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Atmospheric Chemistry and Physics Discussions

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.

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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.

The authors thank the reviewer for the positive feedback and very helpful suggestions. We hope that we have satisfactorily revised the manuscript.

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 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.

The reviewer is entirely correct and we thank him/her for raising this point. We focused on the "joint PDF approach" in the introduction because it is our long-term goal to treat the remote sensing data as such. The main purpose of this study is to evaluate the ERA-Interim data using the Aqua satellite data as an observational benchmark, and determine whether these data contain more information on the variability of the MBL compared to ERA-Interim. Thus, we have rewritten portions of the introduction with this reviewer suggestion in mind to treat this more as a "stepping stone" to further research, and evaluating ERA-Interim is the first step. We rephrased the last paragraph on page 4 (line 24-32) accordingly to point out the limitations and expectations of this study for satellite data and reanalysis with respect to pdfs:

"The coarse horizontal resolution of AIRS/AMSU is a fundamental weakness. The remote sensing data (∼45 km) is only a factor of ∼2 higher than ERA-Interim resolution (∼80 km). While it is impossible to resolve very small-scale features that will be effectively smeared over the 45-km field of regard, the reanalysis is also subject to the assumptions imposed by subgrid-scale parameterizations, for which the satellite data is not subject to. Thus, there is value in comparing the two data sets. In this work, we ask if the remote sensing observations of θ, q, and cloud property PDFs are comparable to current state of-the-art reanalysis data, especially in the case of the stratocumulus (Sc) to trade cumulus (trade Cu) transition."

We have also further motivated this work (in lieu of an additional figure) by addressing some aspects of the biases in ERA-Interim. The following text and reference was added to p. 3, lines 22-27:

"There are systematic biases in the ERA-Interim low-latitude cloud regimes that have been previously discussed (Dee et al., 2011). In particular, the version of the Integrated Forecast System (IFS) most similar to that used in ERA-Interim (CY31R1) overestimates the population of trade cumulus while underestimating the cloud fraction, and has a high altitude bias relative to the Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO) lidar (Ahlgrimm and Köhler, 2010)."

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.

We agree that the latitude restriction is overly restrictive. The additional reviewer also makes this same point and we have removed it in the revised manuscript.

The original intention was to put it in place to overcome possible misclassifications (a cloud fraction of 90% in an AIRS FOV at 10 degrees N is unlikely to be a stratocumulus cloud, whereas a cloud fraction of 10% in an AIRS FOV at 35 degrees N is unlikely to be a trade cumulus). The results are somewhat changed with the removal of latitude restriction, which could be thought of as being somewhat "more blurred" compared to the first version with the latitude restriction.

For the AIRS/MODIS observations, we see a larger amount of Sc, trans Cu and trade Cu in Figure 1. ERA-Interim data was only weakly affected. Additionally, it resulted in some variations of the pdfs of cloud parameters, as seen in Figure 3 and Table 1, and a broader distributions of lower tropospheric stability (see Figure 4 and Table 2) especially for Sc. Another result is a weaker distinction between boundary layer and free troposphere in the moments of θ- and q-profiles, as can be seen in Figures 5-8. We changed the descriptions accordingly in Section 3.3 and 3.4.

Second, it is not clear why daily distributions are constructed and then averaged, versus constructing the full distribution over all Julys.

This is an outgrowth of the thinking towards "Joint PDFs and relevance to subgrid parameterizations". The daily snapshot approach allows us to get rid of the time variability and build a robust composite of the spatial statistics by cloud type. However, given the reviewer comment, we felt as though it would be a nice addition to add a comparison of daily averages (spatial variance only) and total averages (spatial and temporal variances). Figures 5-8 now contain two sets of lines each for the two types of averages. (We decided to discard the "standard deviation" lines for all of the moments, which resulted in more confusion than anything.)

