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Interactive comment on "A satellite and reanalysis view of cloud organization, thermodynamic, and dynamic variability within the subtropical marine boundary layer" *by* Brian H. Kahn et al.

Anonymous Referee #1

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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. 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.

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?

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?

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?

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.

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.

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.

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.

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