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Interactive comment on "Atmospheric parameters in a subtropical cloud regime transition derived by AIRS+MODIS – observed statistical variability compared to ERA-Interim" *by* M. M. Schreier et al.

Anonymous Referee #2

This paper presents some nice results about the distributions of cloud parameters, temperature, and humidity split into cloud classifications in the NE Pacific summer (July). The use of AIRS and MODIS is terrific, and the comparison to ERA-Interim is useful. The paper is written well and is understandable, the methods generally make sense, and the figures are fairly clear. There is one major issue with the current version of the paper: there is no science question that is being addressed. That isn't to say that the results are not interesting, or even that the paper needs to answer a specific question. The point is that as a reader, it is difficult to understand what this paper represents other than a list of results from this interesting methodology stuck up next

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to ERA-Interim as a comparison to a model. So it comes down to framing the paper to lead the reader along some path of reasoning. The introduction should more clearly define why the results to be presented are useful. Currently it is a bit unclear about what is going on in the paper. The second paragraph ("Joint probability ... parameterization approach") is an example of how the text currently is beating around the bush about the issues; it is about parameterizations based on joint PDFs, but never in the paper do we actually see a joint PDF, and there's no discussion about whether the ERA-Interim cloud scheme uses a joint PDF, so what do joint PDF schemes have to do with what is actually presented? Without a more explicit statement of the utility of the results, the significance of the paper seems quite limited. Given that the results focus on the discrepancy between ERA-Interim and the satellite results, it seems like the paper could be re-focused on evaluating the NE Pacific clouds in ERA-Interim; maybe there should be a figure showing ERA-Interim biases to motivate the breakdown into cloud types and higher-order statistics, as a start.

There are a few minor issues that should be addressed as well. There are some detailed notes below, but here are the main issues from my reading. First, the classification scheme does not seem very useful. It is based on cloud fraction, but then also on latitude. The latitude dependence seems ad hoc and overly restrictive. This should be addressed, including a discussion of whether there is a difference in results if the geographic restrictions are removed. Second, it is not clear why daily distributions are constructed and then averaged, versus constructing the full distribution over all Julys. Third, the assumption of unimodal distributions should be justified (either with some analysis or by referencing some of the literature). Finally, regarding the comparison to ERA-Interim, it is not clear that ERA-Interim should be expected to capture the statistics that are being compared, either because of model physics or because of model resolution. It would be useful to have a more complete discussion of reasons for disagreement between the satellite and model data.

Detailed Comments ("page number"/approx line number):

24053/Line 13: It does not seem correct to call the Sc-to-Cu transition a "cloud regime" without decribing the key characteristics of it. My feeling is that it is a mixture of the Sc and Cu regimes, rather than a distinct regime.

24053/Line 24: Is this paragraph necessary? It seems like it does not add any useful information; all readers will be familiar with the limitations or radiosondes and field campaigns.

24056/Line 26: Please clarify the method for producing cloud-top paramters from ERA-Interim. Is a "retrieval" done using just state variables, or ar the ERA-Interim cloud fields directly used? Are any assumptions made to differentiate cloud-top from the rest of the layer-averaged cloud properties? What assumptions are used for the r_e calculation? This is all to say, please provide enough detail for a reader to be able to reproduce the analysis.

24056/Line 10: It is a little disappointing to see the data winnowed down to just 7 Julys over the NE Pacific. It would have been nice to see the data extending to 2012 to get to highlight 10 years of AIRS and MODIS data. Even better would have been to also include an analysis of the seasonal variation; this is probably beyond the scope of the current paper at this point, but I'm sure that there is an appetite for a re-examination of subtropical stratocumulus (and transition) seasonal behavior using high-quality satellite observations over 10 years.

24058/Line 10: The classification scheme is a little strange to me. In particular, the use of geographic criteria seems to suggest that cloud fraction isn't really doing a good job of differentiating the cloud types. The fact that "trade Cu" and "Sc" can not occur in the same area is troubling. Why not investigate the cases in which, for example, cloud fraction is <30% with no high clouds at latitude 30N? This should be better explained in the text.

