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.

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.

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 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.

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We appreciate Reviewer 2’s positive response to the paper and insightful comments regarding the need for uncertainties in the methods used in this study. We have made changes to the text to account for retrieval uncertainty and how they may or may not affect the general results of the paper.

Response to the comments of Reviewer 2:

Regarding figure details: We appreciate the difficulties in interpreting the figures as they were, considering the amount of detail described in Figs. 7, 9, 10, 11 (see also similar concerns of Reviewer 1). We have increased the readability of the wavelet figures by focussing on the relevant timescales 2-240 min, as suggested, as well as increasing the font sizes of the labels in each of these figures.

Page 7 line 14: The reviewer raises a valid point in potential complications of using cloud droplets as tracers of vertical air motions. We have discussed in the text that the absolute magnitudes of w derived from this method are uncertain due to the uncertainties, and general small absolute magnitudes of w, of retrieving vertical velocity from radar. As Shupe et al. (2008b) note, the uncertainties in w-retrievals as a function of radar-volume turbulence and horizontal wind speed are second order relative to the mean (positive) bias observed within these estimates. We argue that since we focus on the statistical and spectral characteristics of the vertical motions using FFT transforms in the form of wavelets and power-spectra, the correction for such uncertainties does not impact the timescales of variances or the vertical distribution of w-skewness. Section 2.2 discusses that analyzing the variance, skewness and spectral timescales of w-variance using both a corrected (for mean bias) and uncorrected w-estimate did not affect the results of this study.

Page 11 line 25: We have accounted for this overstatement by toning down the text to read that the median Sw profile shape generally changes from negative to positive with an interface near zn = 0.6-0.7, despite substantial spread in the distributions of Sw.

Page 12 line 1: We have revised the text to emphasize that it is the median Sw profiles that are shown to change sign with height in the cloud layer.

Page 12 line 1: We have removed the notches and discussion around them. We now include the that the non-parametric Wilcoxon Rank sum significance test shows a rejection of the null hypothesis of distributions with equal medians at different heights within the cloud layer.

Page 12 line 21: Delta theta profiles are estimated by taking the difference in theta between adjacent levels both the scanning radiometer and radiosounding native vertical profile resolutions. To combine the full week period statistics we need to normalize the depth of the sub cloud and cloud layers by the boundaries between near surface (first profile height above surface) with cloud base and cloud top. We then combine all delta theta estimates by combining normalized heights into ten bins; the number of observations in each bin will vary depending upon the depth of each layer at the respective scanning radiometer time (10 min) and for 5 min following each radiosonde release. We have included this information on the method in Section 3.3.

Performing the analysis with inclusion of the lower temporal frequency radiosonde delta thetas further supports the general stability changes observed with the higher frequency scanning radiometer. We do note now in the text the uncertainties in temperature retrieval from the scanning radiometer, which are stated to have a bias less than -0.2 degrees C. Thus the median distributions of delta theta observed are within the uncertainty and we can no longer state there are significant differences in the median values when comparing different heights in the layers. However, the generally changing stability profile with height in both the sub-cloud and cloud layers in both the radiosonde and scanning radiometer profiles suggests these transitions in stability are robust - confirmed by tests of the null hypothesis of equal medians at two heights where changing stabilities are observed (zn = 0.2 and zn = 0.8) using the Wilcoxon Rank Sum significance test.

Page 13 line 7: We have removed artificial adjectives such as “drastically” from the text as suggested by the reviewer.

Page 13 line 17: This text is no longer included in the revised manuscript.

Page 20 line 4: We thank the reviewer for pointing out this shortcoming. The text has been revised with reference to earlier work addressing the phenomenon of radiative shielding.

Technical corrections: These have been revised following the reviewer's suggestions.

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We thank Reviewer 3 for the positive response to our paper. The reviewer raises two points that are relevant and important to the processes occurring within AMPS.

Response to Reviewer 3's comments:

1. With the the observational data we have, it is difficult, beyond speculation, to argue which processes are primarily responsible for cloud-surface layer decoupling. Such a study would be more suited toward an analysis of LES or cloud-resolving modeling results. The end of August in the high-latitude Arctic Ocean means solar radiation is present throughout the day, even though the solar zenith angles are large (low sun elevation). We do not find any relationship between time of day and coupling state. However, since the sun is above the horizon always, it is possible that cloud layer heating through absorption of solar radiation may contribute to a reduction in TKE production within the cloud layer and cause cloud generated turbulence to be decoupled from surface generated turbulence.

Shupe et al. (2013) and Sotiropoulou et al. (2013), using the same ASCOS observations, conclude that changes in the turbulent fluxes near the surface have little to no connection with the coupling state of the surface and cloud layer. Both studies conclude that when the cloud base is sufficiently low in elevation above the ground (order of few hundred meters), these clouds tend to be coupled with the surface turbulence, and decoupled when the cloud layer is raised higher. Absorption of solar radiation within the cloud may be a cause for cloud base height changes. However, differential vertical advection appears to also be important. We show in case study 1 that the heat and moisture advection above the cloud layer in the morning, in conjunction with a mesoscale frontal passage, caused the cloud layer to rise. Mixing continued below cloud base, but now the cloud was displaced higher above the surface and cloud-generated mixing failed to connect with mixing driven by the surface.

The dominance of decoupling means the moisture source sustaining these clouds must be coming from aloft. If the vertical location of this source changes, the cloud geometric heights must also change in order for the cloud layer to survive. Thus vertical displacements of the cloud layer in response to heat and moisture advection aloft over the sea ice appear to play an important role in whether a coupled or decoupled state occurs. Furthermore, these AMPS are often observed as thin liquid layers with ice crystals falling into the sub-cloud layer. Diabatic effects within the sub-cloud layer may also play a role in stabilizing the cloud layer.

Additionally, in Fig. 13, we show that vertical wind speed shear is common during cases when cloud and surface are coupled. Additional mechanical mixing in the sub-cloud layer may also be contributing to the coupling nature of the system.

As the reviewer notes, we find radiative shielding, as well as mesoscale weather changes, to be an important factor modifying the timescales of cloud-generated w-variance and surface-cloud coupling state. Radiative shielding from additional cloud layers above, as well as mesoscale passages, are signatures of ongoing thermodynamic advection.

We have included a summary of these processes in bullet point one of the conclusions Section 7.

2. Following the reviewer's suggestion, we have highlighted some of the major differences between AMPS and lower-latitude stratocumulus and the concluding reasons we observe for such differences.