Response to Review #2 of "Moisture and Dynamical Interactions Maintaining Decoupled Arctic Mixed-Phase Stratocumulus in the Presence of a Humidity Inversion
(Reviewer's comments in blue, our response in black)

Major comments:
The authors conducted nested LES simulations of decoupled Arctic Mixed-Phase Stratocumulus (AMPS) clouds observed during the DOE Atmospheric Radiation Measurement Program's Indirect and SemiDirect Aerosol Campaign (ISDAC) to analyze budgets of water components, potential temperature, and turbulent kinetic energy. Through those analyses, the processes that maintain the decoupled are quantified. This kind of thorough analysis of the cloud and environmental fields is very useful for us to better understand AMPS clouds, although the quantification of the processes is kind of case dependent. This reviewer believe some qualitative conclusions such as the importance of the humidity inversion at cloud top to maintain the mixed-phase clouds and the roles of longwave cooling may be applied to other AMPS clouds. Before it can be accepted as a publication in ACP, some revisions are recommended.

1. My major comment is that the case has not been fully evaluated by the observations. Since the focus of the paper is in quantifying microphysical, dynamics and radiative processes through budget analysis, it is very important to show the simulated cloud case reproduce the observed cloud properties, especially in cloud microphysical properties and radiative fluxes. With ISDAC field campaign, the big advantage is that the in-situ aircraft measurements of cloud properties are available to validate cloud simulations. Therefore, this reviewer would like to see the comparisons of the simulated LWC, IWC and droplet number concentrations compared with the aircraft measurements.
We have added a brief discussion of the model NC,NI,LWC,IWC compared to the April 8 aircraft flight 16 measurements to the paper. We have added Figure 1 below showing NC,NI,LWC,IWC along the aircraft track at 23Z from the 1km nest to the supplement. The added text is:

"A research flight was conducted on April 8 with the National Research Council of Canada Convair-580 (McFarquhar et al. 2011). Between 22:27 and 23 UTC a series of ramped ascents and descents through the cloud layer were executed between Barrow and 71.8N,160W. Avramov et al. (2011) Figure 10 shows vertical profiles of horizontally averaged NI (d>100 microns), NC, IWC and LWC measured during this flight. In the supplement we have included a figure of NI (d>100 microns), NC, IWC, and LWC averaged along the aircraft track at 23Z from the 1km nest (cut off at 500 meters similar to the measurements) (shown below as Figure 1). Comparing the measured and modeled profiles it is seen that the model NI is ~0.75/L from the base of the cloud to 0.5km, which is larger than the observational estimate, but within a factor of two of the mean. NC is essentially determined by the aerosol concentration specified in the model and is on the high end but within the range of observational estimates. As discussed in Avramov et al. (2011), IWC can vary by at least a factor of five when habit is taken into account. Given this, our peak mean values of ~0.018 gm^{-3} above 500 meters may be low relative to the baseline estimates of IWC but potentially consistent with the observational estimate when habit is taken into account. Modeled LWC is at the high end of the observational estimate and mean values are

50% larger than measured mean values. Considering the uncertainties associated with the aircraft measurements (McFarquhar et al. 2011, Avramov et al. 2011) and with making time-space comparisons, the model results are deemed to be in reasonable agreement with the aircraft measurements."

