Review #1 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)

Overview:
This article provides an interesting look at the moisture budgets governing Arctic mixed phase stratiform clouds using LES as an avenue for deriving these estimates. Overall, I think this provides a unique view of a specific cloud layer. As the authors point out, these results are dependent upon the particular model used and the assumptions that go into the parameterizations of that model. In general, I think that this article is in pretty good shape, though I would like to see the article be easier to read. Suggestions for this are included below.

General Comments:
Content:
1. I think that it would be very helpful to include a clear diagram labeling the different layers within the atmosphere, and then sticking to the labels introduced in this diagram. This is done to some extent in Figure 11, with "Upper Entrainment Zone", "Mixed Layer" and "Lower Entrainment Zone" labels. However, there are points in the article where the authors refer to layers differently... For example, in line 5 of page 13494 – "cloud layer" – does this include both of the entrainment zones (see my note on this section below in minor comments)?
We have gone through the text to make sure we stick to the labels specified in Figure 11. However, in addition to the labels in Figure 11 we also need to refer to the cloud layer. To prevent any confusion we have added a sentence to the caption in Figure 11, "Note that the cloud layer is primarily in the mixed layer and extends approximately 50 meters into the upper entrainment zone." We have added red dashed lines to the figures to indicate the cloud top and base. In addition, we have added a description of the layers in the abstract, "Key structural features include a shallow upper entrainment zone at cloud top that is located within the temperature and humidity inversions, a mixed layer driven by cloud-top cooling that extends from the base of the upper entrainment zone to below cloud base, and a lower entrainment zone at the base of the mixed layer. The surface layer below the lower entrainment zone is decoupled from the cloud mixed-layer system. Budget results show that cloud liquid water is maintained in the upper entrainment zone near cloud top (within a temperature and humidity inversion) due to a down gradient transport of water vapor by turbulent fluxes into the cloud layer from above and direct condensation forced by radiative cooling.".

2. Also, while I see the rationale behind including all of the integrative statements in the summary/discussion section, it makes the results section quite dry (no pun intended... See more on this below). It would be nice to sprinkle some statements of relevance throughout the results section, to keep the reader in tune with the authors' way of thinking as results are presented.
We have rewritten the results section to address this concern. For example, we have changed the text to read, " Within the entrainment zone there is down gradient mixing by turbulent eddies that causes an upward (downward) transport of cloud liquid water (water vapor) above the liquid water maximum at the base of the upper entrainment

zone (top of the mixed layer), and oppositely below. In terms of total water, entrainment moistens the cloud layer in the lower part of the upper entrainment zone (Fig. 14b). ".

Readability:
3. I found this article rather difficult/tedious to read. This is due to a couple of things – for one, I think that there are a lot of results presented without any deeper insight into what they may mean. This is particularly true in section 5.3. As an example of what I mean, on page 13489, there is a paragraph that starts on line 9: "At the top boundary of the upper entrainment zone there is a decrease in total water of -1 g m-3 day-1 (Figure 13a), with the dominant term being the vertical turbulent advection of water vapor (WP in Figure 14b). Within the entrainment zone there is down gradient mixing of both water vapor and cloud liquid water such that turbulent vertical advection within the entrainment layer increases (decreases) cloud liquid water (water vapor) above 1.27 km and oppositely below Figure 14b and 14c). Tendencies due to subgrid-scale mixing (RES in figures) are of the same order as mean vertical advection for vapor in the upper entrainment zone. Sedimentation, which is the microphysics term in Figures 13a and 14a because phase transitions conserve total water, is a maximum within the upper entrainment zone due to the fallout of primarily liquid water and some ice within the entrainment zone into the mixed layer..."
This reads as a list of results: a, b, c, d, e... All of this is displayed in the figures, as is pointed out, and I think it would be very helpful to the article if the authors take a close look at sections 5.3 and 5.4 and limit their presentation of results to those that are important for development of arguments that they present in the summary/discussion section.
We appreciate that this paper, which is focused on budgets, may be rather difficult to read. We have revised the paragraph quoted above to include symbols for terms in the equation. However, the point of this paper is to quantify the balances within the AMPS and therefore the details included in the text are necessary to understand the budgets displayed in the figures. We have attempted to add statements when possible that identify the context/significance of specific results.

