Anonymous Referee #1

The authors appreciate the encouraging, thoughtful and helpful comments and suggestions regarding the manuscript by the reviewer, and appreciate the time and effort spent on it.

This is a study of the relationships among cloud properties as retrieved by satellites and meteorological fields from MERRA. Most of the effort is in producing a dataset that combines AIRS/AMSU and MODIS data with MERRA at the smallest possible space and time scales. This effort is commendable and valuable, combining these data at small scales can potentially reveal a lot about the relationships between clouds and their environment. The analysis divides the data set into the four subtropical stratocumulus regions, though bigger than Klein-Hartmann regions to focus more on broken cloud regimes. Numerous quantities are examined, especially through the use of joint distributions and conditionally averaged quantities. This is mostly effective, but the weakness of the paper is that it meanders through the results without a lot of focus which I think will lose a lot of readers.

Since reviewer #2 had similar thoughts on the lack of focus in section 4, we will carefully reorganize the flow of the paper per the suggestions of reviewer #1 below.

My main suggestion is to re-work section 4, but there are probably a couple of different ways that could be done. I will include some detailed comments, some of which might become irrelevant depending on how the manuscript changes in revision.

We will make some major changes to section 4 as suggested by the reviewer below in the detailed comments.

COMMENTS:

1. While the methodology overall seems very good, I have some concerns about sample sizes and statistics. The choice to only look at 2009 must be motivated by the effort expended to gather the raw data and process down to the combined data that is being used here. Very understandable, but it is not clear whether one season of the final combined data is enough to say much. This issue might be resolved with a few words about how many samples are actually retained. This comment definitely applies to the joint pdfs, too. Are the color bars different for the different regions, and how much data is in a black region compared to a white region?

This is a good point. We were indeed limited by the sheer volume of data and processing required. We have processed the full year of 2009 for the entire globe. While we initially debated about presenting the full 2009 record, we felt as though the seasonal story would get lost in the story about the four regions. We also could not decide on a simple set of figures, bar charts, or tables that might show the seasonal variability (that could potentially quadruple, or more, the figures and panels). Also, we note that the seasonal variability is sensitive to the latitude and

longitude of the region selected. For instance, in the SEP region of study, during DJF (local summer), convection and moist intrusions impede on the northern side of the region and that causes systematic changes in portions of the joint pdfs. We concluded that we stood on the most firm ground by choosing JJA in the NH and SON in the SH that correspond with peak cloud frequency as shown in Klein and Hartmann (1993).

In the revision we will add a table that shows the raw pixel counts (or grid counts for MERRA) for all data that go into the joint pdfs.

The gray scale of the joint pdfs that show the counts was settled on after many revisions and ideas. It indicates the log(count), where black is log(3) and white goes to log(8) or so. We will add a single gray scale bar at the bottom of every joint pdf figure for clarity.

2. Using three moments of the reflectance is interesting, but the physical interpretation gets lost in the text. It is made clear that skewness increases in cumulus regimes as ECF drops. Is the interpretation that this is a measure of cloud size? The standard deviation of reflectance seems to be connected to the boundary layer depth (Fig 8 & Page 9). Is that expected? The standard deviation isn't used much except to make this point, and it is not clear that it adds much to the overall story. Maybe it would be worth extracting the standard deviation of reflectance into supplemental material?

We lost reviewer #2 in regards to the connection (poorly) made between cloud organization and skewness. We will revise the discussion of the moments accordingly in the revision and will be clearer about how they connect to the other variables.

With regard to the interpretation of reduced ECF because of smaller cloud size, that is a great question but we cannot provide a firm answer. There could be several causes. First, if the cloud opacity is reduced, the ECF will go down even though the cloud coverage remains constant over the entire AIRS pixel. That is because the ECF is a convolution of emissivity and cloud fraction. Second, it is possible that the emissivity remains fixed but the cloud coverage becomes more broken, also reducing ECF. Third, if the ECF is further reduced (increased), it could be that the small cloud elements could be more widely spaced (packed together) even though the cloud size may be the same. Fourth, it is likely a combination of the first three factors in different combinations depending on the cloud scene and time period.

