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Comment on acp-2021-446

Anonymous Referee #1

Referee comment on "Global evidence of aerosol-induced invigoration in marine cumulus clouds" by Alyson Douglas and Tristan L'Ecuyer, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-446-RC3, 2021

Review – **Global Evidence of Aerosol-induced Invigoration in Marine Cumulus Cloud,** by Alyson Douglas and Tristan L'Ecuyer (ACPD, 2021)

This paper uses observational derived products from the CloudSat and CALIPSO satellites to examine how precipitation and latent heating in shallow marine cumuliform clouds depends upon aerosol loading.

Net latent heating increases with aerosol loading (i.e., "invigoration") is found to occur under conditions of relatively low static stability, which contrasts with precipitation suppression under conditions of static stability. The authors find that such increases depend upon the horizontal extent of the cumulus precipitation system. These results are interesting and worthy of publication in *Atmospheric Chemistry and Physics*. There are a number of places where improved clarity would help the reader understand the results and their implications, and I provide some suggestions below to try to help with this.

The term "invigoration" is used to refer to the case where latent heating rates increase at higher aerosol loading. Latent heat release is essentially a consequence of precipitation. The word invigoration means increased animation, and in the context of clouds this would indicate greater vertical motion and/or turbulence. Vertical motion and turbulence cannot be measured, which leaves me wondering if the term invigoration is really appropriate here. Latent heat can increase without an increase in vertical motion. At the very least, some discussion of why the authors feel that the term invigoration is appropriate is required in the revised manuscript.

Main points:

- It is important to state here that latent heating in this study refers to the net latent heating of the system, which is not the same as the latent heating in an updraft. Weakly precipitating marine cumulus and stratocumulus have considerable latent heating in updrafts and cooling in downdrafts, and yet have close to zero net latent heating. These can be quite vigorously overturning layers. **Precipitation suppression by aerosol in these clouds can drive stronger turbulence (see e.g., Ackerman et al., 2004) by reducing net latent heating aloft and cooling below**. A kinematic definition would refer to this reduced precipitation and increased turbulence as invigoration. The authors' definition would refer to this as the opposite of invigoration. Some discussion of this is needed.
- Most modeling studies that I know of suggest precipitation suppression in shallow marine cumulus, or at least a microphysical suppression that may then lead to a PBL/cloud deepening and a precipitation rate that is similar to the unperturbed state (e.g. Stevens and Seifert 2008). Can the authors provide some modeling studies that demonstrate increased precipitation with increasing aerosols?
- More detail is needed on the derivation of vertical motion profiles from the observations. The paper cited in the manuscript (Nelson et al., 2016) does not discuss how vertical motion profiles are derived. If there is no existing manuscript describing the methodology, then it needs to be described in this manuscript. There seems to be a major issue in my view in being able to infer both latent heating profiles AND vertical motion given the limited information (PIA plus the Z profile). The authors need to demonstrate that there is skill in this derived quantity.
- The abstract states that the manuscript shows that cloud top entrainment rates are increased in response to aerosol. As with vertical motion profiles, I don't see how entrainment rates are derived with the available observations. Cloud top entrainment is a particularly challenging observation to make, and I suspect that changes in entrainment can only be inferred indirectly. Please clarify.
- The authors need to be precise in clarifying how invigoration is defined. The abstract introduces three separate metrics (precipitation, vertical motion, entrainment rate). I assume invigoration pertains to stronger updrafts under polluted conditions, not stronger precipitation rates. Is that correct? Line 182 suggests that it is something other than precipitation, i.e., turbulence, that they are referring to when they refer to invigoration.
- Line 21: Pincus and Baker does not pertain to cumulus clouds, but shallow marine stratus clouds. Invigoration here refers to enhanced PBL TKE by precipitation suppression, which makes the PBL less stable, and increases entrainment. Rosenfeld et al. (2008) is specifically invigoration in clouds where freezing latent heating can be enhanced by suppression of warm rain, i.e. deep clouds. So these references are not particularly pertinent to shallow marine cumulus.
- Line 25: I don't understand why increased turbulence is necessarily indicative of increased precipitation. It is *suppression* of precipitation that has been argued to enhance turbulence in most studies of warm rain impacts, including L'Ecuyer et al. (2009).
- Line 30: I'm confused. Which figure in Kubar et al., (2009) shows this? Kubar shows that deeper clouds precipitate more frequently and that precipitating clouds tend to have lower droplet concentrations. But I don't think it shows that clouds are deeper in polluted environments. Please provide supporting evidence.

