Reply to Referee #2

Adam Majewski[1], Jeffrey R. French[1]

[1]Department of Atmospheric Science, University of Wyoming, Laramie, 82070, USA

Correspondence to: Adam Majewski (amajewsk@uwyo.edu)

We thank the reviewer for pushing to broaden the applicability and better articulate the novelty of the results and also for challenging us to better quantify the perturbation correlations, leading to a change to the conceptual model that seems to agree better with the flight data. This, together with comments from the other reviewer led to a significant revision of the manuscript. We believe that the revised manuscript is easier to read, more consistent throughout, and provides conjecture and explanations that are better supported by the observations presented.

Below, comments provided by the reviewer are in black, our responses are in red.

Reviewer Comments

This paper explores aircraft observations using a W-band radar and in-situ measurements of state parameters, wind motions, and cloud and drizzle drop size distributions within a supercooled layer cloud flowing over heterogeneous terrain. The authors find correlations between km-scale somewhat vertically coherent fluctuations in vertical motion and microphysical changes in the cloud. The measurements appear to support the idea that small scale fluctuations in such clouds may be sufficient to push an otherwise non-drizzling cloud into a state whereby it produces precipitation.
I found the manuscript to be adequately well-written, although the authors should pay better attention to spelling (Rosemount is mis-spelled in several occasions), grammar, and precision in their scientific writing.
Comments from both reviewers have led to a significant revision of the manuscript. In this revision, we have taken care to be more consistent with our wording, grammatically correct and consistent, and more precise in our descriptions. Because of this, we believe that the revised manuscript is easier to read and contains fewer inconsistencies that can lead to reader mis-understanding.

The results, while interesting, are not particularly novel (see e.g. Houze and Medina 2005, who have already documented such correlations between small-scale vertical motions and radar-derived microphysical properties). It is a little unclear how the results presented would move our knowledge base forward.
Houze and Medina (2005; HM05 for brevity) examined the enhancement of precipitation by turbulent overturning cells in coastal frontal systems with contained a significant orographic forcing. At relatively large spatial scales (broad udrafts and large swaths of available condensate) and small spatial scales (~kilometer scale vertical motions embedded in layers of shear-driven overturning cells) orography was shown to modify the flow field to generate or otherwise enhance condensate supply rates, increasing upstream precipitation via increased collectional growth where condensate was locally concentrated. Although the cases analyzed were principally of precipitating mixed phase clouds with active ice nucleation processes, the authors suggested that for clouds with the 0 °C isotherm nearer the surface or with embedded bright bands, these turbulent overturning cells would be expected to similarly enhance growth rates for falling liquid hydrometeors. It is precisely in this context that several of our findings here are novel: (1) despite the w-LWC relationship reported in HM05 for mixed phase clouds (+w' and +LWC' on average), we found the opposite correlation which we believe to be a function of low droplet number concentration; (2) collectional growth still appeared to be enhanced through these layers despite the inverse w'-LWC' relationship; and (3) vertical location of initial collision-coalescence activity appeared to be tied to these layers even well below cloud top. For these reasons, this not only serves as a strong addendum to HM05 with respect to liquid or mostly

liquid clouds, but also raises questions as to whether turbulent motions, locally enhanced SLW pockets, or something else (e.g. condensational kinetic effects for liquid, lengthened trajectories for ice, etc) are responsible for the faster hydrometeor growth noted in these layers *for all conditions*.

The authors need to work harder to make their results appealing to the broader cloud physics community.
Care has been taken in the revised manuscript to place these results in the context of all highly supercooled liquid clouds (for instance, paragraph 7, Section 1; and paragraph 1, Section 5), liquid clouds with marine aerosol character (paragraph 5, Section 1), and to simplify the conceptual model as much as possible (paragraph 3, Section 4.1).

The schematic diagram presented in Fig. 13 is interesting, but I believe that the condensational inertia theory of why the LWC and Nd estimates are not in quadrature is insufficient. As the authors argue, the condensational delay may be around 10 seconds (phase relaxation timescale), yet the time between wave crests is substantially longer than this (probably 100 s or more for wind speeds of 10-20 m/s and wavelengths of 1-2 km). More quantification of this would be helpful.
Comments from both reviewers has led to additional analysis investigating correlations of perturbation quantities. This has resulted in a significant revision to the schematic diagram (Fig. 13) in the revised manuscript. Details of this are provided at the end of our comments.

