

Interactive comment on “Observations and hypotheses related to low to middle free tropospheric aerosol, water vapor and altocumulus cloud layers within convective weather regimes: A SEAC4RS case study” by Jeffrey S. Reid et al.

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We would like to start by thanking reviewer 1 for taking the time to review the manuscript. This paper was a result of a realization that to mine the entire SEAC4RS dataset on aerosol-Ac related issues, we needed a frame of reference. There simply was not a good paper in the literature that demonstrates the many facets of the phenomena. The August 12, 2013 flight was the one flight where we could see everything

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going on. In response to your major overarching comment that it would be beneficial to the paper if we could focus more on the aerosol impact on clouds, we heartily agree, but simply do not have space to do it here. This is especially true that we only have one really good Ac pass and these clouds are only a hundred meters or so deep. So it is very difficult to perform the CCN analysis as suggested on a single case. Remote sensing of Ac is also difficult because of their fine cellular nature. Co-author Posselt and I have been devising strategies on how to best model these Ac clouds as it is quite tricky. The aerosol field is imbedded in the detrained cloud layer. There is some hope in that the cloud formation is at the very top of the layer. But it is for these reasons that this phenomenon paper we focused on the covariability and vertical structure. The next paper we are currently constructing has over a half dozen other cases, but the aerosol-cloud microphysics relationships are anything but clean-cut. We have expressed your points in the current draft of the paper in Section 6 and a new Section 7. As mentioned by reviewer two, the aerosol-Ac problem is something that is worthy of a great deal of attention by the community. It is a focus area of the upcoming CAMP2Ex mission, and has garnered the attention of many in the ACCP community.

Specific comments. L350: On melting level heights, 4.5-5 km. Added

L421: “would be helpful to specify approximately how thick the layer is” I am afraid I don’t understand. The sentence specified 200-300 m in depth. Which thickness are you looking for?

L431-4: On adding flight tracks. Adding flight tracks to Figure 2 looks messy, but we added the full flight track as a new figure, Figure A.1.

L507: On RH fields, Added “80% mid mixed layer reaching ~90% between clouds.” We did not really focus on the PBL in this paper, as the way the flight path was conducted with the strong gradient in mixed layer properties in the lead up to the profile suggested samples were aliased down there.

L514: Thank you simultaneous is better. ...

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L517: You are being more specific, “This enchantment is presumably through the detrainment of mixed layer air via the fair weather cumulus.” We pointed out “Also (not shown) was a likewise spike in SO₂ and NO₂ to roughly mixed layer levels (10’s-> 100’s of ppbv), and a minor dip in ozone. (40->37 ppbv).” But we do not wish to go into details on the gas chemistry here.

L594 “they are not directly observed at Huntsville’ For MT3 this seems inconsistent with Figure 5c and line 623 of the text.” Sorry for the confusion. Our point is that these are not the exact same layers whereas the mixed layer is by definition the same layer at Huntsville. We changed the subsequent text to “These layers are similar in nature to layers observed throughout the day at Huntsville.”

L733 “The phrasing could be improved here – I think the secondary mass should help particles activate and then be nucleation scavenged, but the secondary mass shouldn’t be stripped from the particles by the cloud, which is somehow implied here.” Yes, we did not mean to imply that at all, rather the cloud/precipitation process is a net reduction from the secondary mass production. Corrected to “However these same aerosol particles that grow to larger sizes are more likely to be lost to droplet nucleation and scavenging. “

L750. “I agree with sulfate being produced by homogeneous nucleation, but SO₂ tends to be found at relatively similar concentrations at all altitudes in the troposphere, while organics and nitrates and so on decrease in concentration with altitude. I think this is most likely responsible for the increased sulfate fraction in the upper troposphere.” We agree with the reviewer that in general the overall nucleation mode may well be a regional background, but we have also found in our own measurements and the literature that we can find increasing and decreasing SO₂ and sulfate with height. Prompted by this comment, we look back and find that CN in particular is anti-correlated with water vapor, suggesting that CN may be due to background. But higher sulfate mass in the mass spectrometer is sometimes positively correlated with water vapor and OC mass. A good compare and contrast is UT1 and 4 (high sulfate), versus UT 2 and 3 (Low sul-

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fate). We think that overall throughout the column significant amounts of homogeneous nucleation, but the mass is still a result of some form of cloud processing. Indeed, we find a drop in SO₂ at the locations of detrainment. We mention this now in the paper, although we would prefer to leave a more detailed paper on gas chemistry outside of CO for a separate paper. But we now mention this in the paper, and added SO₂ and CO₂ to our figures.

L773-6 “Perhaps worth mentioning some more recent work here, for example on tenuous warm low clouds, e.g. Wood et al 2018, <https://journals.ametsoc.org/doi/abs/10.1175/JAS-D-17-0213.1> Similarly at line 801, are there parallels with the nucleation seen in pockets of open cells, eg. Kazil et al (2011), <https://www.atmos-chem-phys.net/11/7491/2011/>” Good suggestion!

Textual suggestions: All corrected. Thank you very much for the proof read.

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