Hu and co-authors present results from extensive characterization measurements with the new capture vaporizer for the Aerosol Mass Spectrometers. The capture vaporizer (CV) is designed to reduce losses by particles bouncing off the AMS vaporizer. This is achieved by its geometry which helps directing bounced particles into a cavity where they have several additional chances to interact with the hot vaporizer surface. In their manuscript the authors investigate the impact of this vaporizer on thermal decomposition of inorganic aerosol components and associated changes in fragmentation patterns, on collection efficiency for various inorganic aerosol types, and on the size distribu-

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tion measurement capabilities for such aerosols. In addition the influences of particle beam position on the vaporizer and of vaporizer temperature on such effects are studied. The development of the capture vaporizer is an important step towards more precise aerosol mass concentration measurements with instruments from the AMS family. For reliable measurements applying this vaporizer the potential influences of this device onto various features of the measurements (e.g. quantification or size distribution measurements) need to be well known. Therefore I strongly recommend publication of such investigations and I think Atmospheric Measurement Techniques is an adequate journal for this purpose. A large variety of systematic measurements has been performed thoroughly and provided important information on the performance of the capture vaporizer. The authors have shown how the capture vaporizer increases fragmentation of several inorganic molecules and the collection efficiency of some aerosol types. Furthermore, with the capture vaporizer measured particle size distributions are broadened. While the measurements and data analysis seems to be performed carefully and systematically, the interpretation and the presentation of the results are not satisfying. Several of the conclusions or interpretations of the observations contradict each other or do not completely agree with the observed trends in the data. Some of the conclusions seem to over-interpret the observations and include results from other measurements (from future publications) to end up with statements that go beyond the observations of this study. In many places the text is written rather lax, more resembling laboratory slang than a scientific publication. I have the impression that the text is rather long for the amount of information conveyed and could be shortened somewhat without losing information. I find it rather unsatisfying that here already the second paper on the new AMS capture vaporizer is presented and still many important observations are shifted to the next paper on this device. Why have not internal mixtures of different inorganic and/or inorganic and organic species have been investigated but only statements like that for mixtures of species one would expect better performance are made? I strongly support the publication of this important work. However, I suggest that the authors shorten the manuscript by leaving out repetitive information. The

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wording should be improved to avoid “laboratory slang” (see comments below). Conclusions which are contradicting each other or which are not in agreement with the measurement results should be avoided (see comments below).

Detailed comments: Abstract: The abstract is pretty long. The first paragraph of the abstract leaves the impression the CV is the solution to the CE/bounce issue of the AMS. This provides a too positive picture of the capabilities of the CV as the second paragraph shows: CE is still not 1.0 for all species; size distributions have lower size resolution; stronger fragmentation is observed and no information about organics is provided – can the AMS still be used to identify different types of organics? I suggest shortening the abstract and providing the most important findings here without over-interpreting the results. It should be avoided to include references in the abstract.

L18: This sounds as if CE is a function of measurement location, air mass, or season. It could also vary with time of the day, ambient temperature, and other external factors. At the end, CE depends on particle composition and humidity.

L22: Particle bounce is not “eliminated” as stated here. Particles still bounce off the vaporizer surface however, they are directed into a cavity and have more chances to interact with the vaporizer and to vaporize.

L35/36: Since the influence of the CV onto ambient size distributions and also onto organic particles was not studied in this work about this no speculations should be made in the abstract and presented as if this were results.

L49: The term “aerosol mass spectrometers” is not a synonym for the Aerodyne AMS as suggested here.

L85: What is the “differential mobility analyzer (DMA)-impactor technique”? – Explain.

L86: What do you mean with different “vaporizer surfaces”?

L93: Shouldn’t “Aerodyne” be “Aerodyne Res., Inc.”? The reference (Jayne and Worsnop, 2016) is unclear.

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L98: Add “as” after “distributions”.

L103: “production of CO<sub>2</sub> in the vaporizer while sampling nitrate” – without the information that this CO<sub>2</sub> is from previous deposits this sentence does not make sense to the readers.

