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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This article describes the use of a suite of sensors to to derive and characterize in-cloud vertical motions for Arctic mixed-phase stratocumulus (AMPS), a persistent cloud type in the summer and fall. This cloud type has a substantial impact on the atmosphere and surface radiative budgets in the Arctic and also influences sea ice formation in the fall, thus better understanding the motions within these clouds and what influences these motions is of import.

The authors use surface-based data from ASCOS to investigate profiles of vertical velocity variance and skewness, conduct wavelet and power spectral analyses on the dominant horizontal and vertical scales of motion, and examine how these character-

istics are influenced by the extent of decoupling of the stratcoumulus to the surface.

The results enclosed in and subsequent conlusions of this study were very interesting, and to the knowledge of this reader the techniques applied to the ASCOS data have not been applied previous to AMPS. While I really enjoyed reading this manuscript, I feel it manuscript could be improved in a few important ways:

1). There is a lack of focus at times. In the results section (Sections 3, 4 and 5) I sometimes felt bogged down in details and could not easily relate those details back to what gaps in our scientific knowledge they were filling. Thus the manuscript sometimes reads as a list of results that, while interesting, leave the reader wondering why they are reading about those specific results.

The authors do an excellent job of summarizing the past studies of AMPS in the introduction, and note that vertical motions within them are not generally well understood. Perhaps to the introduction yo can add a bulleted list of questions (three or four) about vertical motions in AMPS that are already examined and answered within the analysis. These could be referenced throughout the manuscript and then referred to in you bulleted conclusions (Section 7). This reframing might bring more focus to the manuscript.

For example, two of these questions clearly is 'what does skewness look like in these clouds?' and 'what processes might be associated with these skewness profiles?' (related to the temperature inversion inside the cloud layer and cloud-top rad cooling, net buoyancy production is mid-cloud). Another quesiton might be what drives vertical motions within AMPS in decoupled or loosely-coupled states.

An additional way to bring focus to the manuscript would be to weave some (or all) of the content of the discussion into the results sections. By citing references relevant to the results, noting previous agreement with the current study or where the current study fills a gap in knowledge, would help focus the reader's interepretations. Disconnecting the references from the figures and results was not helpful for me in this regard, and some of the discussion is repeated from the introduction anyway.

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2.) There was little discussion about quantified uncertainty in the MMCR retrievals of vertical velocity, and how those uncertainties might propogate into uncertainty for skewness, power spectral analyses, etc. A full error analysis would be quite an undertaking and is probably beyond the scope of this study. However these uncertainties have no doubt been characterized in previously cited studies (e.g. Shupe et al. 2008b), so at least acknowledging them and how they might limit interepretation of the results is important (perhaps in section 2.2?).

3.) Figures 7, 9, 10 and 11 are all very busy figures with incredibly small fonts. This reader had to blow up the manuscript to 300% in order to gain any understanding about the results enclosed, and at times investigated parts of the figures that were not referenced within the manuscript. I certainly appreciate seeing all the data created, but perhaps some of the information enclosed could be shuffled to supplementary material.

Alternatively, consider breaking them up into a few figures so that their sizes can be increased and readability can be enhanced, unless there are compelling reasons for keeping the figures as 6 or 5-panel plots. Or consider cropping the results on the figures. For example, in figures 7 and 9 the wavelet information for frequencies less than two minutes are not discussed; is that information necessary to keep in the figure? Also consider highlighting the portions of these figures that you reference in the manuscript (square or circle regions).

The fonts need to be increased in size in any case.

Smaller comments throughout the manuscript:

In the abstract and conclusions, you note finding a postively-correlated vertical motion signal is found; do you mean that it is positively-correlated with itself?? To what is it correlated? Maybe I'm just dense...

