Referee comments are in black. Our responses are in blue.

The authors appreciate the comments made by the three referees. These comments, especially by Referee #1 and #3, motivated us to review the entrainment and scavenging efficiency calculations in terms of both the averages and uncertainties. As a result, there are several minor updates to the numbers presented in Tables S2 and S3. However, these changes do not affect the conclusions drawn in the submitted manuscript. Further, the comments posed by all three referees have strengthened the conclusions of the paper.

Anonymous Referee #3, 27 Mar 2026

The article "Aerosol Scavenging in DC3 and SEAC4 RS Deep Convective Storms" by Barth et al., interpret scavenging efficiencies of speciated mass concentrations of aerosols for investigations performed during DC3 and SEAC4RS field experiments on different storms. The studies are in line with the investigations performed by the research group. The results highlight the importance of multi and intercorrelation of dynamics, physics, and chemistry used to interpret processes occurring during thunderstorms. This study add important information on more robust quantification of aerosol scavenging by studying more convective storm than existing research which was limited to a maximum of 3 storms.

Major comments:

Please add information about the dependency on pressure of instrument calibrations. Aircraft measurements handle with vertical distributions and an accurate measurement needs knowledge on the influence of pressure and temperature on measurements. If the instruments were ground base calibrated and measurements were performed at high altitude it could affect the measurements, please comment.

We appreciate the question, it is a legitimate concern. However, while obviously varying ambient pressure is a major challenge in the design of airborne in-situ instrumentation, calibration considerations is just one aspect to take into account. For aerosol measurements in particular, altitude-dependent inlet losses (e.g. McNaughton et al., 2007) and inlet artifacts due to impaction/resuspension (Murphy et al., 2004) are certainly more important to overall quantification.

For the instrument used in this study (customized Aerodyne HR-AMS), a combination of a NCAR HIMIL inlet (Stith et al., 2009) outside the plane and a pressure controlled inlet/interface inside the plane (Bahreini et al., 2008, Guo et al., 2021, Kim et al., 2025) is used to sample the aerosol and analyze it under constant instrument conditions. Hence the calibrations are performed on the ground post-flight. For DC3, these calibrations were very stable (<7% for both absolute and relative sensitivities), showing consistent performance downstream of the inlet.

Both Guo et al., (2021) and Kim et al., (2025) discuss the in-depth characterization of the losses in the pressure interface and the inlet plumbing.

The comparisons of the AMS measurements as installed on the NASA DC-8 with other sensors using very different measurement principles and inlets show a consistent performance regardless of altitude (deCarlo et al., 2008); Cubison et al., 2011; Nault et al., 2018; Brock et al., 2019). Since such comparisons have not been published previously for the DC3 campaign, we now include them in the revised paper (L169-178; Figs. S7-S10). The new figures document not only the campaign-wide comparisons of AMS reported species with other sensors, but also comparisons of aerosol extinction and aerosol volume. They all show a consistent aerosol dataset with high correlations and slopes, with the exception of the absolute magnitude of the reported physical volume from the UHSAS instrument, which likely has a 40% low bias. Figures S8 and S9 specifically look at the Referee's concern about the altitude dependence for the aircraft campaign wide (Fig. S8) and the specific inflow and outflow periods (Fig. S9).

We think that the agreement shown addresses Referee #3's concerns. They should also serve as an additional confirmation of the 17 and 19% stated accuracy figures used in the uncertainty propagation calculation requested by Referee #1.

Even if the study consider zero initial concentration of organic acids it is evident that these concentrations can not be zero. Please comment based on the existing field measurements studies which stated various concentrations of organic acids in the atmosphere, how the model could be affected.