4. Another part of what makes this a bear to read is the repetitive nature of the writing. For example, in the paragraph starting on line 3 on page 13490 ("Figure 15a,b shows the vertical resolved..."), the word "flux" is used 19 times, and "mean", "vertical" and "water" are all used 12 times each. That's roughly 1/5 of the total words in the paragraph! This sort of repetition is found throughout the article, sometimes justifiably, and other times not. To illustrate this I performed a quick evaluation of word frequency, and out of the _15600 words in the article, "the", "of", "and" and "in" make up 2209 of them (974, 472, 422 and 341, respectively). "Water", "cloud", "layer", "mixed", "entrainment" and "vertical" are used 265, 238, 220, 140, 122 and 100 times, respectively. I would strongly encourage the authors to go through their manuscript, remove any unnecessary words ("the" is often overused – as a test, try the sentence without "the" and if it still sounds ok, remove it!). For example, instead of "...due to the fallout of primarily liquid water...", you can use "due to fallout of primarily liquid water...". Also, perhaps it would help to shorten/combine some currently long statements. Instead of "...above the base of the lower entrainment zone...", try "...above the lower entrainment zone base...".
We have gone through the paper and edited it following these suggestions.

5. Another suggestion would be to use symbols instead of the fully written version of every variable. I realize that too many greek letters can also reduce the readability of a manuscript, but there is likely a happy medium somewhere. There are good results in this text, and it's a shame to have them get lost in a (long) list of results from the simulations.
Thank you for the suggestion but we don't think this is necessary. We have added further use of symbols and think that the text now has a better balance of these.

Specific Comments:
1. I realize that there has been a lot of work done to understand subtropical stratocumulus, but I did not quite understand why this work deserved 3-4 paragraphs worth of discussion in the introduction. Perhaps it would be better to integrate this information into the discussion of the current results, where appropriate. I believe that additional information into the formation of precipitation and moisture budgets in mixed-phase clouds, such as that available in early papers by Rangno and Hobbs, Curry, and Pinto is more relevant in the introduction of this particular paper.
We feel the discussion of subtropical stratocumulus needed to be included in the introduction since the general understanding of the maintenance and persistence of stratocumulus is based on studies focused on cloud systems observed in the subtropics. For example, LES simulations of Arctic stratocumulus assume subsidence at cloud top based on observations of subtropical systems. This may not be a valid assumption in the Arctic! In fact, we show this would not be valid assumption when modeling the single layer AMPS observed during ISDAC. We wanted to compare and contrast Arctic and subtropical systems to make this point clear and to highlight a few of the issues that we will be focusing on in our budget studies. Also, we do include a discussion of Curry and Pinto papers in the introduction and throughout the paper.

2. I think that it would be very helpful to include some sort of information on the evolution of the atmospheric state from clear to cloudy, and how the moisture inversion is maintained/ created in this transition. For example, if the decoupled clouds form through radiative cooling of the atmosphere, and the pre-cloud atmosphere is characterized by a surface-based inversion extending to the eventual cloudy altitude, how does this influence the moisture inversion? Do we have any understanding of how these form?
This is a very interesting topic that we intend to focus on in a future study. It is beyond the scope of this paper to include studies of the evolution of clear to cloudy states, as here we are primarily focused on the budgets within existing cloud layers.

3. What is the source of the "trace precipitation" measurement discussed on page 13476 (line 4)?
We have added the text, "…but only trace precipitation was observed at the surface by the U.S. Climate Reference Network station near Barrow.".

4. What are the parameterizations utilized in this particular model to include the ice initiation mechanisms discussed on page 13477 (line 20)? Also, I'm assuming that "aerosol freezing" is deposition freezing?
"Aerosol freezing" has been changed to "deposition nucleation". We have added "(see

Morrison and Pinto (2005) for details on parameterizations used in the model).

5. How is supersaturation determined in this model? Is this done iteratively?
Supersaturation is calculated explicitly in the model for the ice phase, while saturation adjustment is used for the liquid phase. This is pretty standard for most bulk microphysics schemes used in CRM and LES models (e.g, in WRF).

6. On line 8 of page 13478, is that 30% fraction by mass? Or by volume?
The soluble fraction is 70% by volume. This is clarified in the revised manuscript

7. What ice habits are assumed in the model? How could this influence the evolution of precipitation/sedimentation and the subsequent removal of water mass?
The model assumes spheres for most processes and parameter calculations (e.g., collection), although it is not completely consistent in its assumptions of habit for all processes. For example, the fallspeed parameters for snow are for assemblages of aggregates, bullets, etc. from Locatelli and Hobbs (1974). Again, this is common in most bulk schemes, although our recent work (e.g., Morrison and Grabowski 2008, JAS) has begun to address these inconsistencies. Habit assumptions, especially in terms of impact on fallspeed and sedimentation, could certainly impact removal of water mass. However, given the length of the paper we feel that detailed discussion of this point is beyond its scope.