Why isn't the removal of droplets by coalescence also playing a key role?
This is explicitly addressed in paragraph 4, Section 4.1: The remaining magnitude of CWC variation is likely related to the precipitation dynamics. Removal of cloud water by scavenging from drizzle in perturbation updrafts would lead to lower CWC's and reduced cloud droplet number."

Why is there no map showing the synoptic conditions, horizontal flow pattern etc?
We included no map of the synoptic conditions because we felt they were adequately described in the exposition of the case context, and that the bulk thermodynamic conditions were more enlightening in describing how and where clouds formed. Here we try to strike a balance between completeness and length of manuscript. Synoptic maps can be found in the Master's Thesis from which this manuscript was developed (Majewski 2019; Fig. 3.1, p. 55).

Where are the Payette mountains?
The Payette mountains are a locally-used reference to the western-most foothills of the broader Sawtooth Range. In the revised manuscript all reference to the Payette mountains has been removed and reference is now made to the Sawtooth Range, consistent with the map shown in Fig. 1.

I think the data here could be analyzed in a much more quantitative manner than is presented here. What is the vertical coherence of the small-scale vertical motions as seen by the WCR? This is why we have radars. Yet the radar here is underutilized.
Some attempt to quantify the coherence of the Doppler velocities and the reflectivities have been done in a statistical sense for the CFAD columns with the vertical profile of bulk correlation coefficients. However, going beyond this to investigate the coherence of small-scale motions is not trivial, given the convolved nature of what is measured by the Doppler radar. Variations in hydrometeor terminal fall speeds, especially for drizzle, are much larger than variations in vertical air motion.

Fig. 7 states that hydrometeor Doppler motions are shown, but this implies that the vertical wind field is known. How can this be? This needs some correction to explain what is shown and what was done to remove the wind motions.
Figure 7a shows the measured *Doppler velocity*, as indicated in the caption (note this is the same as shown in Figs. 4, 10b, and 11b). No attempt has been made to de-convolve the vertical air motion from the hydrometeor terminal fall speeds in any of these images. This is explicitly stated in the revised manuscript, at the end of paragraph 3, Section 2 and again in paragraph 3, Section 3.2.

Note that in Figure 7b, we *estimate* the hydrometeor terminal fall speed for range gates located near the aircraft (both above and below flight level). This is done by subtracting the aircraft measured vertical air velocity from the radar measured Doppler velocity in these range gates. The description of this is found near the end of paragraph 3, Section 3.4.

It is interesting that the clouds are ultra clean (very low cloud droplet concentrations). Yet this is barely mentioned later. Is there a real bottleneck for drizzle production given this? The authors could quantify the coalescence by running an SCE solver on their size distributions to quantify the degree to which the clouds could produce drizzle without vertical motion enhancing LWC.
We agree. The very low droplet concentrations encountered on this day and throughout the field campaign were a very interesting (and surprising!) observation. We reference low droplet concentrations throughout the manuscript as we describe the microphysical characteristics of the observed cloud. These low droplet concentrations are critical regarding our understanding of the role of condensational inertia as pointed out in Section 4.1.

To address the reviewer's question as to whether there is truly a condensational "bottleneck" for cloud droplet numbers as low as those reported here, it seems the corresponding marine stratocumulus research regarding ultra clean layers (Wood et al., 2018; Kuan-Ting O et al., 2018) might be most relevant. Laminar veil clouds that detrain from marine cumulus can persist on the order of hours against very weak lift (1 cm s^{-1}). While containing drop effective radii in excess of 20 μm, the persistence of these clouds (timescales on the order of hours) against such weak updrafts suggests weak sedimentation and little collision-coalescence activity else clouds would more quickly dissipate. Subsequent modeling results (Kuan-Ting O et al., 2018) indicate that little if any sedimentation and collision coalescence persists after parcels moved into the detrained quiescent layer. Finally, DSD solutions for marine aerosol populations in vigorous (cumulus) updrafts have already been demonstrated to asymptote to an upper effective radius below 20 μm with diminishing dispersion and spectral width magnitudes above cloud base for a polydisperse parcel model (Pinsky et al., 2014), indicating that without broadening and/or collision-coalescence mechanisms, there is a definite upper limit to the size of droplets produced through condensational growth alone. Regardless, we have removed the "bottleneck" vocabulary from the revised manuscript and just directly refer to a narrow, large drop, condensational mode.