L105: For the measurements presented here (e.g. alignment/misalignment of the particle beam, size distribution measurements) it would be good to have some information on the dimensions of the CV.

L117: According to Fig. 1 a Nafion dryer or a silica gel dryer were used.

L119-121: The authors state that multiply-charged particles were removed using the inlet impactor of the TSI DMA. According to the TSI 3080 manual, different impactor nozzles are used for different flow rate ranges. However, no information on impactor cut-off at certain flow rates is provided. Where are the cut-off diameters for these impactor nozzles and how well do they remove the multiply charged particles? I assume that there is no perfect removal of all multiply charged particles using these impactor nozzles. How large is the uncertainty due to the remaining particles? According to the TSI 3080 manual all three nozzle diameters provided in the text are wrong (should be 0.071 cm instead of 0.0701 cm; 0.0508 cm instead of 0.0580 cm; 0.0457 cm instead of 0.0485 cm).

L135-137: Did all instruments used for these experiments have the same chamber length?

L148 (and many others): Terms like “CPC methods” (methods based on CPC measurements), “particle sizes with 90% transmission …” (L152, particles of sizes that have 90% transmission …), “organic interference at several inorganic ions” (L269, organic interferences at several m/z associated with inorganic ions), “such hysteresis behavior supports that changes …” (L329, such hysteresis behavior supports the assumption that changes …), “… similar to Drewnick et al. (2015)” (L336, … similar to

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observations by Drewnick et al. (2016)), “The measured PToF times represent both the actual (size dependent) particle velocity plus the vaporization and detection process” (L401-402, the PToF times are the sum of particle travel time and time for vaporization and detection, the measured times cannot represent a velocity plus a process), “At the edge of the CV, all ions … while in the center …” (L403, When directing the particle beam onto the edge of the CV, all ions … while when the beam was directed towards the center …), “AMS/CPC ratios” (L458 and many others, the ratio of a mass concentration and a number concentration is not CE but it would have the dimension g  $((\mu\text{g}/\text{m}^3)/(1/\text{cm}^3))$ , it should be something like the ratio of AMS/CPC-derived mass concentrations), “several secs” (L489, several seconds) are rather laboratory slang than adequate wording of a scientific publication.

L151 (and many others): Consistently “evaporation” is used in this text to describe the phase transformation on the vaporizers. “evaporation” is a phase transformation from the liquid into the vapor phase that occurs at a temperature below the boiling temperature at the respective pressure. This is definitely not what happens on the vaporizer. Therefore, “vaporization” should be used throughout the text.

L155: Replace “event” by “events”.

L171-173: Is there any information or estimate about the temperature distribution in the CV or about the temperature of the front end of the CV?

L173: Shouldn’t be “outside out” rather “outside of”?

L180: What exactly means “starts to broaden”, how is this start of the broadening of the size distribution defined? How large is the uncertainty of this definition and how large is the uncertainty of a partially detached thermocouple?

L189-191: How can the temperature readout deliver power to the vaporizer? This sentence does not make sense for me.

L203-207: A “Jayne shape factor” of 0.8 was applied to NH<sub>4</sub>NO<sub>3</sub> to correct for the

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fact that the effective density of these particles is less than the bulk density. Is there any explanation why the effective density of ammonium nitrate particles, which are assumed to be liquid and thus droplets, is less than the bulk density? If there is a difference in the effective density and the bulk density for the NH<sub>4</sub>NO<sub>3</sub> particles, this could also be the case for the other substances. How large is a potential bias or the uncertainty of the results due to this?

L210: I find this section rather long for the information conveyed.

L216: Is Figure 3 really necessary? There is a very large number of figures in this manuscript and it would be desirable to reduce this number.

L236: Replace “increased collisions” with “increased number of collisions”.

L247: Here, for comparison ion ratios for “standard EI” are mentioned. This is a bit confusing/misleading since not the ionization process (EI) is different in the AMS but the vaporization is different and occurs at higher temperatures.

