RESPONSES TO REVIEWER #1

Z. J. LEBO AND G. F. FEINGOLD

1. Reviewer #1

In this manuscript, the authors attempted to use a suite of large eddy simulations of two cloud cases with 4 or 5 different aerosol concentrations (one is stratocumulus DYCOMS RF02 and the other is a trade-wind cumulus RICO) as well as a review of exiting literatures to examine the generality of a climate model-based relationships between the relative LWP responses to relative changes in aerosol number (lameta) and the precipitation frequency susceptibility (Spop) proposed in Wang et al. (2012). As the lameta-Spop relationship represents a potential major advancement in constraining liquid water response to aerosols in climate models and this relationship has not been examined yet in LES models, this study fills this gap and helps to further quantify this Spop metric and the lameta-Spop relationship, and could be interesting to the community. However, I am concerned with their generalizations of their results based on two cloud cases. The paper could also benefit from more appreciation of GCM-based studies. Here I have several comments for the authors to consider.

We greatly appreciate your time and effort in reviewing our manuscript. Thank you for the detailed comments and suggestions. Please find our responses below.

1.1. **Major comments.**

(1) The differences between this study and Wang et al. (2012). Cloud population examined in two studies are very different. The lameta-Spop in Wang et al. (2012) was derived based on data over the global ocean grids in three global climate models. One point in Figure 3a of Wang et al. (2012) represents one model configuration. lameta is derived from a pair of simulations (pre-industrial

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and present day) over the global ocean, while Spop is derived from the present day simulation over the global ocean. So this relationship is based on all large-scale clouds treated in climate models examined. The goal of Wang et al. (2012) is to constrain changes in LWP in response to anthropogenic aerosol perturbations on GLOBAL SCALE (over oceans), but not to derive a lameta-Spop relationship for a particular cloud type or over a particular location. The latter is NOT the intention of Wang et al. (2012), and nor will it serve the purpose of Wang et al. (2012). There is no any mention in Wang et al. (2012) that the derived lameta-Spop relationship can be universally applied to a specific cloud type or location. In contrast, the current paper is a case study on a large eddy scale, based on two cloud cases with 4 or 5 different initial aerosol concentrations. Each point on Figure 4 and 6 is from a pair of simulations (both lameta and Spop/So are from a pair of simulations). Therefore the scale examined in the current study is very limited. Even though it is interesting to see these different relationships for different cloud types (I also agreed that the lameta-Spop is not unique for different cloud types), I do not think the authors can use the relationships derived in the current study to make general comments regarding the lameta-Spop relationship derived in Wang et al. (2012) over the global ocean, unless the authors run a global LES study and perform similar analysis as in Wang et al. (2012). To simply put it, this study and Wang et al. (2012) look at quite different cloud populations, so the lamta-Spop relationship are expected to be different in two studies. LES-type of case studies can be interesting, though it is sometimes difficult to tell how relevant they are to global climate models, due to very limited sample sizes.

Thank you for the opportunity to clarify this point. The reviewer is quite right that the two studies address somewhat different aspects of the problem and has summarized the differences quite well. In fact, if we were to really try to compare the GCM study to our results we would need to either run global LES (as noted by the reviewer) or we would need to aggregate local LES results for different cloud regimes for the global oceans. We could also make more direct comparisons if the GCM $\lambda - S_{pop}$ relationships were broken down in the GCM for different oceanic cloud types. We have made numerous changes to the text to make sure that it is understood that the goal of the present work is to analyze the $\lambda - S_{pop}$ relationship on smaller scales. It was not our intention that the small-scale regime-dependent findings of the present study would be applicable across all scales. The relationships found both in the present work and Wang et al. (2012) are most definitely not universally applicable. In fact, that is a primary conclusion of the present work given the potentially large differences between the results of the current work and Wang et al. (2012). We have altered the wording in the revised manuscript to reflect this point and to ensure that both our results and those of Wang et al. (2012) are not construed as being universally applicable. The concluding remarks emphasize this point.