We changed the explanation in paragraph 2.4 accordingly and added on page 9, line 12-15: "To take into account the influence of time variability, we show, for each data set and cloud type, two types of profiles in these statistics: first, the average from daily “snapshots” for the 310-day period, and second, the total average of all profiles from all 310 days.”
Additionally, some comparisons of total and day-to-day averages are described in section 3.3 (page 14, line 3-10 and line 16-19, page 15, line 21-23)

Third, the assumption of unimodal distributions should be justified (either with some analysis or by referencing some of the literature).

The paragraph with explanations about the unimodal assumption was extended, including some additional literature and analysis (page 9, line 22-27):
"This is a simplified assumption, as bimodality is possible in tropical water vapor (Zhang et al., 2006) and in non-steady state cases (Sukhutame and Young, 2010), which could have an influence on skewness and kurtosis of q for trade Cu and high clouds. However, the analysis of randomly selected layers of θ and q for the different cloud types did not reveal any bimodal distributions in our dataset and the simplified unimodal assumption is used for all calculations.”

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.

This work is a straightforward comparison of the statistical moments between AIRS/MODIS and ERA-Interim. Although the moments of ERA-Interim are somewhat more Gaussian than AIRS/MODIS, we do not speculate whether this is because of

ERA-Interim‘s coarse resolution or is a result of the model subgrid physics. In order to test this, the ERA-Interim data must be reproduced with an attempt at perturbed physics, variable spatial resolution, and perturbed data assimilation experiments. To our knowledge, that type of study has not been published to date.
For the present work, the AIRS/MODIS satellite data has a spatial resolution comparable in magnitude to ERA-Interim. Thus, if the satellite data contains structures in the higher moments that are absent in ERA-Interim, we argue that ERA-Interim must be missing these variations. These thoughts are added to the revised manuscript in the last paragraph of the introduction (page 4, line 24-32).

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.

We thank the reviewer for pointing this out. We changed this phrase to "a particular area of cloud regime transitions".

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.

This part was deleted from the revised manuscript.

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.

We used the same approximations as in ERA-Interim reanalysis, but only the assumptions applicable to liquid water clouds. The formulas for deriving effective radius and cloud optical thickness from ERA-Interim data are described in (ECMWF, IFS documentation CY31r1, 2007). We added the reference (page 6, line 21-22) and some additional information to this paragraph (page 6, line 22-29), which is describing the approach to derive effective radius and cloud optical thickness.

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.

Per the reviewer's suggestion, we included the additional July months through 2012 to form a decadal-scale record. The processing power and data volume required to calculate seasonal variability is well beyond the scope of the present article and time scale for the paper revision. However, we fully intend on extending this type of analysis to additional stratocumulus regimes as well as other cloud regimes in tropical, midlatitude, and high latitude areas.

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.

Per the previous comments, we have removed the latitudinal restriction, and the implications of this removal are discussed earlier on in the response.

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.

Around 40% of the profiles didn't fall into any of the cloud classifications (i.e., they were mixtures of multiple classifications). Also, the addition of July 2010-2012 and removal of the latitude restriction slightly changed the amount and relationships of the datasets. We updated the paragraph accordingly (page 8, line 17-28):
"The sample size of all profiles in this area exceed 1,800,000 for AIRS/MODIS. Around 55% of them (1,000,000) fell into the defined cloud categories. Cloud properties were only retained when MODIS retrievals were successful within the AIRS footprint, reducing the total sample size to approximately 980,000 data points.[...] In this study, approximately 900,000 profiles with successful retrievals are obtained for the 310-day period. Approximately 27% of the data are found in trade Cu, 26% in trans Cu, 20% in Sc, 18% in high clouds, 5% in mid-level clouds, and 4% in clear sky.

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.