L255: I suggest replacing “also” with “therefore”.

L261-262: Is there any explanation why RIE(SO<sub>4</sub>) is larger for the CV compared to the SV?

L265: I suggest replacing “recommended fragmentation table” with “recommended changes of the fragmentation table” since only the changes are presented in Table 1, not the whole fragmentation table.

L269: Here it is stated that “the fragmentation of organics is also different in the CV”. This was not shown in the experiments presented in this manuscript and is not published. If this is an assumption this should be made clear.

L285-287: The statement that for the CV the NO<sub>2</sub><sup>+</sup>/NO<sup>+</sup> ratio showed less dependence on temperature than for the SV is not correct. According to the numbers provided here, the ratio changes by 62.5% for the CV while it changes by ~40% only for the SV. So

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the dependence is even larger for the CV than for the SV.

L291-293: The boiling points are relevant for ambient pressure only; under the conditions on the vaporizer boiling will start at much lower temperatures. May be this should be mentioned here. “substantially are higher” should be replaced with “substantially higher”. “In SV . . .” should be replaced with “In the SV . . .”.

L299-301: Also here it is claimed that the NO<sub>2</sub>+/NO<sub>+</sub> ratios show less dependence on T<sub>v</sub> for the CV than for the SV. While it is true that the values of this ratio are much lower for the CV than for the SV, the change with changing T<sub>v</sub> is a factor of 20 for the SV while it is a factor of 60 for the CV. So the dependence on T<sub>v</sub> is also in this case larger for the CV than for the SV, contrary to what is claimed in the text. In addition, the numbers provided in the text for the ratios observed for the SV (0.01-0.03) do not agree with the data presented in Figure 4b (rather 0.005 – 0.095).

L305: Here, it should be stated what is thermally decomposed to avoid confusion.

L306-307: From Figure 4c it does not look like that there is a change in SO<sub>x</sub>+/SO<sub>+</sub> ratios for T<sub>v</sub><300°C only and then the ratio levels off, as stated in the text. It rather looks like that for higher T<sub>v</sub> the SO<sub>2</sub>/SO ratio decreases again and the SO<sub>3</sub>/SO ratio seems to increase.

L319: Replace “particle” with “particles”.

L320: Remove the first “from”.

L323-325: It is unclear to me how the variations in the Cl/HCl ratio are due to the stickiness of HCl on the vaporizer surface.

L332-333: Replace “vaporization surfaces” with “vaporizer surfaces”.

L356: Replace “. . . pure NH<sub>4</sub>NO<sub>3</sub> since . . .” with “. . . pure NH<sub>4</sub>NO<sub>3</sub> particles since . . .”

L360: I suggest replacing “movement” with “experiment”.

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L363-364: Replace “... cross section of vaporizer ...” with “... cross section of the vaporizer ...” and “vertical directions” with “vertical movements”.

L371: Move “( $\sim 0.07$ )” after “low  $\text{NO}_2^+/\text{NO}^+$ ”.

L373: There are several data points in Figure 6 which show  $\text{NO}_2^+/\text{NO}^+$  ratios for the SV below the range provided here (0.29-0.75). These low values could be associated with very low signal intensities, therefore error bars or a discussion of the uncertainties is necessary.

L375: “... when the particle beam hits the CV edge, as illustrated in Fig. 1a.” This is not illustrated in Figure 1a.

L379-386: As a potential reason for higher  $\text{NO}_3^-$  signal when the particle beam is directed towards the edge of the CV, reduced fragmentation and higher  $\text{HNO}_3(\text{g})$  fraction is mentioned. Another possible reason could be lower vapor molecule velocity due to the lower temperature at the front end of the CV and consequently longer residence time of the molecules in the electron beam.

L386: “... on the right edge vs left edge ...” should read “... on the left edge vs right edge ...”

L390-391: Here it is claimed that in the SV no enhanced nitrate signal was observed on the vaporizer edge. When looking at Figure 6 it looks like there is a small enhancement of nitrate signal at both edges of the vaporizer. However, without error bars it is hard to judge whether this is significant.

