

***Interactive comment on* “Drivers of cloud droplet number variability in the summertime Southeast United States” by Aikaterini Bougiatioti et al.**

Anonymous Referee #2

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The authors use measurements of aerosol number and composition along with updraft variability to identify the role each plays in determining simulated cloud droplet number concentrations. I have many concerns with this manuscript. The authors reference anthropogenic and biogenic aerosol precursors as a possible driver of climate over the southeast united states, however there is little to no discussion of this feedback. Also, simulations of cloud droplet concentrations are not compared to any actual measurements of cloud droplet concentrations. One of the major key points of the manuscript relies on comparisons of night flights and day flights however there are only 3 night flights and a total of 10 day flights. It is hard to keep track of which cases are night time and which are day, though the diuranl variability of sigmaw is a key point of the paper. This makes it hard to follow this point. You refer to several different flights, and

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honestly the flight number is somewhat meaningless to the reader. Referring to the flights by certain properties (i.e. night flight 1, night flight 2) would be more useful. The Figure quality is inconsistent and a few figures are repetitive, showing the same result in slightly different ways. Some Figures and tables do not contain data from all cases, leaving the reader to wonder why the other cases were omitted. It is not clear that any result came from section 3.1. Section 3.2 is confusing as it mainly involves a comparisons of individual cases and many sentences and paragraphs do not relate to one another. I was so lost that I stopped reading in this section. It is unclear what data (from tables) was used to calculate many of the numbers listed in this section.

The main result appears to be that updraft velocity and variability are higher during the day, leading to more "simulated" cloud droplets, which is not surprising or new. Comparisons to the contribution of organic mass and particle concentration is also not new. Overall, the manuscript lacks new and measurement supported results, lacks organization, contains figures of low quality, and hard to follow discussion. I am not suggesting rejects of the manuscript only because the measurements published are of high value. I suggest the manuscript be reconsidered after substantial revisions are made to the overall message and clarity of the text, and quality of the figures.

Specific comments:

Line 25: Different how? Explain how it is different before you talk about why.

Line 94: Can you provide a source for the WLOPC?

Line 194: I believe you meant to cite Table 1. What do you mean by "overall values"?

Line 196: For what? It would be helpful to lead the reader more currently it is hard to see where this text is going. Are these the distributions in Figure 2? If so cite them in this sentence.

198: I don't think you need this statement twice within 10 lines of each other.

Line 203: You only chose 4 distributions for each plot in Figure 2. How did you choose

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which flights to include/exclude? I suggest making your y axis the same for each flight. It would be more obvious that the concentrations are different. A log scale for the y axis may be helpful if the authors choose. At the very least please use the same notation for the y axis tick labels.

Line 205: " the modal diameters did not vary much" Why is that significant?

Line 209: You previously mentioned that the organic mass fraction was high during a night flight, but here you are saying 'contrasts between day and nighttime aerosol characteristics/variability may not be as large' Are you saying contrast in composition should be small between night and day? Are you saying the difference in accumulation mode concentration between night and day plays a bigger role in determining cloud droplet number concentration than aerosol a characteristics/variability? It is not clear and if you are saying the latter then you should reference you partial derivatives that you mentioned in line 164 to confirm. If you are going to "discuss the variability of the total aerosol number on droplet number in section 3.2" then it should probably not be mentioned here.

Line 212: It is not clear that "Cont kappa" and "Cont Na" is the partial derivative in Table 3/4. Be consistent with your abbreviations. "contribution" is listed as 'Cont' and 'Contrib' which is confusing.

Line 217: suggest changing "chemical composition" to kappa or hygroscopicity parameter and if that is how "chemical composition" is expressed throughout the paper I suggest using one consistent term or symbol.

Line 228: reference table/figure that identifies daytime sigma₂ varies little and is large. Sigma w at night seems to vary less than during the day based on your next two sentence.

Line 231-232: Is the data used to obtain 0.23+/-0.04 and 0.97+/-0.21 in one of these tables?

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Line 234: " total variability in Nd based on $dNd/d\kappa$, dNd/dNa and $dNd/d\sigma_w$ and the variances of κ , Na and σ_w " this is repetitive.

Line 241: you should state these "sectors" were in atlanta and alabama respectively. You haven't referred to sectors at all so far, making it confusing to suddenly mention them. This paragraph is hard to follow. There are several numbers compared for different cases at different time periods

Line 257: these exact flights and "sectors" were discussed 2 paragraphs ago. This could be better organized.

Table 2: are times in local time? Why are some flights missing from this table? Is there a reason for the order in which flights are placed in the table?(flight 12 is listed after flight 14?)

Figure 3: your plot sizes are inconsistent. What are the hourglass markers? You should mention these are simulated droplet numbers.

Figure 4: Add units to the y axis label

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