- Line 95: Controlling for meteorology. Using only two stability bins still gives an enormous range of EIS within each of the two categories to allow confounding meteorology to creep in. What about limiting to narrow EIS ranges to limit this issue?

Other comments:

- Line 8: What is the "pristine cloud response" responding to? I thought that the responses being investigated here are to aerosol, so I am a little confused.
- Line 40. Jiang et al., (2009). Presumably the additional cooling results in stronger downdrafts. Is that the case, or are updrafts also enhanced?
- Line 42: What is "droplet mobility"?
- Line 45: "cloud lifetime with increasing lifetime"? Do you mean "cloud lifetime with increasing droplet concentration"?
- Line 53: This makes sense, because FT humidity tends to increase cloud longevity (see Eastman and Wood 2018, for example).
- Line 61: Please motivate the choice of 150-200 g/m2 for the LWP range used here. Are the results at all sensitive to this choice?
- Line 87: It would be useful to know what the distribution of cloud horizontal size looks like. AMSR-E footprints are variable depending upon the frequency, so 15 contiguous CloudSat pixels (~30 km) equates to what frequency of AMSR-E? In the subsequent figures (e.g. Fig. 2), why are results only shown for clouds up to 7 km in size?
- Line 109. I assume that the latent heating rate is the difference between condensation and evaporation derived from the LES simulations. Can this be stated explicitly?
- Line 117: These simulations are not really LES, because their resolution is too coarse (250 m and 100 m vertical will not accurately represent the scales of mixing responsible for cloud top entrainment). I wonder how sensitive the LH profiles are to the vertical and horizontal resolution in the model. Has this been tested? The ATEX simulation used is Cu under Sc (cloud cover ~50%).
- Is Figure 1 a single case as opposed to a composite of many cases? Please provide date, time and location if the former.
- Line 127. "which include cooling by evaporation". What else do the cooling rates include?
- Line 128: Isn't the entrainment zone everywhere where there is latent cooling, not just where the profile shifts from positive to negative?
- Line 136: I'm confused here. Pollution decreases collision-coalescence generally. If the smaller supersaturation in polluted conditions enhances latent heat in the updraft, then this could eventually overcome precipitation suppression I guess. Is that what is meant here? I'm just confused about what invigoration really is in the context of this paper.
- There needs to be some attempt at quantifying the sampling uncertainties in Fig. 2. There needs to be a confirmation that there is no correlation between aerosol loading and stability in the different quadrants used (stable/unstable, polluted/clean). If there is, then the whole result could simply reflect meteorological covariation.
- Fig 2 caption. This is showing results as a function of the cloud size, so why does the

caption state that the results are for clouds with an extent of 15 km?

