

Interactive comment on “In-situ measurements of tropical cloud properties in the West African monsoon: upper tropospheric ice clouds, mesoscale convective system outflow, and subvisual cirrus” by W. Frey et al.

Anonymous Referee #3

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Major comments: Outflow is not well defined, nor the nuances involved in the concept appreciated. Outflow from thunderstorms involves a great deal of surface air, but also entrainment from mid and higher levels. All this air is mixed into the air exiting a thunderstorm at the upper levels. Does outflow by some convention only imply air from the surface? How do the authors here identify outflow? What exactly are the tracers that will be used, and what will be concluded from them? We need to know how the authors plan to identify outflow. Can NO/NO_y produced by lightning be used to identify outflow? If so outflow from what level? Yes it identifies air from lightning, but is it indicative of

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surface air, or just air in the cloud? How does air in the anvil of a storm differ from the outflow? Over what altitude range may we expect outflow? How representative of outflow are the measurements presented here? In the contrasting cloud samples from non outflow regions where is the air from?

The authors are too ready to identify any increase in particle concentration as a new particle formation (NPF) event. All measurements are seen as evidence supporting this point of view, while competing possibilities for the source of these high particle concentration measurements, are not discussed seriously. Particle concentrations below the NPF events are missing with no explanations. Surface sources of particles from biomass burning, or other activities, are not discussed, nor measured. No source of fresh SO₂, assumed required for the NPF events, is identified. The measurements of high particle concentrations should be included, and pointed out. The suggestion that they may be indicative of new particle formation can be made, but with the caveat that other possibilities cannot be ruled out by the analysis presented in this paper. Thus further work will be required to establish the source of these particles, and leave it at that. Much better than jumping to unproven conclusions.

On the positive side the ice particle size distribution measurements near African convection and sub visible cirrus (SVC) form a nice data set, and are well presented, except for one serious problem. The FSSP-100 is sensitive to particles between 2.7 μm and 31 μm , which means it is sensitive to cloud droplets as well as small ice. This point is not discussed, and all size distributions and cloud particle number concentrations are presented, and discussed, as if they represent only ice, except in one case on 16 August. What is the evidence for this conclusion of ice in the FSSP data? Were all clouds sampled glaciated? If so how is this determined? The authors discuss one observation of ice particle number concentrations of 8.3 cm⁻³, which is > 8000 per liter of ice, which is a very high ice crystal concentration. How do the authors know that liquid droplets were not part of this measurement?

More specific comments about these points and others follow here, along with some

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minor suggestions, given manuscriptototically.

747.13 – comma after images.

755.23 – comma after concentrations

762.3 ... are compiled in Table 2, ...

762.26: Calling this a mesoscale convective system (MCS) is a stretch of the language. This may become an MCS, but the evidence in Fig. 2 for an MCS is slim. Did these cells in fact develop into an MCS after the measurements, which, however, are characterized as young outflow from an MCS?

763.5-10 and Fig. 3, 4: Either add altitude to Fig. 3, or present Fig. 4 using potential temperature not altitude. “Apparently, the cloud layer ...” Why apparently. Do you not trust the Ncloud/IWC/RH measurements? Are not these pretty clear evidence of cloud between 347 and 353 K? Or is it subvisual above 350 K?

763.5-10: Provide a justification for the labels “above outflow” and “outflow”. I presume the latter is from the NO, NO_y mixing ratios, but the discussion is not clear about this. Label Fig 3 with at least the 3 sub layers if not also the AOF/OF 1-2 layers.

763.11: Lower compared to what, the layers below?

763.15-20: Several comments.

... considered as MCS anvil part ... What does this mean? Do you intend, “... considered as part of an MCS anvil”? If so where is the MCS? See comment above on MCS classification.

What about the “ice particle data” identifies this as an MCS anvil?

Then, but the low NO/NO_y indicates it is not outflow from an MCS. This is also confusing. How does a thunderstorm produce an anvil without the anvil being part of the outflow from the storm? Does outflow have some special definition that I am missing?

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The labeling of the region with high particle number concentration as a new particle formation event is not justified. The Weigel et al. (2011) reference is “in preparation”, and even if submitted to acpd is not a refereed source, yet this reference is repeatedly used. The “evidence” is only for an abrupt increase in particle concentration, which could result from transport, which has not been ruled out. In Fig. 3 the particle concentrations are not supplied below 348 K, which would help identify how unique the layer is. Why are these particle data not shown? Then finally talking about “quenching” a nucleation event, which has not been demonstrated, is artificial and speculative, and should be avoided.