L390-393: While here, differences in  $\text{NO}_2^+/\text{NO}^+$  ratio between SV vaporizer center and edge are discussed, the clear trend of this ratio across the vaporizer is not discussed. What is the reason for this trend?

L396-397: “Slightly lower RIE( $\text{NH}_4^+$ ) are observed at the edges of the CV due to higher nitrate signal and constant  $\text{NH}_4^+$  signal.” The higher nitrate signal and constant  $\text{NH}_4^+$  signal is not the reason for the lower RIE for ammonium (as suggested by this sentence)

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– this is just the observation. What is the reason?

L397-398: The authors claim that there is no systematic difference of RIE(NH4) between center and edges. If I compare Figure 5 and Figure 6 I would say that the dip of RIE(NH4) at the edge of the CV (Figure 5), which was claimed to be real, is of similar magnitude as the trend of RIE(NH4) across the SV (Figure 6). Without a proper discussion of the uncertainties all this is highly subjective.

L407: "Fig. 7a" must be "Fig. 7c or d".

L411-412: Not the measured PToF times are narrow (as written) but the measured PToF time distributions.

L420: Here the authors suggest that for correction of the CO2 signal that is produced on the surface of the vaporizer the fragmentation table should be adjusted. I doubt that this is possible. As the authors show in their manuscript the CO2 generated on the surface of the vaporizer depends on the history of the vaporizer (i.e. on the material that was previously deposited on the vaporizer) and thus is potentially highly variable.

L424: Were NH4NO3 particles sampled in air or in argon for these experiments?

L432-437: Two different reasons for negligible CO2 formation in the CV (compared to the SV) when sampling ammonium nitrate particles are provided: Different thermal decomposition pathways between the vaporizers (mainly NO(g) produced in the CV while in the SV also NO2(g) is produced) and different vaporizer materials (molybdenum in the CV which is more inert than tungsten used in the SV). For me the first reason is not very convincing: in the CV, thermal decomposition into NO is the consequence of more interaction of the material with the vaporizer due to the trapping in the cavity. During the first interaction with the vaporizer I would expect that thermal decomposition is similar to that observed on the SV with similar fractions of NO2(g) as in the SV. This NO2 should then result in similar production of CO2 from material on the vaporizer surface as in the SV.

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L437: I suggest replacing "... based on lens alignment ..." with "... dependent on lens position ...".

L439: The CO<sub>2</sub>+/NO<sub>3</sub> ratios are a little higher at the vaporizer edge. Is this difference significant? There is very low signal intensity, likely associated with high uncertainty.

L447-449: Here the authors explain the CO<sub>2</sub>+ artifact in the CV from NaNO<sub>3</sub> (but not from NH<sub>4</sub>NO<sub>3</sub>) by the substantial NO<sub>2</sub>(g) formation from NaNO<sub>3</sub>. This is a contradiction to what was shown in section 3.1.2 where the authors show that NaNO<sub>3</sub> produces much less NO<sub>2</sub> compared to NH<sub>4</sub>NO<sub>3</sub>. According to this the CO<sub>2</sub>+ artifact should behave the opposite way. Also the related statement about T<sub>v</sub> dependence of the CO<sub>2</sub>+ artifact contradicts the observations in 3.1.2 and Figure 4a/b: also at low T<sub>v</sub> the NO<sub>2</sub>+/NO<sub>+</sub> ratio is about 10 times lower for NaNO<sub>3</sub> compared to NH<sub>4</sub>NO<sub>3</sub>.

L453-469: Vaporizer-temperature dependent collection efficiencies for the CV determined by comparing the calculated mass concentrations from CPC measurements with AMS mass concentrations should show a SQRT(M<sub>w</sub>) dependence as predicted by Murphy (2016) since the vapor molecules emitted from the vaporizer should be in good thermal equilibrium with the vaporizer after leaving the cavity. Was this observed in the measurements?