The strength of the NASA Aqua satellite observing swath is the high spatial sampling across 1350 km for AIRS and ∼2300 km for MODIS (although we only match the two when AIRS observations are available). The polar orbiting satellite observations are only available at 0130 and 1330 local time (and for this study, only 1330 LT is useful because of the use of visible/near-infrared MODIS Level 2 products, only available during daytime). These swaths allow one to generate robust spatial statistics and do not suffer from sampling anisotropy unlike the active profilers CloudSat and CALIPSO, which only sample along-track. Coupling this capability with our long-term interest in exploring how satellite data can be used to address the subgrid-scale parameterization problem in climate models, we naturally were curious about evaluating the spatial variability and higher statistical moments of climate models and reanalyses. The Aqua data is not sufficient to resolve the daily time variability (e.g., diurnal cycle), thus the daily averaging (and compositing) was done to avoid influence of time variability.
However, we have made a few changes to the new Figs. 5-8. We removed the "standard deviations" for every moment and cloud type as it added more confusion than clarity. Furthermore, we added a new average over the full 310 July days that includes the time variability that arises from variability from day-to-day. These changes were not applied to the cloud parameters in the first few figures and tables.

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.

We agree that transitional clouds could have bimodal distributions. However, visual inspection of temperature and water vapor profiles of transitional cloud profiles did not reveal or indicate any bimodal variation in our data. We assume therefore a unimodal variation and explain this on page 9, line 22-27: " the analysis of randomly selected layers of θ and q for the different cloud types did not reveal any bimodal distributions in our dataset and the simplified unimodal assumption is used for all calculations."

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.**

We considered liquid clouds only and did not include the effects of precipitating hydrometeors. We used these simplifications because we are mostly interested in the subtropical low-level clouds, which are primarily liquid and mostly non-precipitating and we do not have enough information from reanalysis to include the effect of either the precipitation or ice clouds in a consistent manner with the parameterizations. We agree that these approximations could potentially lead to a bias of the ERA-Interim cloud properties for the mid-level and high clouds. But we expect that the shallow low-level Sc, Cu and transition Cu clouds are well characterized. Therefore we do not expect that the key differences between the observations and ERA Interim are due to the above-mentioned approximations. These differences are probably the result of the very simple parameterization in ERA-Interim, but could also be a result of overestimation of cloud droplet radii in MODIS retrievals since the instrument is sensitive to some precipitating hydrometeors (see King et al., 2013, ACP). We revised the text in the revised manuscript, please see lines 31-32 on page 11.

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?

The instrument specifications and retrieval algorithm of AIRS are not optimized to retrieve MBL information, and are also sensitive to the presence of clouds (e.g., reduced sampling and sensitivity with increased cloud fraction). Analysis of the AIRS averaging kernels indicates sensitivity in the MBL, but how much relative to truth is still a topic under investigation. From this point of view, the fact that AIRS picks up on some variability in the higher moments while ERA-Interim does not is very interesting (and perhaps a little surprising to answer the reviewer's question).
We made the following addition in the manuscript to address this point (page 14, line 19-24): "AIRS cannot accurately resolve the fine vertical detail of the MBL (Maddy and Barnet, 2008). However, these results demonstrate a systematic change in the distributional characteristics of θ between the MBL and free troposphere in both clear and cloudy sky compared to ERA-Interim. Overall, the observations by AIRS show structured patterns in the profile of the higher moments, whereas the variability is more

random in the ERA-Interim data."

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."

We changed the sentence according to the reviewer's suggestion.

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?

Showing the distribution by using "% of max" is for better visibility only, as there are different sampling rates for the different cloud types. Removing the normalization made the plot much less clear.

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.

Per the reviewer's suggestion, we deleted the mid-level and high clouds from this plot.

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.

We thank the reviewer for the suggestions regarding the plots. We changed and divided up Figures 5 and 6 into plots according to these suggestions. We divided the Figure for potential temperature into two Figures: Figure 5 shows the mean profiles and standard deviation profiles, whereas Figure 6 shows the skewness and the kurtosis. We have panels of 100 to 1000 hPa and separate panels that zoom in to the 1000-800 hPa layer (we went to 800 hPa instead of 850 hPa because we wanted to capture the transition to the free troposphere). In addition to the day-to-day variability, we added the moments of the total amount of profiles, which added some valuable additional information. On the other side we got rid of the dots showing the day-to-day variability, as it resulted in less readability of the plots. We hope the revised figures are in line with the reviewer's ideas.