It is hard for me to understand why it is important that the cloud is supercooled. Wouldn't the same physics affect warm layer clouds?
In short, yes, the same physics apply in warm cloud layers. But consider for a moment marine StCu. Such clouds are BL phenomena occurring over a flat surface. There appear few if any drivers for SCVVFs to occur within the middle of these clouds. In such cases, vertical velocity fluctuations that can act to enhance drizzle production will almost certainly be confined to cloud top and therefore it should not be surprising that drizzle initiation occurs at the top of these cloud layers.

However, the emphasis here on a supercooled cloud must consider that: (1) this cloud had extremely cold cloud tops (T~-30°C), which Demott et al. (2010) suggest should lead to high INP concentrations, so the near absence of ice is quite surprising; and (2) for supercooled mixed phase clouds to produce SCDD requires relatively few CCN and INP. This scenario all but requires that supercooled clouds be inefficient precipitators with most of the mass distributed in the SLW categories. This also means that nearly all supercooled drizzling clouds can be expected to respond in kind to SCVVFs/Overturning Cells, which have already been acknowledged to be nearly ubiquitous in an orographic environment.

Condensational Inertia and Conceptual Model

R1: To me the correlation between Ncld and w' looks poor - can you plot scatter plots and give correlation coefficients - or even do lagged correlations given the discussion at the end of the paper?

R2: The schematic diagram presented in Fig. 13 is interesting, but I believe that the condensational inertia theory of why the LWC and Nd estimates are not in quadrature is insufficient. As the authors argue, the condensational delay may be around 10 seconds (phase relaxation timescale), yet the time between wave crests is substantially longer than this (probably 100 s or more for wind speeds of 10-20 m/s and wavelengths of 1-2 km).

Both referees indicated a desire for better quantification of the relationship between w' and CWC'/N_{cld}'. For referee 1 this had to do with some suggested N_{cld}/CWC relationship while for referee 2 it concerned the time/spatial scales and the lack of time series signals being in the expected quadrature relationship as per the proposed conceptual model (Fig. 13). To examine these relationships more quantitatively, the higher (5 Hz) resolution w', CWC', and N_{cld}' time series for were detrended and filtered of frequencies smaller than 0.1 Hz (wavelengths longer than 1 km). These time series were lagged by 0.2 s increments over a full 10 s period and correlated with a Pearson autocorrelation function to determine the correlation coefficients at each time lag. The results are presented below:

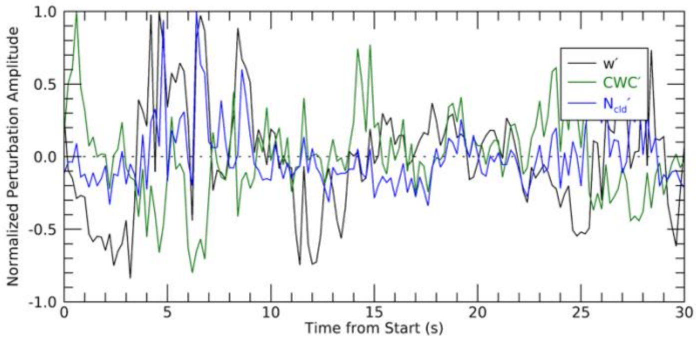

Figure 1: Normalized perturbation time series for selected kinematic and microphysical measurements. Measurements have been de-trended and filtered of frequencies lower than 0.1 Hz (corresponding to wavelengths longer than ~1 km).