With regard to the standard deviation, in the few uncommon cases with very high standard deviation (note the blacker shading), that the MBL is quite a bit deeper than in the mean and skewness dimensions. These cases are aligned with the highest values of RH in the standard deviation seen in Figure 10 for the SEP. We agree that the standard deviation was not sufficiently teased out in the text. We can speculate about the causes, but in lieu of careful investigation beyond the scope of this work, probably the best approach is to move the standard deviations in Figure 8 to the

Appendix.

3. Section 4.2 is lacking. If I understand correctly, the point of this section is to whittle down the number of variables to look at in the later sections, settling on reflectance and ECF as the "phase space" (or maybe the "independent" or "predictor" variables?). The weakness is that the selection seems to be mostly arbitrary rather than by systematic evaluation. In the list of comparisons, two combinations are missing that would fill in the matrix: visible reflectance and tau; ECF and cloud fraction. Both of these seem like they would exhibit strong correlations, so maybe that is why they are omitted. But in other parts of the text there is a contrast made between the MODIS and AIRS cloud fractions, so seeing ECF versus cloud fraction would be useful. That accounts for an assessment of the "phase space" variables, but MBL depth is also being discussed here, but it is not clear why. Is the MBL standing in here for an integrated "thermodynamic" variable?

We agree that the starting point for dimensionality choice is pretty arbitrary. We draw upon a response to reviewer #2 to partially address this concern:

"The most honest way to go into this investigation is to ask what do we do when confronted with a choice from an enormous selection of available data? Dozens of geophysical variables are available from each instrument and reanalysis system. These variables can be plotted against each other in 1000s of combinations (or much more). The moments of these variables are also another dimensional choice, so to speak. On top of that, any field can be overlaid onto these two dimensions as done in the figures. So where does one start? As we pointed out, our reasoning for starting where we did is found here:

Line 3, page 8: "Motivated in large part to link cloud and thermodynamic properties derived from infrared and visible bands…"

For the revision, it is a great idea to add the two additional panels for visible reflectance and tau, and ECF and cloud fraction. We will do this.

Lastly, we chose MBL depth as a representative variable of the MBL just to make the point of why we chose the dimensions that we did. We have made these plots with RH, dMSE, etc., and the story is very similar. Basically, the overlying quantity in the joint pdf has a larger dynamic range with the dimensions when choosing ECF and reflectance. The values are distributed more widely across the dimensions and look more structured. The optical thickness retrievals from MODIS are only obtained for a subset of all MBL clouds since the retrievals fail within cumulus that are subpixel in size. This is why we made the argument in the paper that there is a population of clouds at the subpixel scale picked up in the AIRS reflectance data that is completely missed by the MODIS cloud mask and cloud optical property retrievals, driving our choice for reflectance in the end. We will tease this out more fully in the revision.

4. The comparison of the regions. Early in the paper (Sections 1-3), it makes sense to

look at the four regions separately. Going through Section 4, my feeling is that mostly the NEP, SEP, and SEA act very similarly, while NEA is an outlier. This is likely due to the NEA being more strongly influenced by midlatitude systems (even when filtered for mid and high clouds). A few points are raised about the difference between the hemispheres, but it isn't clear whether there is enough sampling (especially with only one season) to make any definitive statements. So I wondered, especially at the end of Section 4.3, whether it would simplify things to combine NEP, SEP, and SEA into one population and exclude NEA or show it as a contrasting population? The advantage is to reduce figure panels and increase overall sample size at the expense of having a comparison of the regions. In the present form, I don't see that the bottom line of the paper is really emphasizing any differences in the regions except that NEA is different from the others. As a related note, the title of Section 4.3 is "Regional differences in MBL depth and dMSE," but my main takeaway from Figure 8 is the similarity of the regions, and I felt like dMSE was not much emphasized in the section.

Thanks for the suggestion of combining the three regions. The sample sizes are quite large for portions of the joint pdfs that are gray-ish and not as large for other portions of the pdfs that are black-ish (see figure 8, upper row). Some differences still show themselves throughout the paper even in portions of the pdfs where the sample sizes are larger, so we would be concerned that those differences would be averaged out into a composite pdf that more poorly resembles each of the three regions individually. Another significant concern is that the distribution of samples throughout the joint pdf can be different in the SEP, NEP, and SEA that would further smear out the subtle differences if all were summed together into a single pdf.

