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Interactive Comment

Interactive comment on "Characteristic nature of vertical motions observed in Arctic mixed-phase stratocumulus" *by* J. Sedlar and M. D. Shupe

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We appreciate Reviewer 1's detailed reading of our manuscript and insightful comments for improvement. We have considered all the comments/suggestions of the reviewer. Below are detailed responses to each of the points raised.

Response to the 3 main comments of Reviewer 1:

1) We appreciate the concern of the reviewer of a lack of focus in the results section, especially during the case study descriptions in Sec. 4.1 & 4.2, as well as the results description in Sec. 5. Granted these results are for specific case studies and require sufficient detail to fully describe the processes ongoing, the results description has been improved following the reviewer's suggestion to focus more on how the details

relate to the over-arching questions and how they fill gaps in the current knowledge of AMPS vertical motion. We have also taken the suggestion to include a bullet list of these over-arching questions to be analyzed in the introduction. As the reviewer suggests, this allows the reader to more easily understand why many of the details in the results section are included. We have also included a better relation of the findings presented in the results section with previous cited work to avoid 'disconnecting' the references with the results.

2) As noted by the reviewer, a full error analysis of the uncertainty in MMCR retrieved vertical velocity is beyond the scope of this paper. Section 2.2 contains a number of references to literature that describe the uncertainties present in the retrievals for the intention of analyzing absolute vertical velocity magnitudes. As discussed in that section, we are not interested in the absolute magnitudes of w, but rather how the variance and skewness of w vary with time and elevation within the cloud. This is why we discuss estimating the 'corrected w' (removing the mean 30 min bias which Shupe et al. 2008b claim as the largest source of w uncertainty) and comparing the statistical results (variance, skewness) with those from uncorrected w estimates (p. 31085, lines 14-27).

3) We understand the concern of the reviewer regarding the 'busy' nature of Figures 7, 9, 10 and 11. We already have a lot of figures in this paper, and we choose to keep them together as sub panels, especially Figs. 7 and 9 to explain the results of the case studies. To aid in readability and understanding, fonts have been increased and the wavelet figures have been focussed on the timescales of interest (2-240 min) following the reviewer's suggestion (also see comments from reviewer 2).

Responses to smaller comments throughout the manuscript:

Abstract and conclusions: The 'positively-correlated vertical motion signal' refers to the a positive correlation in vertical velocity variance between vertical levels within the cloud. This clarification has been added.

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Introduction, line 12: We have changed the comparative from ‘cooler’ to relatively cool as suggested. Additionally, we have scoured the text and removed additional comparatives that are incorrectly used.

p. 31082, line 3: Here we are referring to an understanding of low-level AMPS in general. Vertical motions are one important characteristic (hence the purpose of this paper) but in this statement we are referring generally to all AMPS characteristics (frequency of occurrence, lifetime, distribution of liquid and ice, radiative impact, etc.).

p. 31085: The intention of the first paragraph is to acknowledge previous research that has shown vertical velocity retrievals from MMCR should be corrected if one is interested in the absolute value of w. The paragraph goes on to state how such corrections can be ignored as we are interested in examining the statistics of w distributions (skewness, variance) and the dominant temporal frequencies corresponding to w-variance within cloud using transformed, Fourier analyses. As such, the following paragraph describes how we tested the notion of ignoring the corrections to w-uncertainties with profiles where we made a correction for the largest source of uncertainty - the removal of 30-min mean bias. This comparison showed no changes in the Fourier analyses and thus is the motivation for not correcting all the profiles of w in further analyses, which would have not have allowed us to examine frequency changes on timescales longer than 30 min.

p. 31085, line 8: We have removed ‘themselves’ entirely.

p. 31086, line 3: the a priori dataset is the interpolated radiosondes, as originally stated.

p. 31087, line 12: We disagree with the reviewer’s comment to remove this statement. If these cloud layers examined in this study were exceptionally different (occurrence, vertical location, phase) from what is commonly observed across the Arctic, then the representability of results from this study would not translate to AMPS in general.