L465: Why were 300 nm NaNO<sub>3</sub> particles used for the measurements and a (relatively large) correction factor applied to correct for incomplete lens transmission instead of using 250 nm NaNO<sub>3</sub> particles? I would expect that this would result in much lower uncertainties in the results.

L471: If it is really 1+-0.07 then you should write "1.00+-0.07".

L475: I disagree with the statement that the AMS/CPC nitrate mass ratios for NH<sub>4</sub>NO<sub>3</sub> did not show a clear trend with T<sub>v</sub> for the SV. According to Figure 9a1 there is also for the SV a clear maximum around 400-450 °C.

L479-481: If the smaller AMS/CPC mass ratio for the CV for very low T<sub>v</sub> is due to

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slow vaporization this should also be the case for the SV where vaporization occurs at the same temperature. However, for the SV no such strong decrease in the ratio is observed for very low  $T_v$ .

L482-485: Why should the interaction of the analytes with the vaporizer surface be stronger at hot surfaces? I would expect the opposite behavior.

L494: How is tau defined? In Drewnick et al. 2015 it was discussed that the decrease or increase of the signal could not be described with a single time constant but the time constants changed over time.

L498-500: Why is the decay of the nitrate signal faster for the CV than for the SV? Due to the geometry of the two vaporizers the opposite behavior would be expected. What is the uncertainty of these results?

L500-501: The sentence starting with “Slower decays . . .” makes no sense to me.

L504-505: Here vaporization processes and the residence times in the two vaporizer types are mixed together. It is not slow vaporization which causes the nitrate signal to decay slower in the CV but the times the vapor needs to escape the vaporizers. The vaporization process itself should not have different time constants for the two vaporizer geometries since it occurs at the same temperature.

L506-510: I find it counter-intuitive that at higher  $T_v$  stronger interactions between the decomposition products from nitrate and the vaporizer surfaces should occur. I would expect the opposite. Isn't it possible that at higher vaporizer temperatures also the surrounding ionizer cage is hotter resulting in more desorption of material and consequently in larger background signals?

L517: The AMS/CPC mass ratio of  $\sim 0.85$  is the result of several assumptions or corrections (RIE for  $\text{NO}_3$  from  $\text{NaNO}_3$ , Jayne shape factor = 1, lens transmission at the respective particle size, negligible fraction of doubly charged particles from the DMA). How uncertain is the resulting CE (i.e. mass ratio)?

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L519: The provided vaporizer temperatures (300°C for CV and 500°C for SV) are not the temperatures where the AMS/CPC mass ratio increases (as stated in the text) but where the maximum or plateau is reached.

L522: What does the “right” in the parenthesis mean?

L522-524: Also for this species faster decay of the signal when the particle beam is closed is observed for the CV compared to the SV. It makes no sense that this is due to faster vaporization as stated further above. Could it be that the SV scatters particles onto nearby surfaces of the ion source which are sufficiently hot to slowly vaporize the material while the CV can only emit particles in the direction from where the particles came from?

L524-526: Here it is stated that the plateau of AMS/CPC mass ratio for NaNO<sub>3</sub> being less than 1 in the CV is probably due to the uncertainty of the particle lens transmission loss correction. Of course this could be the case but this is not a strong argument. The fact that the mass ratio exceeds 1 significantly for lower  $T_v$  points rather towards a correction which is too large instead of too small. In addition, there is no significant difference in the ratios for CV and SV for large  $T_v$ . So if the correction factor needs to be larger this would result in  $CE=1$  also for the SV for this particle size. To avoid this uncertainty, it would be desirable to perform these measurements with smaller particles and not to apply a correction for lens transmission.

L533: Na has a melting point of 98°C and a boiling point (at ambient pressure) of 890°C. I would not call this a “refractory species”. In addition, NaO should probably be Na<sub>2</sub>O.