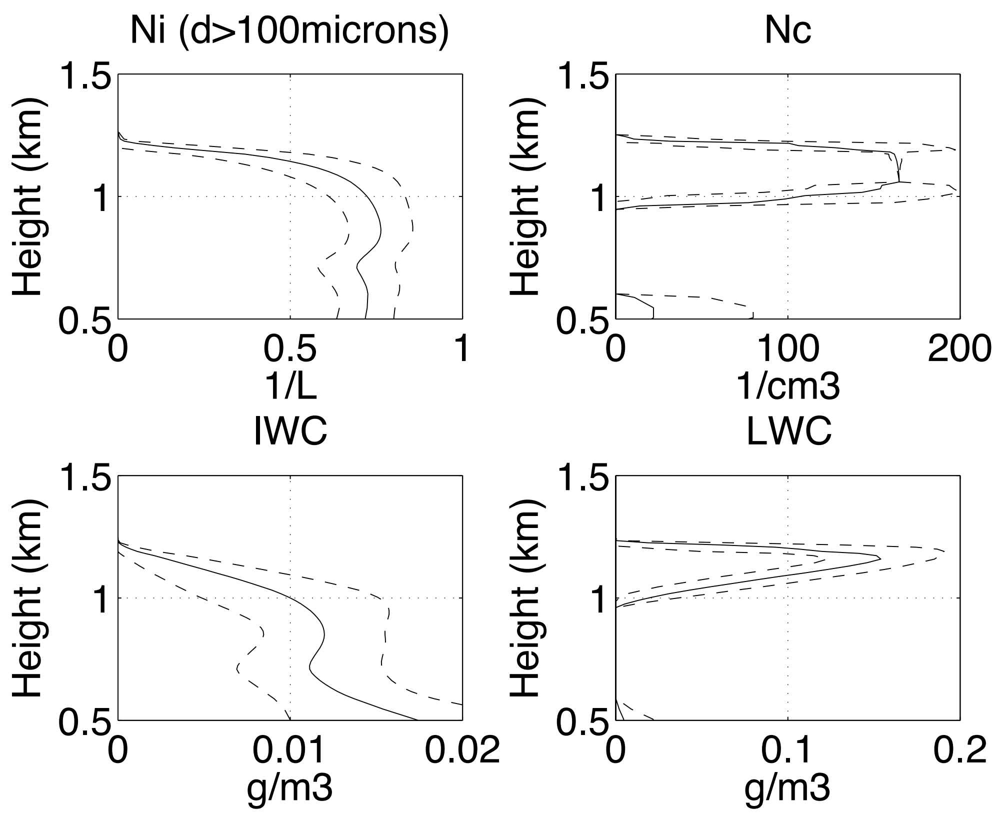

Figure 1: Mean +/- one standard deviation NI, NC, IWC, and LWC along the aircraft track at 23Z from 1km nest.

2. In addition, based on my impression, the cloud top and base heights from the aircraft measurements are much lower than the simulated values in this study.

Yes, this is true. The large scale forcing for our model simulation has a time delay relative to observations. This causes the cloud at Barrow to start descending at approximately 20Z in the model instead of 18Z. In our nested WRF simulations the large scale is not constrained by observed soundings. Observational reanalyses are used as lateral boundary conditions approximately 900km from Barrow. This causes variability in synoptic conditions at Barrow relative to observations. We have chosen this model setup to allow for feedbacks between the dynamics on cloud-scale and synoptic-scale. The approximately 200 meter difference in the height of the base of the inversion between the model and sounding at 20Z (see our Figure 2) does not change the qualitative characteristics of the mean state, i.e. both have cloud layers that are decoupled from the surface layer.

3. Another main concern is that the authors used IN observation from another case (MPACE) which is a fall time mixed-phase cloud case and aerosol properties could be very different, but no any justification and validation are provided for it. The aircraft measurements of Ni

can be used to justify or validate this assumption.
We have modified the discussion of how IN is parameterized in the model. Yes, we did use the MPACE value of 0.16/L in the parameterization of IN due to condensation freezing and deposition. This parameterization is a relaxation back to a fixed value and is only switched on if NI drops below this fixed value. We ran a few additional simulations increasing this fixed value and found that IN due to contact and immersion freezing caused NI to generally exceed 0.16/L, preventing aerosol freezing from switching on. We found this constant value needed to be increased to greater than 0.7/L to produce IN due to condensation freezing and deposition. This is consistent with the NI~0.7/L averaged along the aircraft track at 23Z in the model for sizes greater than 100 microns, i.e. the value of IN produced by contact+immersion freezing alone. Average NI measured by aircraft between 22.3-23Z was ~0.5/L for sizes greater than 100 microns (see Figure 10 Avramov et al 2011 JGR). Increasing the fixed value for IN in the condensation freezing and deposition nucleation parameterization to 8/L (the average value measured by aircraft 22-22.4Z) resulted in a complete depletion of liquid water in the cloud. These results indicate that it is not appropriate to specify IN=8/L uniformly throughout the column. In addition, a recent study by Fridlind et al. (2011) shows that NI can be orders of magnitude smaller than the above cloud IN concentration, due to loss of IN from nucleation, crystal growth, and sedimentation from the cloud layer. We believe the model simulations used in the paper with NI~0.7/L primarily due to contact+immersion freezing are consistent with the aircraft measurements and, based on the observed NI

4. The writing of the manuscript is kind of sloppy. In section 5, it gives readers an impression that each subsection are randomly put here and they are not coherent with a theme. In addition, I often had a hard time to get a main point from a paragraph after reading it. Also, some of the details are very minor which can be removed. Then, the paper would be much shorter and the discussion would be much more succinct.
See our response to Reviewer 1's comments 1,3,4. We have attempted to improve the readability of the text in a number of ways.

Specific comments:
1. Abstract: "The results show the maintenance of liquid clouds in both the shallow upper entrainment zone. . ., should be "the maintenance of liquid water" or you can say "the maintenance of mixed-phase clouds".
Changed.

