RESPONSES TO REVIEWER #1

Z. J. LEBO AND G. F. FEINGOLD

1. Reviewer #1

Major comment #3: The use of various Th in the manuscript is not just used to demonstrate how the analysis changes as a function of the chosen rain rate threshold, but is also used to draw some conclusions listed in Section 4, for example the asymptotic behaviors at sufficient large Spop, which is mainly from results at Th=0.001 mm/day. So this bears the question that which Th is more appropriate. What is more, if all these Th are not equally realistic, it makes little sense to examine how the different Th might influence the results. So it is important to discuss which threshold may be more important. Also, when a conclusion is drawn in Section 4, it needs to make clear which Th is used.

In our earlier revision, each of the relevant figure captions carried the following text:

"Note that the previously reported value of S_{pop} is used for reference and may not directly correspond to the rain rate thresholds used in the current study."

At the reviewer's request, we have gone to great lengths to now only compare the Wang et al. (2012) value of $\lambda = 0.12$ to LES values derived using a threshold of 0.5 mm day^{-1}. Therefore, only figures that use a threshold rain rate of 0.5 mm day^{-1} now include reference lines to the 0.12 value. For the comparisons with Mann et al. (2014), the 0.66 reference line appears in figures that have thresholds of 0.001 mm day^{-1} and 0.5 mm day^{-1} because their data covered the range of approximately 0.002 mm day^{-1} to 0.5 mm day^{-1}.

It is important to show data for a rain rate threshold of 0.001 mm day^{-1} because surface remote sensing data, such as those presented in Mann et al. (2014), can actually approach rain rates that are this low.

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We have added text in numerous places to explain that Wang et al. (2012) pertains to global oceans and that Mann et al. (2014) includes oceanic and continental clouds. We have made it clear that the contribution of cumulus and stratocumulus to these datasets is unknown, and that the two reference measurements should be used purely as guidance. Note that the scales at which the Mann et al. (2014) results are acquired are on the order of 600 m, which is reasonably close to the LES resolution.

Without some observational comparison, the LES results would be incomplete.

1.1. **Specific comments.** 4). Here we are talking about change in lameta, the percent change in LWP from doubling aerosol loading. It is irrelevant whether this can be easily discerned with satellite observations or not.

This comment appears to derive from an earlier response to the reviewers; there is no text in the current manuscript to change.

10). Though LES has no “cloud fraction” for its individual grids, it has cloud fraction if we consider the whole LES domain. That is where cloud fraction comes from in my original comment.

Understood. The current model analysis focuses purely on the LES results; therefore, cloud fraction is considered in an LES sense. Based on many GCSS intercomparison studies, our cloud fractions are reasonable.

11). How POP is calculated in Wang et al. (2012) is clearly described in LEcuyer et al. (2009). It is my understanding that POP is calculated differently in this study from LEcuyer et al. (2009), as I detailed in my original comment. I do not think the “self-consistent” is sufficient here. As the authors compare this study with Wang et al. (2012), it is important to keep the methodology consistent. Even if the authors can not keep them the same, the reason behind this and its implication for the comparison between two studies need to be discussed.

The current study is an LES study and we can only comment on results from the LES. We make it very clear how POP is calculated. Because of the inherent differences between

GCM and LES studies, any attempt to assess the implications of differences between the approaches would be conjecture.

13). I appreciated that Spop-lameta relationship from Wang et al. (2012) is now removed from Figure 1. The first sentence in Section 3.1 about Spop is now irrelevant (and the probably the Spop in the second sentence as well).

The text has been modified.

16). But this asymptotic behavior is now a major conclusion in Section 4, though this asymptotic behavior is mainly found with Th=0.001 mm/day.

A close look will show that the asymptotic behavior is found in *all* cumulus cases, regardless of the chosen rain rate threshold. This behavior is pronounced in the stratocumulus case for $T_h = 0.001$ mm day^{-1} and also in the $S_{\circ,mod}$ analysis for 0.5 mm day^{-1}. This point has been clarified in the text.

17). I understand that the authors can not get Spop for other Ths. But that is the point, so Spop derived from satellite with zero dBZ radar reflectivity can not be used with Th=0.001 or Th=5.0 mm/day. That is just wrong. The same is also applied to comment #19 and #24.

As stated in our response to the major comment, the text has been changed accordingly.

23). The discussion about Wang et al. (2012) is still not satisfactory. I still think the discussion there is confusing and can be even misleading. The authors need to keep in mind that the relationship derived in Wang et al. (2012) is only applicable to all cloud types over global ocean, while the case this study examined is RICO. This difference needs to be reflected in the discussion.

Again, we have explained that the Wang et al. (2012) data come from global oceans and have cautioned that the comparison should be used solely as guidance. We have stressed that we do not actually know the relative contributions of stratocumulus and cumulus to the observed datasets and that our two representative simulations are just that; they do not cover all possibilities.

We refer to two published values of precipitation susceptibility, i.e., Wang et al. (2012) and Mann et al. (2014), because these are the only large datasets available. Moreover, we only compare these values at relevant rain rate thresholds.

26). The non-zero intercept. It is still not clear to me how RICO case shows the positive intercept. Also, what is the physical reason for a lameta likely larger than 0? What is more, the comparison between this study and Wang et al. (2012) here is not appropriate, as I documented in my original major comments #1 that these two studies look at a quite different cloud population.

The non-zero intercept is the well-known "lifetime effect" — more aerosol results in less collision-coalescence and deeper clouds (positive λ). Small aerosol perturbations are not sufficient to change POP; therefore, S_{pop} is $\approx$0.

As explained in the text, λ has been shown to be positive in clouds that readily precipitate. In polluted conditions, there is modeling and some observational evidence of an evaporation entrainment feedback that creates a situation in which $\lambda < 0$ (as discussed in the text).

A comparison to Wang et al. (2012) here is not unreasonable because both the stratocumulus *and* the cumulus clouds exhibit a positive intercept. Presumably, these two cloud types are prominent features in a global ocean satellite analysis. However, again, because we do not know this for a fact, we have been very careful to state the appropriate caveats upfront. Given that the stratocumulus and cumulus analyses of λ vs. S_{pop} have large slopes near the origin, it is important to make note of the positive intercept.

References

Mann, J. A., Chiu, J. C., Hogan, R. J., O'Connor, E. J., L'Ecuyer, T. S., Stein, T. H. M., and Jefferson, A.: Aerosol impacts on drizzle properties in warm clouds from ARM Mobile Facility maritime and continental deployments, J. Geophys. Res., 119, doi:10.1002/2013JD021339, 2014.

Wang, M., Ghan, S., Liu, X., L'Ecuyer, T. S., Zhang, K., Morrison, H., Ovchinnikov, M., Easter, R., Marchand, R., Chand, D., Qian, Y., and Penner, J. E.: Constraining cloud lifetime effects of aerosols using A-Train satellite measurements, Geophys. Res. Let., 39, doi:10.1029/2012GL052204, 2012.