L545: Does the word “reproducibly” mean that these measurements were performed multiple times or does it mean that there is no  $T_v$ -dependence in this temperature range? I would disagree with the latter, there is a clear maximum visible around  $T_v=500^\circ\text{C}$ . In addition: “Fig. 11” should be “Fig. 9”.

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L549: Replace “... inside CV cavity ...” with “... inside the CV cavity ...”.

L550: Collection efficiency is compared for the CV (0.7-0.8) and the SV (0.2-0.55). This is an unfair comparison since different Tv-ranges were used for comparison. If the same ranges were used it would be either CV: 0.05 – 0.8, SV: 0.2 – 0.55 or CV: 0.7 – 0.8, SV: 0.45 – 0.55.

L550-554: Again the faster decay of the signal (here for SO<sub>4</sub>) only makes sense if the origin of the signal is from cooler surfaces like from the ionizer surface.

L555: The decrease of AMS/CPC mass ratio is not observed for  $Tv > 700^{\circ}\text{C}$  only but starts already around 550 °C.

L557: Remove “the” in “due to the particle bounce”.

L563-574: If the high background signal and the long response times for the SV are due to slow vaporization it makes no sense that for the same  $Tv$  both is improved in the CV, even though the temperature is the same (no faster vaporization) and the geometry would probably cause an opposite effect.

L579-584: The low NH<sub>4</sub> background and short decay lifetimes contradict the assumption of slow vaporization of some of the substances. It is unclear to me how the anions could vaporize slower than the cations. There must be another effect affecting the anions (e.g. interaction with the surface of the vaporizer) to explain this behavior.

L586: Replace “... decay lifetime (<2s) at medium  $Tv$  ...” with “... decay lifetime (<2s) as at medium  $Tv$  ...”.

L587-588: The decay lifetimes of the various substances cannot be explained by their vaporization time constants (then the NH<sub>4</sub>NO<sub>3</sub> should have shorter lifetimes than the others) or by particle bounce (then the sulfate which bounces deeper into the cavity of the vaporizer should have longer lifetimes than the nitrate-related NH<sub>4</sub>). There must be another effect causing this behavior.

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L596-607: Wouldn't one also expect a variation of the NH<sub>3</sub> fragmentation pattern as a consequence of more thermal decomposition in the CV, similar to what is found for the anions? If actually intact salt molecules would be vaporized (as suggested by Murphy, 2016), why would one expect a variation in fragmentation patterns? In all cases fragmentation would probably occur then in the electron cloud which does not differ for different salts. However, another argument against the suggestion by Murphy, 2016 could be added: As shown in Figure 9 there is a completely different behavior observed for the anions and NH<sub>4</sub> (e.g. Figure 9a3, c3, d3) when T<sub>v</sub> or the vaporizer type changes. This shows quite clearly that the separation of NH<sub>4</sub> and the anions already occurs on the vaporizer and not in the ionization process.

L609: The “~” is written as subscript.

L621: How are the rise times defined?

L624-625: The authors state that although lower sizing resolution are obtained with the CV, size distributions can still be measured with this vaporizer. The authors should discuss how the slower escape of the vapor from the CV affects the measured particle diameters (not only the width of the distribution).

L641-642: The references to the panels on Figure 13 are wrong.

L646: The transition temperature is not the “T<sub>v</sub> above which particles show a narrow distribution” but the T<sub>v</sub> above which the measured particle distribution is not broadened by vaporization effects.

L650-651: Replace “... similar to the reported melting point trends ...” with “... similar to the order of reported melting points ...”.

L655-657: The explanation given for the lower transition temperature for NaNO<sub>3</sub> in the CV, compared to the SV (due to longer residence time and more collisions between particle and vaporizer surface in the CV compared to SV) does not make sense: At a temperature below the transition temperature particles on the SV can either vaporize

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slower or bounce. Particles that vaporize slower would also vaporize slower on the surface of the CV which is at the same temperature. This should not cause any difference in the width of the size distributions. If at all, the size distribution measured with the CV should be broader due to the geometry of the CV. Particles that bounce off the vaporizer do not contribute to the measured size distribution and thus cannot broaden the size distribution. Therefore also here a possible explanation of the behavior is bounce of particles off the SV onto nearby relatively warm ionizer surfaces from where they desorb more slowly. This could also explain the broadening of the size distributions for very large  $T_v$  (L667-669).