We appreciate that the subtle differences among the three regions may arise because of insufficient sample size, or because there is year-to-year variability and the differences can be easily flipped around to another year. While this may be true to some degree, there are some subtle differences that we believe are actual differences between the regimes in portions of the pdfs, specifically in relation to MBL depth, dMSE, omega, reff, and u925. We will emphasize these more subtle behaviors in the revised text. We may consider highlighting certain portions of the pdfs in the subpanels with boxes or labels or lettering to point these features out.

In the revision, we will also make clear that the overall similarity is the most apparent feature. Lastly, we will think about an improved title for the subsection.

We will move the standard deviation panels to the Appendix for figures 8 and 11 per the previous response above.

5. Comparing different scales. This study focuses on the smallest scales possible for the data, which is interesting by itself. There should be some care taken when comparing to previous studies that are explicitly working at much larger scales. This comes up in a few places in the text, but prominently at the end of section 4.3 where there is a conclusion that dMSE is correlated with small-scale spatial structure *rather than* large-scale

thermodynamic structure. This might be misleading. When averaged up to longer time scales, it seems reasonable that dMSE is more representative of the large scale thermodynamic structure than the spatial structure of clouds. The same holds for LTS and EIS; the relationships between these bulk measures of inversion strength and cloud cover are only valid on relatively long time scales. Recall that the Klein-Hartmann line is derived using seasonal averages. This is discussed occasionally in the literature; one example is found in Zhang et al. (2009, DOI:10.1175/2009JCLI2891.1) where they point out that sampling the low-level divergence distribution is important for capturing the relationship between LTS and cloud cover.

Thanks for pointing out the scale context of the agreement. In the revision, we will be clearer about this and will revisit our references and text with regard to extrapolating between small land large scales, and instantaneous versus seasonal time periods. We will make the point that there is added value in quantifying instantaneous matchups with dMSE and cloud structure, but that does not negate its correspondence with the large scale thermodynamic state.

6. Value of Section 4.4? The text seems to suggest that the point of this section is to compare AIRS and MERRA RH, showing they are similar and therefore useful. The MERRA RH isn't shown here (added as Figure A1), which undercuts this as the main message of the section. The section title is just "vertical structure of RH," but it is pretty hard to get a good sense for the vertical structure from the conditionally averaged contour plots showing one level at a time. The question is what aspect of the RH structure is needed to advance the overall argument of the paper? Based on Section 5, it is not clear that the vertical structure of RH is integral to the paper and Section 4.4 and Figure 10 could be deleted.

We agree with the reviewer that this section is somewhat tangential and in the revision we will move Fig. 10 to the Appendix. We will absorb some of the text into the Appendix as well but will try and delete a good portion of it. We would like to keep the MERRA and AIRS RH figures in the Appendix because it shows very clearly the moistening and drying with respect to reflectance and ECF, and that RH does not simply depend on altitude. These points will be made clear in the revised text.

7. The connection to microphysical effects. Section 4.5 brings r_e into the picture, and suggests that the difference between the stratocumulus and cumulus is due to microphysical processes. The next section makes the connection to wind speed, which is interesting. I'm not sure I understand the physical interpretation of the result. Also, it seems like making the link via the comparison of the contour plots in Figures 11 e-h and 13 e-h is a little cumbersome. Does viewing this relationship within the reflectance cloud fraction phase space make the most sense here, and if so, what do we get from this view that would not appear by directly correlating r_e and u925, for example? This seems like a key finding in the paper, and it might be better drawn out by combining sections 4.5 and 4.6 into a more unified discussion of the r_e variation and connection to meteorology and microphysical processes.

The physical connection between wind speed and effective radius is found in the first paragraph of Section 4.6 on page 11 and is motivated by the work of Nuijens et al. (2009) and follow-on studies based on bulk theory afterwards.

Given that we will remove the standard deviations in figure 11 in the revision and place them into the Appendix, we can move the wind speed panels from figure 13 to figure 11 as the lower row for easier comparison.

Given that we have selected the reflectance dimension as the most appropriate for comparison, we will leave that dimension as is for the revision. However, we will add correlations between u925 and effective radius for the revision as a new figure. Part of the reason we wanted reflectance for effective radius is that it is easier to show some of the 3-D radiative transfer issues that arise in the highly skewed portions of the joint pdfs that are discussed in section 4.4.

Given these comments above and elsewhere, we will combine section 4.5 and 4.6 into a new unified section that united effective radius, u925, omega, theta and thetae.