24059: In the distribution of the classifications, it would be good to know how many "good" quality profiles are neglected due to not being classified.

24060/Line 6: Please provide justification for averaging the daily distributions rather than constructing the actual distributions over all data. It seems like the "average daily distribution of standard deviation" is far less interesting/useful than the "distribution of standard deviation." Don't we need to see additional statistics by doing the average of the daily distributions, like the standard deviation within each bin over all 217 days? Something like that is shown later for the profiles of theta and q. The text mentions a few lines later that the theta and q statistics are calculated in each vertical layer in order to preserve "height dependent behavior." Perhaps this is also the reasoning for averaging daily distributions, but that seems to assume that we are interested in covariance between layers being preserved after averaging over 217 distributions. Some discussion on this topic would be greatly appreciated.

24060: "The interpretations assume a single mode that neglects bimodality, but for the MBL clouds of interest, this assumption is arguably justified." Please make the argument that justifies the assumption, as it is far from obvious that transitional clouds won't be intrinsically bimodal because they are a mixture of stratus-like and cumulus-like clouds.

24063/Line:4-13: The characterization of ERA-Interim r_e calculation isn't sufficient to understand the differences shown in Fig 3. From the technical description of Cy25r1: "The effective radius of the liquid water cloud particles is computed from the cloud liquid water content using the diagnostic formulation of Martin et al. (1994) and specified concentrations of cloud concentration nuclei over land and ocean. For ice clouds, the effective dimension of the cloud particles is diagnosed from temperature using a re- vision of the formulation by Ou and Liou (1995)." (http://www.ecmwf.int/research/ifsdocs/CY25r1/pdf_files/Physics.pdf) Dee et al. note in Table 2 that this is the version of the IFS that incorporated "interactive radius of cloud droplets". The discrepancy between the satellite data and ERA-Interim is dramatic. Is it possible that the model's cloud water does not include falling hydrometeors, which could broaden the distribution toward larger effective radius? Understanding how the

result of Fig 3 comes about would be a useful contribution, and a good lesson for other satellite/model comparisons.

24065-24066: The variability in theta around the MBL and tropopause is unsurprising, since these are where strong T gradients exist. The text indicates that AIRS finds structure in the higher moments while ERA-Interim doesn't. What isn't clear is whether that could have been guessed beforehand or not, and also what it might mean. Is it surprising, for example, to see more structure in observations than in a model?

24067: "the mean profile of q is lower for Sc and trans Cu than for trade Cu and clear sky" This is not clear. It should say something more like, "The mean q profiles show that Sc and trans Cu conditions are drier than clear or trade conditions, and are less well-mixed in the lowest levels."

Fig 2: 1. Why are Figs 2c and d shown as "% of max" rather than just % ? Doesn't this view give too much weight to cloud types that are relatively rare? 2. There's no need to show the high and mid-level types in Fig 2c and 2d because they are defined as only having >90% cloud fraction. Unless the difference between AIRS/MODIS and ERA-Interim shows something noteworthy.

Figs 2,3,4,5,6: I recommend labeling the columns as AIRS/MODIS and ERA-Interim in each of the figures. Especially in Figs 5 and 6, this could reduce text clutter by labeling the columns (maybe at the top, using big bold text).

Figs 5 and 6: These figures are difficult to look at because there are so many lines and dots. 1. Why the 200K range in top panels? It's impossible to see the different lines and dots. 2. Since the focus of the paper is on boundary layer clouds, why not restrict all the panels' vertical axes to 1000-850, as in the inset in the top panels? 3. The structures are not quite coherent enough to show both the mean and variability of all the classifications on each panel. There are lots of ways to address this, but one simple one would be to get rid of the dots for classifications that are less important, like high and mid and maybe clear. Focusing on just the lower troposphere might also help

to expand the horizontal axes, so the lines could be more easily seen.

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