L660: I would be careful calling it a “linear relationship” what is presented in Figure 14: For  $T_m$  there is rather a square relationship;  $T_b$  is the temperature at which the vapor pressure reaches ambient pressure. This is not needed for boiling under the conditions found on the vaporizer.

L671: Replace “decomposition rates” with “vaporization and decomposition rates”.

L675: In the SV the peak widths are all even within 0.5 ms.

L682: “Figure 13c1-2” should be “Figure 13b1-2”.

L682-683: There is no data on ambient size distribution measurements and therefore I do not think it is adequate to draw conclusions about the applicability of the CV for such measurements. In addition, no discussion is provided on how the CV has an impact on the sizing of the aerosol, by delayed measurement of the particles.

L686-687: I wonder how the PToF/MS mode signal ratio can provide quantitative information on vaporization rates.

L693: “Fig. 13c3” should be “Fig. 13a3”.

L696: “Figs. 13c1-c3” should be “Figs. 13b1-b3”.

L702: “reduced” is not adequate here. Fragmentation at 500-550 °C is not reduced

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(compared to SV measurements) but only less increased (compared to higher  $T_v$ ).

L725: The bounce fraction is not reduced in the CV (the particles still bounce, but they bounce into the cavity), only the fraction of particles lost due to bounce is reduced.

L725-729: This is speculation and should not be in the conclusions.

L734-735: This shows that the numbers provided in this paragraph are highly dependent on the vaporizer history.

L736: "... should much smaller than for the SV, ..." should be replaced by "... should be much smaller for the CV than for the SV, ...".

Conclusion: I am missing a fair and critical assessment of the CV in this final section. The conclusion sounds too positive to me and does not sufficiently include the limitations and drawbacks of the CV.

Table 1: In the headline "Ions" should be "Ion" and "Parent ions" should be "Parent ion". Some of the numbers in the table have too many digits. E.g.,  $33.19 \pm 0.53$  is rather  $33.2 \pm 0.5$ .

Table 2: I suggest replacing "Fragmentation table ..." with "Changes of the fragmentation table ...".

Figure 1: "SMPS" should be "DMA". It would be helpful if some dimensions would be added in panel a).

Figure 2-15: Units should be provided after a "/", not in parentheses.

Figure 2: Add "(FWHM)" after "Peak width" in the Figure caption. Replace "... which is a transition ..." with "... where a transition ... occurs ...".

Figure 4: Shown are ion ratios not "fragmentation patterns" as stated in the caption. Error bars are standard deviations of what? The different line types used for the  $\text{NH}_x/\text{NH}_x$ ,total ratios cannot be seen in the graphs; in the legend there is twice

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"NH<sub>2</sub>+/NH<sub>x</sub>,total".

Figure 5 and 6: Not only NO<sub>2</sub>+/NO<sub>+</sub> is shown in the graphs (as stated in the caption) but also NH<sub>2</sub>/NH and NH<sub>3</sub>/NH. Replace "hit" with "hits" in L806/814 and "particle" with "particles" in L807/815. In L806/813 "a" should be added between "are" and "rough". Why are data points missing in Figure 6 for NH<sub>x</sub>/NH on both ends of the graph?

Figure 7: The position data referenced in the caption do not mean much if no reference is given. What does "double charged 300 nm particles selected by DMA" mean?

Figure 8: Replace "... period as shown Fig. 4a ..." with "... period as those shown in Fig. 4a ...".

Figure 9: What does "... from four dry monodisperse particles ..." mean? Have only four particles been measured? "were corrected with the lens transmission curve" is laboratory slang.

Figure 10 and 11: Add units to the time axes. Is it seconds or minutes? In L860 replace "T" with "tau".

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2016-337, 2016.

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