Introduction line 12: 'cooler surface temperature'; cooler than what? If you use a comparative, you must say what we are comparing to. Otherwise just say 'cool.' This

issue involving comparatives exists in numerous other places within the manuscript.

p. 31082, line 3: say ' an understanding of the vertical motions within quasi-persistent'

p. 31085 – the first paragraph suggests that not focusing on the absolute value of vertical velocity retrievals will allow you to avoid worrying too much about biases. However, the next paragraph starts with a discussion about velocity biases. The contrast was confusing; perhaps a different introduction to the paragraph following is in order?

p. 31085, line 8 – 'themself' to 'themselves'

p. 31086, line 3 – From where does this a priori dataset originate? Citation?

p. 31087, line 12 – That the physical characteristics of this cloud layer bear a strong resemblance to those in others' observational studies is a very broad assertion; probably best to leave it out.

p. 31087, line 20 – change 'cloud' to 'AMPS'

p. 31088, line 7 – elaborate on why 20 minute running windows were chosen.

p. 31089 line 11 – You use equation (2) for normalizing height profiles outside of clouds too (e.g. Figure 5b), so define Z_n accordingly.

p. 31090 line 5 – 9; the sentence is awkard, suggest rewording.

p. 31091, line 29; say 'net absorption' instead of just 'absorption'

p. 31092, line 5 – I'm not sure what '75% of the time' means; does this mean only one out of four large-eddy downdrafts breach this layer?

p. 31094, line 22 – From Figure 7a, it seems to me the largest spectral peaks are for a frequency of > 60 minutes for some time...

p. 31097, line 20 – Change 'at all cloud levels' to 'at all three cloud levels' since you only conducted wavelet analysis at three cloud levels.

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p. 31097, line 29 – 'longer timescales' relative to what? To clouds that are not radiatively shielded from above?

p. 31099, line 3 – Cite from which figures you are determining these aspect ratios.

p. 31100, line 2 – Label these figure references with their appropriate letters as well.

p. 31101, line 1 – I don's see that this statement is true from the 'above analysis'; please elaborate on why this is the case.

p. 31101, line 20 – I'm concerned about the correctness of this particular analysis. You can infer the MEAN cloud LWC from LWP/delta-z, but you don't automatically know what the vertical distribution of the cloud LWC is, and it seems that this distribution is important. Are you assuming that cloud LWC is adiabatically distributed by height? This doesn't seem to me to be a good assumption since the cloud layer exists within a temperature inversion....what am I missing here? I think more elaboration on this analysis is needed before I can follow the related conclusions.

p. 31102, line 6 – This analysis is interesting, but ignores directional wind shear altogether and focuses only on speed wind shear. Directonal wind shear could also lead to vertical momentum flux across the cloud base and the below-cloud region. Are you arguing that directional wind is negligible for these observations?

p. 31103, line 7 – I had a hard time following this sentence ('coupled in-cloud w-variance'?)

p. 31103, line 25 – State what season and region was studied in Shupe et al. 2008a

p. 31106, line 9 – This conclusion is well, well known from the literature and as written is not particular to AMPS. Make it particular to analysis of AMPS in this study, and if this is in agreement with other studies make sure to state so.

p. 31106, line 13 – To me this is a central conclusion, where you work to explain why the skewness profiles look the way they do! Good stuff.

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p. 31106, line 25 – Remove 'Despite these profile shapes.' If you are going to write your conclusions in bullet form there is no need to connect the thoughts this way.

p. 31107, line 12 – As I say above, I would like to see more explanation of how you have come to these conclusions.

p. 31107, line 27 – The last sentence is a bit of a run-on, I suggest splitting it in two.

Regarding the Figures

Figure 4 – make it clear that your parenthetical phrase refers to the horizonal layers above and below the layer of interest.

Figures 7d, 7e and 9 d,e and f - S_w is not skewness, it's power spectral density. Be sure the labels reflect this!

Figure 7d has a different horizontal wavelength scale than Figures 7e and 7f.

Figure 11 and 12 – RFD has not been defined (is it regional frequency distribution?), and on Figure 11 I think you mean to use cumulative frequency distribution.

Figure 13 has a typo (physical)

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

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