- Line 149: I don't see an inflection point. I see a maximum at 5km and then a decrease. What am I missing? What do the authors mean by an inflection point? It is defined as where a function changes curvature. Do the authors mean a maximum?
- Lines 153:160. This paragraph is difficult to understand and read. First, I don't see how Fig. 3 shows what is being discussed. It doesn't show values in the center, but simply the maximum values. How do we know these are in the center? Does this mean the center in the horizontal or the vertical direction? Second, how is it possible to determine an inflection point at a size of 7km, when 7 km is the maximum size shown?
- Line 162: Should not start a new section with "However,…"
- Line 165: How can evaporation take place above a cloud, when it is the cloud that is evaporating? I think you mean greater evaporation rates near cloud top. Also, cumulus clouds evaporate mostly by lateral rather than cloud top entrainment, so how is this factored into the analysis?
- Line 166: Under what conditions does increased mixing with the free atmosphere lead to cloud deepening? Mixing causes evaporation and a loss of buoyancy, and momentum friction, all of which slow the rise rate down. Exactly the opposite of what is stated here.
- Line 171: The authors' explanation seems reasonable, but the result runs counter to our understanding of how entrainment works in marine stratocumulus. All else being equal, a drier FT produces greater negative buoyancy of mixtures of cloudy and clear air, and thus promotes greater entrainment rate, and thus greater evaporation (e.g., Nicholls and Turton 1986).
- 3: Why are the latent heating rates here so much higher than those in Fig. 2? Does one infer that nearly all the condensate evaporates?
- Fig 4 and elsewhere. Figure panels should be labeled (a), (b)… and referred to with these labels.
- Line 174. I do not understand this sentence at all.
- Line 176: But the authors have essentially limited their cloud thickness by using only a narrow range of LWP values. So how can this deepening effect be determined if the cloud thickness is fixed?
- Line 184: Cooling below cloud increases stability of the boundary layer unless the cooling is focused just below cloud. Often the cooling profile below cloud will maximize further down where it is relatively drier and thus promotes stability. This all depends upon the precipitation drop size. Here it is stated as decreasing stability as a general effect.
- Can CloudSat really constrain below cloud evaporation given that most precipitating marine low clouds have LCLs below 1 km and this is the lowest observable level with the CPR? There needs to be some caution in interpreting these results.
- Line 194-196: Is there any in-situ observational basis for this supposition? Why not express the evaporation rate as a rate per latent heat release in the cloud, so it can viewed more as an efficiency of evaporation?
- Line 197-201: There are just too many pieces to this argument for me to wrap my head around. It also seems to involve not just one cloud but an ensemble, since the CCN activation process has to follow the other processes, so it must involve a second cloud. Perhaps the authors should include a schematic to help the reader understand all the mechanisms they are proposing/investigating.
- Line 207-208: I don't follow. How does reduced evaporation below cloud imply anything about cloud lifetime?
- Line 210: This argument about stabilizing the PBL is the exact opposite of the one on Line 184, where the authors argued that evaporation below cloud can destabilize the PBL.
- Line 217: How are the results consistent with this? Latent heat release is a consequence of precipitation and does not necessarily imply anything about turbulence as far as I can tell.
- 6: I don't understand how cloud updraft speeds of cm/s can be measured.

- Line 246: Clarify what the difference is between peak and core.
- Line 249: They show the opposite of invigoration, which is suppression. This is not a dampening of invigoration. Again, there is confusion about the definition of invigoration. This sentence states that invigoration is dampened by reducing precipitation formation rates.
- Line 251: This flies against understanding. Ackerman et al. (2004) show increased entrainment as aerosol increases. This is because suppression of precipitation increases TKE by reducing stability in the PBL. Precipitation is suppressed in polluted clouds by reduced collision-coalescence.
- Line 256: "As the rain system grows, ... are a function of the size of the rain system". Grammatically it would be better to remove one of these references to size.
- Line 265-266. This is not a sentence. I suggest putting in a semi-colon instead of the comma before "only".
- Line 279. In thinking about their future work, in addition to Christensen et al. paper, the authors may be interested are a number of recent papers by Eastman and colleagues on Lagrangian tracking of low cloud systems and PBL properties, and how to isolate different meteorological controls on their temporal evolution (e.g., Eastman and Wood 2016, Eastman et al., 2016; Eastman et al. 2017; Eastman and Wood 2018)

References:

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Nicholls, S., & Turton, J. D. (1986). An observational study of the structure of stratiform cloud sheets: Part II. Entrainment. *Quarterly Journal of the Royal Meteorological Society*, *112*(472), 461–480.

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