We believe the reviewer is referring to the cloud chemistry parcel model calculations used to determine if aqueous-phase chemistry in the cloud water is contributing to OA concentrations in the outflow region of the storm. We reviewed the literature to find typical values of organic acids that were initialized to zero in the cloud chemistry parcel model. Nah et al. (2018) measured gas-phase organic acids at a rural site in Georgia during September-October 2016. Since these measurements are from the southeastern US and during the same time of year as the SEAC^{4}RS measurements, it seems reasonable to use their findings as guidance here. Their average diurnal profile for the Georgia campaign revealed mid-day formic, acetic, and oxalic acid mixing ratios of 1.3 ppbv, 0.6 ppbv, and 10 pptv, respectively. Glycolic, glyoxalic, and pyruvic acids were not explicitly reported. Adjusting only the organic acid initial mixing ratios to those that were measured and 10 pptv for glycolic, glyoxalic, and pyruvic acids, the cloud chemistry parcel model was integrated again. The results show higher oxalic acid and SOA mass at cloud top compared to the original simulation (which used initial mixing ratios of 565 pptv formic acid, 224 pptv acetic acid, 0 pptv for other organic acids). That is, cloud top oxalic acid as SOA increased from 0.46 µg C std m^{-3} in the original simulation to 0.78 µg C std m^{-3} with the new initial concentrations and total SOA mass at cloud top increased from 2.39 µg C std m^{-3} to 2.58 µg C std m^{-3}. This information is added to the manuscript (L597-600), noting that the numbers

reported in the submitted paper have been adjusted to report results from the no lightning-NO_x simulation.

Figure 5S represents the scatter plot of pNO3 and the NO3 - fraction of NO3 - + HNO3 for 6 different cases. Excluding the extreme purple dot the outflow linearity change drastically and data became more representative in terms of linearity. Please discuss how this potential exclusion would affect the results.

We believe the reviewer is referring to the purple point where pNO_3 = 0.42 μg m^{-3} and NO_3^- fraction is 0.82. The reviewer is correct in that the linearity of the outflow points increases (r^2 value of 0.95 without the outlier). Excluding this point suggests that pNO_3 would contain 100% NO_3^- at pNO_3 concentrations of 0.26 or greater. However, this does not affect the conclusions that a) the outflow NO_3^- fraction is higher than the inflow NO_3^- fraction or b) the points where pNO_3 concentrations are greater in the outflow region than the inflow region have NO_3^- fractions > 0.8. Since the conclusions of the paragraph in the manuscript (L470-481) are the same, no changes have been made to the text.

May you better explain why the aerosols scavenging efficiency differ for all of them (sulfate, ammonium, etc.) during the 18 May 2012 and 2 June 2012 events? It could be an effect of experiment failure?

As discussed above, we do not think the 18 May and 2 June 2012 scavenging efficiencies are a result of experimental failure. Reasons are already discussed in the manuscript. For example, comments about the effect of the mid-troposphere aerosol layer on scavenging efficiency is mentioned for each aerosol component. Further, we mention that SO_4^{2-} scavenging efficiencies may be affected by aqueous-phase chemistry. Likewise, aqueous-phase chemistry can affect OA scavenging efficiencies.

Minor comments:

Line 129: better add „using i- to n- ratios of butane and pentane isomers”

Following the reviewer’s suggestion, this has been changed.

Line 148: Faloona et al., 2004 is missing from reference list. Please check all the references in this sub-section. Most probably references from Supplementary information should be added in the list of main article

The Faloona et al. (2004) paper has been added to the reference list and we have proofread the paper again to ensure all citations are in the reference list.

The ACP guidance in the author instructions for the journal is not clear where to place references from the supplementary information. We will follow the guidance of the editor.

Line 379-381: please revise the sentence

It is not clear what the issue is with this particular sentence. It has not been changed.

Line 389: Isn't there „West"?

Changed

Line 542: „hydroxyacetone"

Line 542 writes hydroxyacetone, which, in the literature, is an accepted way to write this compound.

Line 562-563: please add „;" for reference literature separation

Changed

Figure 2 (06 June 2012) pNO3 x 5 is missing

The new figure includes this information.

Please add a short note to table 2S to explain the reason of outflow time representation in the form of e.g. 25:50:00.

A footnote has been added to Table S2.

Please be consistent with the cases representation form „12 June" or „12 June 2012"

There are no instances of 12 June in the paper or supplement. It would help to have specific line numbers for reference.

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