

Interactive
Comment

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

Anonymous Referee #2

Received and published: 13 January 2014

General Comments:

This is a very well-written paper that explores the dependence of cloud vertical motion characteristics in deep Arctic mixed-phase stratiform clouds. The authors show that the vertical velocity structure in the upper layers of these clouds differ from their counterpart mid-latitude marine stratocumulus clouds, even though the dynamics of both cloud systems are primarily driven by radiative cooling at cloud top. They attribute this difference to Arctic mixed-phase stratiform clouds existing up into the capping inversion. The results suggest that even though the characteristics of the circulations in the inversion layer differ from that in the mixed layer below, those circulations are vertically coherent through the cloud depth. The mixed layered extended below cloud base throughout the period, but these summer-time clouds were largely decoupled from the

C11010

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



surface.

Specific Comments:

Many of the figures have details so small that this reviewer had to look at several of the figures in the document at 400% magnification to follow the text (particularly the wavelet output). I understand you are trying to keep the number of figures low, but please help the reader. The wavelet figures may be improved somewhat by limiting the range of periods, say from 2 – 300 minutes.

Page 7 line 14: The error in using the cloud droplets as tracers is not a single offset. The standard deviation about the offset is about on the same order of magnitude as the vertical velocities, and a function of the radar volume liquid water content, turbulence and shear. These may well be expected to vary from up- to downdrafts. The technique described here may very well be sensitive to it. That said, I do not expect changes to the fundamental conclusions drawn from the analysis as a result.

Page 11 line 25: "... clearly depict an interface between positive and negative skewness" is an overstatement. A large part of the cases have positive skewness as low as 0.4, and all cloud heights have positive skewness some of the time.

Page 12 line 1: In light of your "clearly" statement (see comment above), please comment about the positive median just about cloud base.

Page 12 line 1: I know this makes your point, but I cannot see the notch in the figure. Please comment to that effect in the text; I was trying to see what I miss. Please specify the test you used to determine the 99% confidence interval.

Page 12 line 21: I don't understand how you calculated the delta potential temperature, and what the uncertainty is in the determination of the temperature from the microwave radiometer. How much is the accuracy impacted when crossing cloud boundaries? The only citation given is personal communication. If I read the text correctly, the radiometer only provide input to 1.2 km, so above that you only have data from the six hourly

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



soundings, interpolated to the radar times. Looking at a few of the days reported in this paper (figure 6 say) I see cloud base height (and thetae profile) changing substantially in the six hours between the two soundings. Surely an interpolated sounding must be largely in error because you will get physically unrealistic situations? Have you performed an error analysis to see if one can have any confidence in any of the results reported? At minimum please put profiles for the compilation of say only the 10 minutes surrounding all the radiosondes to bolster your conclusions. The meaning of the delta pot temp must depend on the depth of the cloud, as the delta z will vary in the normalized depth. Does it make more sense to look at $d\theta/dz$ at normalized heights?

Page 13 line 7: Please avoid overstatements such as “drastically”. It is in the eye of the beholder. State numbers. Also, see comment above.

Page 13 line 17: “more rapidly.” State numbers.

Page 20 line 4: I don't think Shupe et al. 2013 was the first paper to refer to radiative shielding. (I spend 30 seconds to find a 1957 paper that refers to the same effect.) This is textbook material.

Technical Corrections:

Abstract line 11: Latitudes Page 16 line 17: “..suggestive of warm of air advection”

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper