Comments on "On the transitions in marine boundary layer cloudiness" by I. Sandu, B. Stevens, and R. Pincus. Submitted to Atmospheric Chemistry and Physics.

General Comments

The authors use observations to diagnose characteristics of the transition in marine cloudiness from overcast stratocumulus to scattered cumulus that occurs over the eastern sides of sub-tropical and tropical oceans. A lagrangian analysis of cloud and environmental properties is performed using trajectories calculated with ECMWF interim reanalysis products.

Undoubtedly a lot of work went into making the calculations and this appears to me to be largest set of lagrangian tracking studies performed for marine boundary layer cloudiness. Not unexpectedly, the results reinforce multiple prior studies and thus the paper does not significantly advance our understanding of the stratocumulus-to-trade-cumulus transition. However, it is nice to thoroughly confirm what has been known and to do so in a rigorous way as the authors nicely do.

Specific Comments

As I read the paper, I kept asking myself what was new or interesting about this study. Perhaps the most interesting thing I could think of was that low-level divergence does not decrease until the fourth day and after most of the decrease in cloud fraction has occurred. This suggests that thermodynamic changes in SST and LTS are far more important that decreased subsidence in affecting the transition. This would somewhat contradict the conventional explanation for the transition that usually invokes both reduced subsidence and increased SST and lowered LTS in causing the cloudiness transition. Indeed, this deserves some mention in the abstract. As currently written, the abstract only emphasizes the robustness of the transition and does not mention any other results of the paper. I would suggest that the abstract be expanded to include the other key results of the paper, even if redundant with previous work, such as the dominance in SST and LTS in driving the transition over other factors including divergence, aerosol optical depth, and upper tropospheric humidity, based upon Figure 5.

The authors emphasize the similarity between the view of the transition from a lagrangian perspective to that formed by analyzing climatological data along the typical path of a trajectory. To some degree, the authors seem to be surprised by this result, as suggested in the last paragraph before section 6. However, this result is fully expected based upon previous research. In particular, the trade winds are among the most steady of wind circulations on the planet. In the scientific literature this was well documented in Figure 3 of the classic 1951 Quarterly Journal article of Riehl (The north-east trade of the Pacific Ocean, Quarterly Journal of the Royal Meteorological Society, Volume 77, Issue 334, Date: October 1951, Pages: 598-626, H. Riehl, T. C. Yeh, J. S. Malkus, N. E. la Seur).

More importantly, the authors argue that climatological data would be sufficient to evaluate the model-simulated changes in cloud and environmental properties for a transition from stratocumulus to trade-cumulus. Certainly to some extent this is true. However, it also depends on how you use the data; are you going to use the observations to qualitatively or quantitatively assess models? Consider Figure 7 of the present paper. For the Northeast Pacific region, it shows that the climatological value of cloud fraction is ~0.8 at the start of the transition whereas it is always 1 in the lagrangian analysis (nearly by construction). Note that this climatological value could result from a situation in which 2/3 of the time there are overcast clouds and 1/3 of the time there are scattered clouds with a cloud fraction of 40%. Now if I am trying to make a composite of cloud observations to compare to a model simulation of the transition that begins with overcast stratocumulus, I would only want to make the composite of observations that were overcast at the starting point. This would be because some cloud or environmental properties at non-overcast times may not be suitable for quantitative evaluation of models simulating the transition. While the density of observations from platforms such as CloudSat/Calipso is too low to observe the transition in individual trajectories, I suggest that the authors examine these observations in their composite lagrangian perspective first to examine differences with the perspective based upon climatological data along a streamline. I think you may find differences that would be important to consider if you are quantitatively evaluating a model.

Another concern of mine is that composites of a number of key variables were not presented. In particular, liquid water path was hidden away in an appendix. Also, no observations were presented of cloud optical depth (although there is a reference to this variable with the phrase "not shown") or cloud-top height (for example, Zuidema, P., D. Painemal, S. de Szoeke, and C. Fairall, 2009: Stratocumulus Cloud-Top Height Estimates and Their Climatic Implications. J. Climate, 22, 4652–4666). These variables are among the most important variables that can be used to assess a model and these variables often vary the most between models. I am fully aware that are significant observational problems with these variables but the authors could provide a service by presenting composites of these variables, perhaps from multiple sources, and documenting and discussing the spread in plausible estimates.

A related point is about the use of ECMWF data for upper-air humidity and horizontal wind divergence. Both of these quantities are available from satellites, specifically from AIRS for humidity and QuickScat for surface wind divergence (see R. Wood et al. QJ2009). Why not composite the satellite observations for these quantities?

Technical Corrections

Abstract. “This opens new”. I think that the use of the word “opens” exaggerates the significance of the work presented in this paper with respect to the similarity between climatological and lagrangian view points.

Section 2. “The air masses flow” should be “air mass flow”.

Section 2. Why did you omit from your analysis the Australian stratocumulus region? Observations in Klein and Hartmann (1993) show that stratocumulus occur in DJF in this region with a frequency greater than that of the Northeast Atlantic in JJA that you show in this paper.

Section 2.1. I am puzzled why you perform 3-dimensional trajectories and not 2-dimensional trajectories. Since you want to track the evolution of the cloudy boundary layer, it seems to me more appropriate to calculate trajectories for the mean horizontal wind in the lowest 1-km of the atmosphere.

Section 2.2.1. Following similar considerations, I would think you want to examine the horizontal divergence of the average horizontal wind in the boundary layer and not at a single level. Additionally, it would be worthwhile to examine subsidence rate at 850 hPa from the analysis.

Section 2.1. Why 11 local time? Why not a standard hour like 0, 6, 12, or 18 LT?

Section 2.2.3. Although it may well identify precipitation from medium and deep convection, the GPCP precipitation data is probably not good for shallow boundary layer clouds. You should acknowledge this.

Section 3.3.1. Because the median backward trajectory does not appear to move much, it would be more informative to show a map that identifies the geographical distribution of backward trajectories at a fixed time (say 3 or 6-days prior).

Section 3.3.1. How long are the backward trajectories? Are they also 6-days? I couldn’t find this described in the text and the figures don’t have tick marks indicating different days.

Section 3.1.2. “we adjust the initial time in this (and other)”. In the figures, I only see the adjustment done for the NEA region. What other regions?

Section 3.1.2. “the cloud fraction suffers”. The cloud fraction cannot suffer. It is not human nor an animal.

Section 3.2. “are likely of secondary importance”. Secondary to what? Be specific.

Section 4. “We suspect that the remaining spread simply reflects…” I didn’t understand your answer to this issue since you invoke the influence of anomalously cold SST. However, I would have guessed that this influence would have been removed by the normalization to start at the median LTS value. Or are you saying that there is an influence of SST independent of LTS?

Section 4. Last paragraph. In the classic explanation for the influence of LTS, one usually invokes the reasoning that higher LTS promotes shallower boundary layers that are easier to maintain as overcast and well-mixed. You didn't mention that here.

Section 5. "Which adds another inconvenience." This is not a complete sentence.

Section 5. The commas surrounding "mean field" should be removed.

Section 6. The word "As" before "our analysis emphasizes that in most…" renders this sentence incomplete.

Figures 2 and following. The caption says that the "y-axis labels the values at the initial time, after 3 days and respectively at the end of the trajectories". However, I can see that this is only true for the trajectory from the NEP, and not all trajectories.

Appendix B. "e.g." is redundant with the "such as" in the same sentence.

Appendix B. "the" should be inserted between "from" and "AMSR-E".