8. Is the "relatively moist surface layer" discussed in line 10 of page 13483 the result of a surface temperature inversion?
No, it is due to evaporation near the surface (see original Figure 5, now Figure 2).

9. Line 16, page 13488: "The notation below (Eq. 6)..." should read "The notation above (Eq. 6)...".
We have changed the text to read, "The notation below (6)…".

10. Lines 5-9, page 13494: Is this backwards? The authors state that "Within the cloud layer, in downdrafts, opposite vertical gradients of cloud liquid water and water vapor cause an increase (decrease) in cloud water (water vapor)...". When I look at figure 17, I see a negative Qc in the mixed-layer for downdraft areas (which makes sense to me). This doesn't seem to match what is implied by the statement. This is where clearly defining "cloud layer" is important.
The sentence in the text is correct. The net tendency in Qc is negative. The tendency due to advection is positive and the tendency due to microphysics is negative (and dominates).

11. Line 10, page 13495,: Personal preference, maybe, but I prefer 17:36Z over 17.6Z
We have changed the text following this suggestion.

12. The overview of the five distinct layers in lines 13-19 would be good to have in the introduction (or somewhere earlier in the text). Maybe it would be good to include this in figure form.
This information is included earlier in the text, lines 14-25 page 13484.

13. Line 28, page 13496: The cloud persistence time is calculated to 6.7 hours – how close was this to the actual persistence time of the cloud? Can you make any statements about your derived budgets through comparison of your persistence to that of the observed atmosphere?
It is difficult to make inferences from the observations since locally, at Barrow, the synoptic conditions were constantly changing. For example, there is a systematic decrease in the height of the cloud layer such that, based on turbulence measurements from radar, the cloud layer becomes coupled to the surface layer by 4Z April 9, thus likely changing the budgets at that time.

14. Line 19, page 13498: I think that this discussion on model-dependence is important and relevant, and am glad that the authors include it. Having said that, I would like to see it expanded somewhat. Are there specific model traits that could introduce large differences? How have the "configuration and physics impacting cloud" been carefully chosen? What does this mean? Can the authors provide any discussion on how the underestimation of ice can still result in "the most realistic evolution possible"? First, is this underestimation of ice mass, or number? Does this discrepancy imply an inherent moisture budget bias? In other words, is there something in the moisture budget compensating for the error in ice production? If so, that would seem relevant to discuss in detail, since this budget is the topic of the paper! If not, how do you still get the realistic cloud?
The configuration impacting the cloud refers to the increased vertical resolution in the mixed layer and entrainment zones. Coarser vertical resolution results in increased entrainment efficiency. The sentence "Both the configuration and the physics impacting the cloud have been carefully chosen to produce the most realistic evolution possible." Has been removed since we did not do extensive testing of the model physics options to get the most realistic simulations possible. In terms of the underestimate of ice and the potential impact on the moisture budget, please refer to our responses to Reviewer #2's major comments 1 and 3.

15. Figure 7: Please use vertical coordinates that are consistent with those used in the rest of the paper (k, not hPa).
We use pressure on the left and height on the right (sorry this label was missing). The right y-axis label has been added to the figure.

16. Is it possible to combine the information from tables 3,4 into a "budget figure", something similar to the atmospheric energy budget figure in Trenberth et al.? If not, perhaps just a combination of the two tables into one would be helpful – no need to change much, but I don't see any need for them to be separate from one another. Conversely, in order to compare the quantities for the upper entrainment zone and the mixed layer, it would be helpful to have the tables combined.
We spent many hours working to combine these tables but budgets for the different water states are not straightforward since both advection and microphysics contribute to the tendencies. Therefore, fluxes in and out and the boundaries of our layers don't necessary tell you about the mass flux into the layer since there can be a conversion from one state to another within the layer. Because of this we decided to have two tables.

17. Is table 1 necessary? If so, I would think it may also be interesting to include the microphysics in there. Personally, I’m not totally convinced it’s necessary at all.

We have added the microphysics in Table 1. This table is necessary to indicate the specific model setup use for the experiments, without taking up too much space in the primary body of text.