Overview

This study performed a factorial analysis on LES of an Arctic Mixed Phase Cloud (AMPC) case study and compared it to observations from a tethered balloon system and ship-based ground observations. With observational targets of LWP, IWP, and downwelling longwave (DWLW) radiation, they perturbed the aerosol number concentration, aerosol type (i.e., hygroscopicity), ICNC, and ice crystal habit. They found that for these observational targets, the most influential variables describing the variance in the cloud properties were ranked: (1) aerosol number concentration, (2) ICNC, (3) ice crystal habit, and (4) aerosol type, though the ranking depends on the target observable. They then show that ice habit is important for simulating the cloud glaciation. Overall, the study's objectives are well-constructed, the presentation quality is excellent, and the writing is clear and concise. I have some verbose comments below that are ultimately all addressable without substantial revision. Upon addressing these relatively minor points, the manuscript will be well-suited for publication in ACP.

Major Comments

1. My only major comment isn't really that major. The factorial method is a tidy way to present these findings and I really enjoyed it. However, describing the results can become quite difficult, and interpretation for the reader even more difficult. I have some recommendations below regarding technical things that could help (e.g., naming convention), but I note another place or two where adding some brief physical inference could help (e.g., Minor Comment #7). You do a great job of extrapolating this inference in the Discussion section, and I don't think you have to go into it very much when the results are presented, but a brief mention of physical inference *when the results are provided* would be very helpful for the reader. Otherwise, several areas seem like a data dump without much purpose.

Minor Comments

1. **Abstract, Conclusions (Lines 529-531):** The last 2 sentences seem to particularly highlight the influence of ice habit, despite the statement that it ranks "third" in "importance". Although you mention that it determines the *final* phase state of the cloud above, it seems to be introduced as, by definition, a tertiary control and then is given more clout further down, which is a bit confusing. Also, the factorial analysis method maybe recognizable to some readers, but not all; I'm not suggesting you remove that from the abstract as it's crucial to your methodology, but I think something should be included for context of these "rankings" of importance and what that actually means, if you could do so briefly. My understanding is the primary deliverable of the factorial analysis is ranking the contributions to explained variance of the target observables. But a big part of your science question is phase partitioning, which doesn't make ice habit necessarily "less important". That depends on what is important: is it a target LWP, or is it

phase partitioning? There is a brief but better way to conjure this, perhaps by stating that ice habit ranked third in explaining variance of X observables but controlled Y. This applies to your conclusions as well, since a majority of readers are going to reader the abstract and conclusions, primarily.

2. **Lines 75-78, 112, Section 2.4.3:** In the Introduction (lines 75-78), you state that you prescribed ICNC and bypassed the interactive ice nucleation schemes. Then on line 112, you state that primary ice formation is parameterized using different schemes. I was initially pretty confused. Section 2.4.3 finally clears this up by explaining that the baseline experiment uses prescribed ICNC while you also did experiments with INP(T). However, up until reading Section 2.4.3, I couldn't get a handle on it. I would make it clear, around lines 75-78 and/or 112, that both methods are evaluated and compared.
3. The chronology of the model setup is a little confusing. Following are some specific points:
    a. **Line 121:** When you define the initial Qc profile here, it's a little bit of putting the horse-before-the-carriage. The reader doesn't know yet how or why this is being initialized this way and whether it was observationally guided. Of course, this is revealed in Section 2.4.1. If you want to introduce that Qc profile here, I would add something referencing that this is observationally constrained and explained in the upcoming section.
    b. **Lines 115-120:** Somewhat similar to the previous comment, this reads as you giving very little information on the aerosol input constraints, and the reader has no reason to expect more information at this point. It's fine to introduce it this way, but again I'd reference the upcoming section in which more details will be provided. I would also say here very briefly that you are representing the aerosol distribution via observationally derived Aitken and accumulation modes (more details in Section 2.4.2).
    c. **Lines 124-125 and 190:** Okay, so sensible heat fluxes are guided by the constant snow temperature. How are latent heat fluxes handled in this capacity over (snow-covered) sea ice?
4. **Fig. B1:** It appears to me that you are evaluating the simulation around the middle radiosonde launch, but this is after the Helikite flights. Can you explain why you did that? I'm assuming because you wanted a thermodynamic profile representative of the in-cloud launch? (This is more for my own edification)
5. **Lines 163 & 167:** While I understand that surface measurements and the balloon-system camera provide evidence of a mixed-phase cloud, some brief mention should be made about the algorithm used within CloudnetPy via combined ceilometer, radar, and radiometer to classify the cloud as mixed-phase, especially since it does not appear that depolarization capabilities were present. It's not that I'm doubtful, but curious, and I think a reader would be too. Is it because of detectable IWC/IWP via Hogan et al. (2006)?