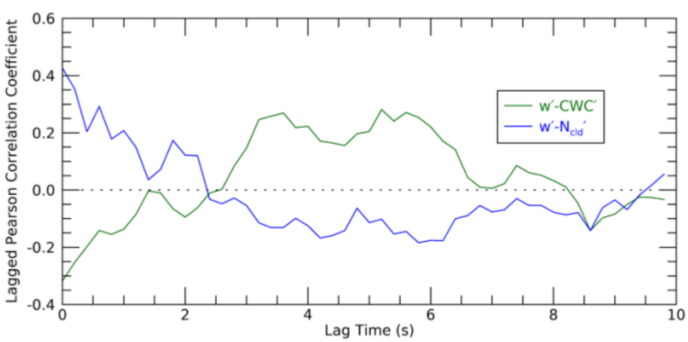

Figure 2: Lagged Pearson correlation coefficients for the time perturbation quantities in Fig. 1.

The lagged correlations (with maxima at position 0) clearly indicate that these condensational kinetic responses are zero-lag, with w'-CWC' being anticorrelated and w'-N_{cld}' positively correlated. Zero-lag correlations in this context likely indicate that cloud parcels are moving with(in) the kinematic pattern as opposed to through it as the latter should result in some spatial lag corresponding to the motion of parcels relative to w' pattern. This analysis has led us to modify the conceptual model presented in the original conceptual model with a more simple one. The analysis, while discrediting the original model, otherwise strengthens the suggested microphysical response. Furthermore, for the phase relaxation time to have led to a spatial lag exactly in quadrature now seems obviously unlikely, and we thank the referees for asking us to quantify these relationships. A new conceptual model incorporating this insight is proposed with perturbations caused from kinetic responses to a vertical velocity couplet more closely resembling the overturning cells suggested in HM05 to avoid the flow continuity issues that arise from this vertical parcel motion framework (e.g. when considering the location of maximum vertical displacement of parcels).