2. P 13471: ". . .when the open ocean produces large fluxes of heat and moisture into. . .", the heat and moisture fluxes are never large even if it is open water, especially the heat fluxes. Check the MPACE case. So, "large" is not appropriate here.
"large" has been changed to "relatively large".

3. P 13471: Morrison et al. MPACE study should be Morrison et al. 2009
Changed.

4. P 13473: You spent two paragraphs in discussing the subtropical Sc. I do not your point there (i.e., why it is that important to take such large space). I'd suggest simplifying it with a

couple of sentences.
See our response to Reviewer #1's Specific Comment #1.

5. P 13473: You can not jump to Figure 5 from Figure 1 without introducing Figures 2-4 when they appear at the first time.
The figures have been reordered.

6. P 13474: ln 15-17, the logic of these two sentences does not sound right. Please reword.
This sentence has been removed.

7. P 13476: ". . .with AMPS near the base and up into the inversion. . .": do you mean cloud base? If so, you need to be specific and also need to give the value of the height.
This sentence has been changed to read, "At 18Z, the interpolated temperature profile shows a sharp (~3K) inversion at ~1 km, with maximum LWC at the base of the inversion and liquid water extending up into the inversion by approximately 100 m (Figure 2a)."

8. P 13476: ln 21-25: did you check the correlation between LWC and IWC from aircraft observations and your simulation? Basically the correlation between LWP and IWP does not mean much since vertically the LWC and IWC maximum at very different altitudes in the mixed layer. In many single mixed phase clouds, correlation between LWC and IWC is very poor from both observations and simulations.
We agree with the reviewer that the correlation of LWC and IWC from aircraft measurements is likely not expected because maximum values occur at different heights. However, the ground-based instruments provide a vertically-integrated perspective on the cloud properties. Since the circulations in the cloud are vertically coherent (i.e. they are up or down) and the ice crystals fall vertically, there is reason to believe that in some cases the liquid and ice might be correlated. Indeed that was observed in this case.

9. P 13477: ln 21-24: what is the justification to use the IN observations from the MPACE field campaign, which was from a very different season? IN measurements are also available for ISDAC. In addition, it is even better justified to set the IN based on the in-situ observed ice crystal number concentrations than using the data from MPACE. This could be the one of reasons why the simulated cloud ice is underestimated much since IN in the ISDAC (spring) could be much higher than MPACE (fall).
See our response to your major comment #3.

10. P 13478: ln 7-8: The sentence is misleading. Do you mean besides ammonium bisulfate there is 30% for the other component? If it is, what is the component (generally it would be organics). If not, you can not assume 30% of ammonium bisulfate is insoluble since it is an soluble component. In addition, did you really account for the insoluble composition for your droplet activation (this is the purpose to consider the composition in cloud models)?
We assume that 30% by volume is insoluble (without specification of its actual composition, which is not relevant), and the other 70% is soluble, consisting of ammonium bisulfate. This has been clarified in the revised manuscript. Yes, we did account for the insoluble component in the droplet activation parameterization.

11. Please add one or two sentences to state the necessity for the details of eqs in p13479-80.
Done.

12. P 13481: first paragraph, the retrieved IWC has larger uncertainty. You should compare with aircraft data for LWC, Nc, IWC and Ni. Since you are using MPACE IN measurements, it is important to look at the modeled Ni to see if they are close to the observed values (for size larger than 200 microns).
See our response to major comments 1 and 3.

13. Section 5.1, again, aircraft measurements should be compared. In addition, I do not quite understand the last sentence of this section.
We have added a comparison with aircraft data as suggested.

14. P 13481: ln 8-10, please check if it is ice-supersaturated near the surface. Looks like you got a thick moist layer there (300 m) from Figure 7. is it real?
As seen in Fig 5, both the model and soundings show a moist layer near the surface. What is unrealistic in our model simulation is the relatively dry layer ~600m. There is an indication of this in the sounding but it is more pronounced in the model simulations.

15. P 13498: last sentence of (3), that is a common feature for the mixed-phase or even ice phase clouds, and it is nothing special or new.
Yes, but it is part of the general picture we are describing so we state it here.

16. Figure 5, suggest to plot the model and observed results in the same panel.
Thank you, but we think this figure is clearer as is.

17. Figure 8 and other figures, what do you mean about total cloud domain? Do you mean cloudy points in the entire domain? But in the text it is said averaged over the square box in Figure 6?
We have changed “cloud domain” to “square box”.

18. Figure 9, where is the gray shading in the figure?
Gray shading in all of the figures indicates entrainment zones and is noted in the figure captions. We have added red dashed lines to indicate the cloud top and base.

19. Suggest changing “Water” in figure legends to “Liquid”.
Done.