6. **Simulation naming convention:** I mostly like how you did this, but once I got to the discussion ~ Line 365, I didn't like having to go back and see what "anc1" versus "anc2" meant. The underscore-style naming convention is indeed appropriate and there is no way around it for this many simulations, but it will always plague the reader's memory and desire to spend the time understanding them. Perhaps consider using "ancLO" and "ancHI", which would be more intuitive, like you did for the type and habit with letters. Recommend a similar convention for ICNC.
7. **Lines 383-386:** I've read this paragraph about a dozen times and am still really struggling to piece it all together, and I think this is just a paragraph of statements of relative performance. The figures and table already show this, meaning I think this paragraph isn't very useful in its current state. It would be much more insightful if some physical inference were given for *why* these relative relationships are presenting themselves and why they're important. Otherwise it's just redundant with the figures and tables. You did this *super* well for the following paragraph regarding glaciation and again in the Discussion. Would suggest doing the same here, or removing if it's just not important/insightful. In general, it's helpful to at least provide a brief phrase of the physical reasoning and optionally referring to "more on this in the Discussion" or something like that.
8. **Lines 440-449:** This is a nice summary of trying to explain why INP(T) w/ SIP couldn't realize any ice. You could go a bit further by referencing the study of Fridlind et al. (2007), who simulated an MPACE AMPC case and also found ice-ice collision-induced mechanical breakup to be irrelevant (in addition to drop-shattering and phoretic scavenging). Still unable to realize observed ICNC, they considered the following additional processes: (1) ice nuclei formed via the residual of evaporating drops, (2) drop freezing during evaporation, (3) spontaneous freezing per unit drop volume, and (4) per unit drop surface area, with the latter two meant to account for chemical pathways. These mechanisms were found capable of sustaining observed ICNC, though some are arbitrary and sought an all-encompassing chemical influence. They also discuss other potential mechanisms not considered (see their Section 4.5) that involve evaporation processes. At any rate, it could be worth discussing some of these possibilities, however briefly.
9. **Line 534:** I think your phrasing here undersells how important it is to have in-situ ICNC measurements. This has plagued the community for a long time, and even with in-situ measurements is marred by uncertainty historically (via instrument shattering). In an ideal world, ICNC would be an observational target in your study, not a sensitivity test. Since you're advocating for designing future field campaigns, this seems like an ostensible target, lest we resign ourselves to prescribing ICNC forever.
10. **Lines 476-484:** Very elegantly written, concise explanation. Kudos.
11. The observed evolution of the cloud system in Fig. B1 is perhaps more important than you think. Most readers are probably not going to go there to see it. For example, the glaciation discussion was very interesting, though I had to remind myself of whether or

not the observed system glaciated. Even though you're not trying to simulate the exact evolution of the case and rather the steady-state solution, I think Fig. B1, perhaps in a somewhat condensed form, should be moved to the main manuscript. And perhaps when you discuss glaciation (and other aspects of the cloud system evolution), you can reference back to what the correct solution is, broadly. Again, I know the simulated evolution isn't necessarily the target, but context helps us connect a lot more dots than just presenting the sensitivities, especially with the large uncertainties in some of the perturbed quantities (e.g., ICNC, IWP).

Technical Corrections

- **Line 67:** Maybe say "naturally scarce active INP concentrations" instead? The concentrations could still be there but their ability to initiate freezing may not.
- **Line 72:** I know you discuss this in Section 2, but briefly saying "ship-based expedition" here would help prepare the reader for what to expect from an observational standpoint (static ground vs. ship vs. aircraft). Most of us will have never heard of this campaign (I hadn't).
- **Line 74:** Again, I know you go into more detail in the next section, but I recommended briefly adding "with bulk microphysics" somewhere here to prepare the reader for the scheme structure. I had actually been anticipating a bin microphysics scheme for some reason.
- **Lines 133-138:** You state that the aerosol measurements are taken via an interstitial inlet. It would also be helpful to state how the cloud droplets are measured (open- versus closed-path). Or is it the same instrument? A few additional details would be helpful. A brief mention of the size range that each instrument measures would also be helpful.
- **Fig. 6 Caption:** Please define what the mean diameters represent in the bulk scheme context (e.g., median volume diameter? Mass-weighted? Number-weighted? Median-mass diameter?)
- **Line 165:** It's not clear to me what you mean by "physical sampling" and how that is different from "successfully conducted measurements"
- **Line 319:** Is this line really blue? Looks black to me. If it's actually blue, there's probably not a reason to just not make it black.
- **Fig. 5b:** Just curious, would IWP be better displayed on a logarithmic axis?
- **Fig. 7 and all time-height series:** How are these values horizontally-averaged? Are they conditioned on condensate in any capacity? I'm assuming the cloud layer is effectively horizontally homogeneous, which helps, but if there is any sense of mesoscale structure then we would need to know.
- **Lines 343-344:** Isn't the lower size limit for rime-splintering, as defined by Hallett-Mossop, 24 microns? Why is a diameter of 50 microns used (perhaps you meant radius…)? That range makes a very big difference in CDNC number concentrations

available for the process to be active. This could be related to the definition you use for the representative parametric diameter, which is not described here (see earlier comment).

- **Fig. 10:** I'm assuming the plus signs are distributed horizontally across each box as a temporal evolution? Neat method. But I would state that in the caption.

References

Fridlind, A. M., A. S. Ackerman, G. McFarquhar, G. Zhang, M. R. Poellot, P. J. DeMott, A. J. Prenni, and A. J. Heymsfield (2007), Ice properties of single-layer stratocumulus during the Mixed-Phase Arctic Cloud Experiment: 2. Model results, J. Geophys. Res., 112, D24202, doi:10.1029/2007JD008646.