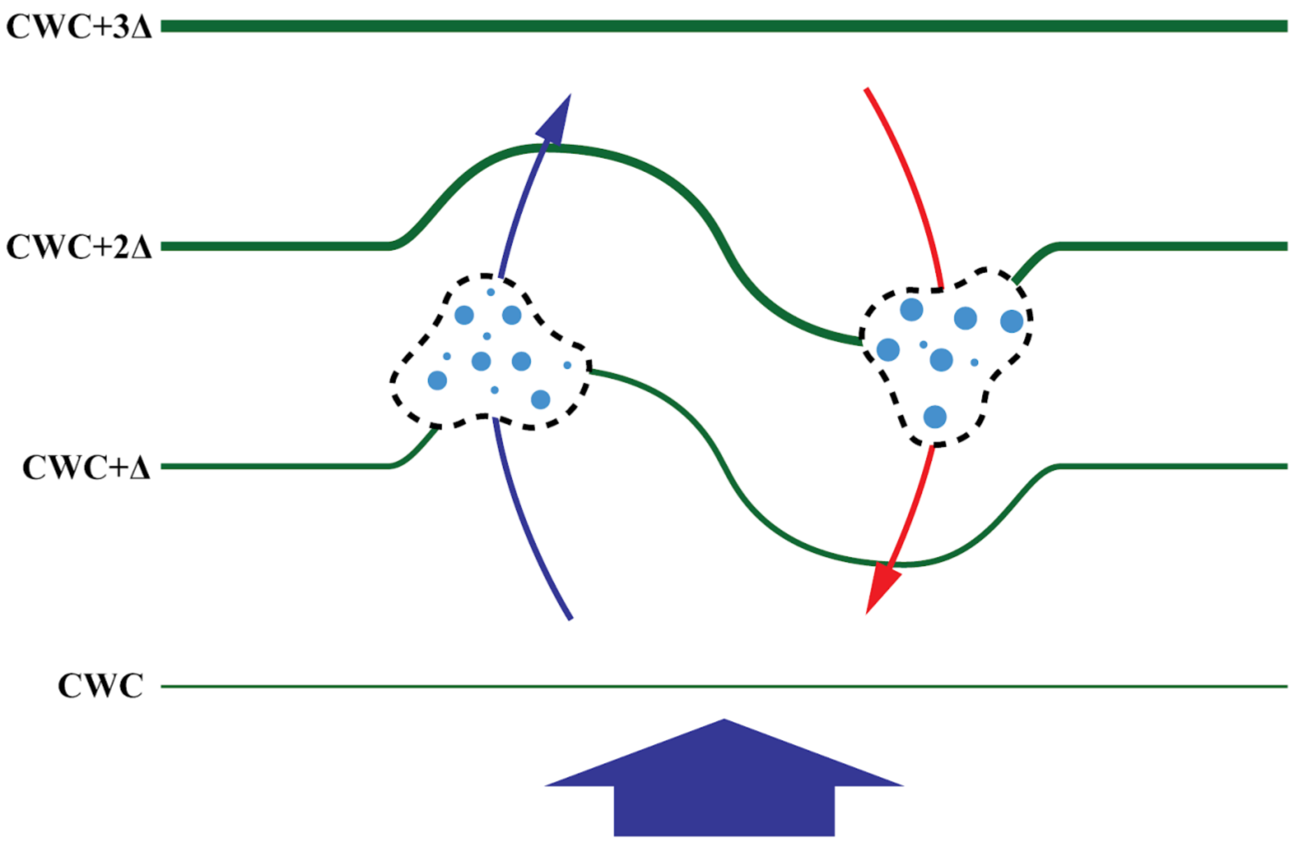

Figure 3: Revised conceptual model: simplified schematic of spatial responses to the perturbation updraft (blue) and downdraft (red) pattern superimposed on broader orographic lift (broad blue arrow bottom). The colored trajectories indicate the approximate path of parcels passing through the kinematic pattern following the schema of Houze and Medina (2005). Lines of constant cloud water content (green) indicating the expected deformations due to condensational kinetic effects, with line weight corresponding to relative condensate mass. Cloud parcels circulate within the vertical velocity perturbation pattern and more and smaller drops are located in perturbation updrafts than downdrafts. CWC contours appear flat and unperturbed above and below the vertical velocity fluctuation pattern as they are determined by the adiabatic ascent in the broader uplift pattern.

References

DeMott, P. J., A. J. Prenni, X. Liu, S. M. Kreidenweis, M. D. Petters, C. H. Twohy, M. S. Richardson, T. Eidhammer, and D. C. Rogers, : Predicting global atmospheric ice nuclei distributions and their impacts on climate, Proc.Natl.Acad.Sci.USA, 107, 11217-11217-11222, https://doi.org/10.1073/pnas.0910818107, 2010.

Houze, R. A. and S. Medina: Turbulence as a Mechanism for Orographic Precipitation Enhancement, J.Atmos.Sci., 62, 3599-3599-3623, https://doi.org/10.1175/JAS3555.1, 2005.

Kuan-Ting, O., R. Wood, and C. S. Bretherton: Ultraclean Layers and Optically Thin Clouds in the Stratocumulus-to-Cumulus Transition. Part II: Depletion of Cloud Droplets and Cloud Condensation Nuclei through Collision–Coalescence, J.Atmos.Sci., 75, 1653-1653-1673, https://doi.org/10.1175/JAS-D-17-0218.1, 2018.

Majewski, A. J., 2019: Supercooled Drizzle Drop Development In a Postfrontal Orographic Layer Cloud in Response to Semi-Coherent Vertical Velocity Fluctuations, University of Wyoming. https://search.proquest.com/docview/2282179118

Pinsky, M., I. P. Mazin, A. Korolev, and A. Khain: Supersaturation and diffusional droplet growth in liquid clouds: Polydisperse spectra, J.Geophys.Res.Atmos., 119, 12,872-12,872-12,887, https://doi.org/10.1002/2014JD021885, 2014.

Wood, R., K. O, C. S. Bretherton, J. Mohrmann, B. A. Albrecht, P. Zuidema, V. Ghate, C. Schwartz, E. Eloranta, S. Glienke, R. A. Shaw, J. Fugal, and P. Minnis: Ultraclean Layers and Optically Thin Clouds in the Stratocumulus-to-Cumulus Transition. Part I: Observations, J.Atmos.Sci., 75, 1631-1631-1652, https://doi.org/10.1175/JAS-D-17-0213.1, 2018.