Review of "On the drivers of droplet variability in …" by Georgakaki et al.

By Jeff Snider, University of Wyoming

This is my third review of the manuscript. In my opinion, the following things should be addressed before the manuscript is published in Atmospheric Chemistry and Physics.

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L546 - L548. "The Gaussian fit to the updraft velocities gave a distribution with σw = 0.24 and 0.16 ms-1 for the first two clouds present on the 7th of March, and, σw = 0.37 ms-1 for the cloud system observed on the 8th of March."

Two comments:

1) The σ_w data (these are 1-hour averages) plotted in Fig. 5e indicate a factor of four spread over the Cloud-1 interval. Consequently, the σ_w evaluated for Cloud-1 (0.24 m/s; see L546 - L548) does not seem reasonable for either the first part of Cloud-1 (time < 17:00) or for the second part of Cloud-1 (time > 17:00).

2) It's not clear how to reconcile the σ_w averages in Fig. 5e with the σ_w evaluated for Cloud-2 (0.16 m/s; see L546 - L548). By eye (Fig. 5e), the value for Cloud-2 is $\sigma_w \sim 0.12$ m/s. It must be that there is there more updraft variability during the times you have cloud data (Fig. 5c, Cloud-2).

Since the Nd closure (Fig. 6) is central to your paper, I think it is appropriate to explore further this aspect of your analysis. Here is my recommendation. Please present averages of σ_w (in and updated Figs. 5e and 5f, or in a response) for intervals shorter than 1 hour. For example, present a 10-minute average corresponding to 120 updraft samples (temporal resolution 5 s max). In my opinion, this would make clear the basis for the σ_w you report in L546 - L548. It could also make it simpler for you. For example, should you care to rationalize splitting Cloud-1 into an earlier (time < 17:00) and later interval (time > 17:00) interval. Or, it could make it easier for you to argue that the σ_w average for time < 17:00 (7 March) is biased low by updraft measurements collected prior to start of Fig. 5e at ~ 16:30.

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L556 – L563 "The good agreement between measurements and predictions - even under mixed-phase conditions, reveals that processes like condensation freezing and.."

It's not clear what you are getting at with "condensation freezing." It's established, by your group and others (Korolev et al. 2017), that pathways generating ice beyond a few tens per liter, within moderate updraft (≤ 1 m/s), significantly reduce the Smax. I think this is what Sud et al. (2013) and Barahona et al. (2014) were getting at. Can you rewrite L556 – L563 for clarity?

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I recommend the following addition of "***by***":
Reutter et al. (2009) pointed out that droplet formation in clouds can be limited by the amount of CCN present (called the "aerosol-limited" regime), or ***by*** the vertical velocity that generates supersaturation in the cloudy updrafts (called the "velocity-limited" regime).
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"..may decrease CCN activity through the formation of glassy aerosol, has not been assessed in a closure study to date." Is this speculative or is a reference missing?
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"With box model simulations, Hammer et al. (2015).." These simulations applied a closed adiabatic parcel model, I think. "Box" seems like a rigid container.
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L299 – L303 "Aiming to examine how N_d responds to different vertical velocity-aerosol situations, as a sensitivity test, potential N_d for both sites are calculated at 10 values of σ_w between 0.1 and 1.0 ms^{-1} that cover the observed range (Section 3.2.4). Note that we use the term "potential" droplet number throughout this study, as its calculation is performed regardless of the actual existence of clouds over the measurement sites."

This application of the word "potential" is useful. Given what you are saying, Section 3.2.1 is _not_ about potential droplet number, rather it's about measured Nd and measured σ_w in (near) actual clouds. In contrast, Section 3.2.2 is about potential droplet number.

Here is what I'm advocating for: Please improve the section titles so that they apply your definition (L299 – L303) and especially so for titles of Sections 3.2, 3.2.1, and 3.2.2.

Related to this is L541 – L542:
"Note that the ***potential*** droplet formation is evaluated using the updraft velocity PDF calculated for each cloud period, rather than the hourly σ_w data shown in Figures 5e and 5f (Section 2.3)."

In L541 – L542, it's my opinion, you should remove the word "potential."

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L606 "Lower N_d values are visible during nighttime due to the limited turbulence."

Turbulence is lower near a surface at night, however, turbulence is being prescribed in Fig. 7. The diurnal cycle is explained on L320 – L321: *"*N_{aer} at WOP peaks in the evening, reaching up to $\backsim 10^4$ cm^{-3} presumably because of BB emissions in the valley which seem to stop around midnight (Fig. 1a)."
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Why did the locations of the SEA points change moderately from Figure 9 (acp-2020-1036-manuscript-version3.pdf) compared to an earlier draft of Figure 9?

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Reference

Korolev, A., McFarquhar, G., Field, P. R., Franklin, C., Lawson, P., Wang, Z., Williams, E., Abel, S. J., Axisa, D., Borrmann, S., Crosier, J., Fugal, J., Krämer, M., Lohmann, U., Schlenczek, O., Schnaiter, M., & Wendisch, M. (2017). Mixed-Phase Clouds: Progress and Challenges, Meteorological Monographs, 58, 5.1-5.50. Retrieved Apr 8, 2021, from https://journals.ametsoc.org/view/journals/amsm/58/1/amsmonographs-d-17-0001.1.xml