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p. 31087, line 20: Changed according to reviewer's suggestion.

p. 31088, line 7: A running window is needed because variance and skewness statistics need a sufficient sample of data. We choose 20 min windows to sufficiently cover the dominant time scales observed (~8 min) by Shupe et al. (2012). Due to the relatively course vertical resolution (45 m) of the MMCR, variable cloud boundaries further require that at least 50% of the w-estimates at a particular level must be present in order for the statistics to be calculated. We have clarified this in the text.

p. 31089, line 11: The equation has been changed as suggested.

p. 31090, lines 5-9: The sentence presents a fundamental difference in vertical velocity skewness distribution between the ASCOS AMPS with those found in lower-latitute stratocumulus. We have chosen to keep the sentence as is.

p. 31091, line 29: This has been changed as suggested.

p. 31092, line 5: This statement refers to the inter-quartile range of delta theta-e, where we show that the 25th percentile of delta theta-e is commonly found at or above zero in the lower portion of the sub cloud layer.

p. 31094, line 22: The general description of the wavelet peak timescales has been modified, with an emphasis placed on the longer time scales (> 30 min) from which we relate to mesoscale forcing as opposed to cloud-driven forcing.

p. 31097, line 20: This change has been made as suggested by the reviewer.

p. 31097, line 29: We have changed the text to 'timescales longer than 20 min'

p. 31099, line 3: We have included the reference to the top panel in Fig. 8 where mixed layer base height and the mid-level height of the cloud are shown.

p. 31100, line 2: Panel letters have been included as suggested.

p. 31101, line 1: This statement has been removed in the revised version, and instead

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is related to previous studies that have observed this feature of cloud-boundary layer stability.

p. 31101, line 20: We appreciate the concern of the reviewer regarding estimates of bulk LWC. We are not attempting to estimate the distribution of LWC within the cloud. The estimate of bulk LWC was included to better infer whether coupled/decoupled clouds contained more liquid water because either they contain more LWP or because the cloud layer is in fact geometrically thicker. We then relate these estimates to the w-variance observed in the 5-10 min time frequency. We have changed the description of the analysis from 'bulk LWC' to 'scaled LWP (LWPscaled). The same result holds, only we no longer refer to this value as a liquid water content.

p. 31102, line 6: The reviewer is correct in that directional wind shear may also (likely is) be responsible for vertical mixing across the cloud and sub-cloud layers. We are not arguing that directional shear is negligible. However, directional wind shear is notoriously difficult to estimate from noisy radiosonding data, and it is for that reason that we do not include estimates here. This is a feature we are currently looking into as a mechanism for coupling between cloud and sub-cloud layers.

p. 31103, line 7: The sentence has been clarified to read as '..one of the reasons for the observed increases in w-variance when the surface and cloud are coupled (Fig. 11).'

p. 31103, line 25: The region and time period have been specified following the reviewer's suggestion.

p. 31106, line 9: We have added references to recent studies confirming the persistence of decoupled cloud layers, even though the shallow surface boundary layer is often observed at near-neutral stability.

p. 31106, line 25: We have removed the statement following the reviewer's suggestion.

p. 31107, line 12: We have revised the conclusion point in order to specify that we are not looking at the distribution of LWC within the cloud layer, but the relationship between LWP and cloud thickness (scaled LWP) as a function vertical velocity variance. This is now consistent with the analysis of the revised results presented in Section 5.

p. 31107, line 27: As suggested, the sentence has been split into two.

Figure 4: We have removed the reference to 'notching' in the figures (see also reviewer 2's comment) and now explicitly state in the text that the medians at adjacent levels are significantly different from each other at the 99% confidence levels).

Figures 7 d,e and 9 d-f: We intended the axis label to be power spectral density of w (Sw). We now see the confusion with our earlier definition of w-skewness (also Sw). Therefore the labels in these figures have been changed to PSD (power spectral density).

Figure 11: We have changed cumulative RFD (relative frequency distribution) to 'cumulative frequency distribution.

Figure 12: RFD has now been defined in the figure caption

Figure 13: The caption has been corrected following the reviewer's suggestion.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31079, 2013.