“ Below, from 56 385–56 557 sUTC, the third cloud sub-layer (“Sub-layer 3” in Fig. 4) extended between 11.9km and 11.0km ...” Why now all this detail and switching to time to the second, when this wasn’t necessary before? Also “the third cloud sub-layer (“Sub-layer 3” in Fig. 4)” is rather redundant. Could you not just say here, and elsewhere, ...Cloud sub-layer 3 (Fig. 4) extended ...”

The elevated NO and NO_y is indicative of lightning activity or surface air, or? Please clarify.

763.25: How is the source region of the NO_x identified?

Fig. 4: I presume the widths of the blue bars differ because the ice particle concentrations are different enough that larger sampling times are required to reduce the counting uncertainties? Still why are the very thin sampling regions picked where ice particle number concentration is slightly disturbed rather than where it is quite stagnant?

765.9-16: Same complaint as above about classifying the regions of high particle formation as particle nucleation events. This may be the case, but the evidence shown here is equivocal, although better here with the volatility measurements. In addition from the data shown, the reader cannot discern when the high particle concentrations are in or out of the cloud, and similarly for the volatile fraction, in our out of cloud. Finally why no particle concentrations below 353 K, yet volatility below this point?

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765.26: “The other coloured dotted lines of additional COPAS data show mostly non-zero differences . . .” What is intended with this statement? Does it not just indicate there are particles in these size ranges? What are the authors concluding from this fact?

765.28-766.1: The CO₂ concentration is changing rapidly through the region of high particle concentrations, suggesting a region of strong mixing? This complicates the picture of the conditions which produced the high particle concentrations.

766.1-9: This is one explanation of the observations, but not the only one, and one could imagine other scenarios not requiring new particle formation. Even if there were new particle formation, the argument that a significant fraction would have already grown above 10 nm needs more support. Could coagulation do that in the time available with the particle concentrations observed? Thus the claim here of another “NPF” event has to be tempered with other possibilities.

766.15-28: This section should be re-titled to entrainment and mixing as possible source of the high particle concentrations, and then rewritten to address that possibility. The present title assumes the “NPF” has been proven, and the paragraph and Table 3 add nothing new to Figure. 9, except the relative humidity, which could be added to Fig. 9. In short this section should either be deleted, or really focused on what mixing would mean to possible sources for observations of high particle concentrations. The present effort here provides nothing new.

Fig. 10: Which measurements are above, inside, below the outflow? What identified the outflow? Why include CO₂ on two panels, when it is exactly the same, and then missing on all other panels? What is the CO₂ used for?

767.1-11: Here is a good example of my confusion about outflow. Here are multiple cloud passes with ice crystal size distributions, but some are classified as outflow and some not. What is the difference? How is the outflow identified? Where is the air in the other samples from?

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767.27: What does an outflow age represent? The time since the air left the cloud, or since it was in the updraft core, or since it was at the surface, or ???, and how is it measured?

768.6: Start a new paragraph with, “A closer . . .”, then explain what criteria are used for this separation, and that they differ from the boxes – BOF1 . . . in Figure 13. Here is a good example, in the discussion of the first layer, of the difficulty of using the FSSP-100 for ice particle measurements. Here for the first time the authors point out that the FSSP-100 could be sensitive to particles other than ice, e.g. here aerosol particles, precipitation, and haze particles. But if it is sensitive to these particles it also sensitive to cloud droplets.

769.24-26: ... events did the CIP detect shattering. . . . SVC are noticeably affected . . .

769.28: How did Lawson do it, if the particles are too small to be imaged?

775.21: $8.3 \text{ cm}^{-3} = 8300 \text{ per liter}$. Are you confident of this measurement? It is very high for ice, but not for cloud droplets.

776.29: I do not understand the purpose of this contrast of particle shapes in outflow during AMMA with former SVC particle observations. Is this to suggest that the SVC shapes in AMMA, which were too small to be observed, were similar to other SVC measurements?

777:3-10: These conclusions on NPF are premature based on the measurements presented and discussed.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 745, 2011.

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