As a general rule, when addressing aerosol-cloud-precipitation interactions, it is important to note that clouds are inherently controlled by small-scale processes. Therefore, to determine the $\lambda - S_{POP}$ relationship, one must do so by resolving the relevant clouds (in this case, those that occur primarily over the global oceans) and then aggregate such results up to the global scale. The results presented in the current study shed light on the fact that the small-scale processes result in relationships that differ from those predicted by coarse-grained global climate models. Averaging the results presented in the current study for the different cloud regimes is not a sufficient technique for determining a globally representative $\lambda - S_{pop}$ relationship.

(2) The distinction between Spop and So. The current paper seems to suggest that Spop and So is exchangeable in terms of their ability of constraining LWP response to aerosol perturbation. This is particularly evident in their analysis of extant literature (Figure 1 and Section 3.1), as Figure 1 includes So but not Spop from literatures they surveyed. lameta-So relationship is then compared with the lemata-Spop relationship from Wang et al. (2012) in Figure 1. However, as discussed in Wang et al. (2012) (page 4, paragraph 14; Figure S4 and appendix),

So is strongly influenced by accretion process, and the MMF results show that So strongly depends on many nonmicrophysical factors, and is not able to constrain the dependence of autoconversion rate on cloud droplet number concentration. Upon further examining Figure 4 and 6 in the current study, I believe the authors' results also suggest that Spop works better. If we focus on lameta vs. Spop and lameta vs. So relationships for the cases Th=0.5 mm/day (see my next comment about rain threshold and why Th=0.5 mm/day is a more reasonable threshold), we can clearly see that lameta varies near linearly with Spop, while it is not the case for So for DYCOMS II RF02. This is also where I see the current study can make a real contribution: to compare Spop and So metric, and to see which one may be a better metric. Given the differences in Spop and So discussed in Wang et al. (2012), and the difference in lameta-Spop and lameta-So relationship we see here, I do not think it is fair to compare lameta-So relationship from literatures with the lameta-Spop relationship from Wang et al. (2012) in Figure 1 and then make a general comment regarding lameta-Spop relationship derived from Wang et al. (2012) (in the abstract).

One of the goals of the current study is to explore the robustness of the various $\lambda - S$ relationships, which is why we presented both S_{pop} and $S_{\circ}$ metrics. We would argue that the current manuscript does not suggest that S_{pop} and $S_{\circ}$ are interchangeable. The results do suggest that the relationships with λ are similar for very specific rain rate thresholds. This finding is expected because R and POP exhibit different responses to changes in N_a. For example, a small increase in N_a reduces R but may not be sufficiently large to inhibit rain; hence, POP remains unchanged (i.e., $S_{\circ}$ is greater than 0 and S_{pop} is 0).

The main point of Fig. 1 is to demonstrate that the literature suggests a variety of possible relationships between λ and S_{pop} (or proxies) and that aggregation might even change the slope (see the results presented in Fig. 1 of Wang and Feingold, 2009). Nevertheless, we have removed the curve from Wang et al. (2012) in this figure in the revised paper.

Regarding the rain threshold T_h, the use of various T_h values is purely to demonstrate how the analysis changes as a function of the chosen rain rate threshold. We are not being prescriptive. We simply feel that it is important to get a sense of how this threshold might influence the results.

Lastly, we do not understand the "fairness" comment. The purpose of the current study is to analyze our model output and compare with previously published studies; the only existing study is that based on GCM analysis. We believe that this is a reasonable approach given that the true response is in fact an aggregation of local/small-scale responses.

(3) The threshold rain rate for defining a rain event. In the manuscript, the authors tested lameta-Spop and lameta-So relationships uses three different thresholds, Th=0.001 mm/day, 0.5 mm/day, and 5.0 mm/day. The authors seemed to imply that all three Th thresholds are equally possible. However, I would argue that Th=0.5 mm/day is the most reasonable one to use. Th=0.001 mm/day is too low. Though the minimum detectable CLOUDSAT radar reflectivity is -30 dBZ, that is for cloud water, but not for rain water. The cut-off radar flectivity is about -15 dBZ (around 0.1 mm/day) for drizzle, and about 0 dBZ (around 0.6 mm/day) for rain (L'Ecuyer et al., 2009). In Wang et al. (2012), two threshold rain rates are tested (-15 dBZ and 0 dBZ), and only a small sensitivity was found. Though the minimum detectable radar reflectivity is 17 dBZ (5 mm/day) from TRMM, TRMM is mainly used for studying heavily-raining clouds, but not for clouds with light rain that are the majority of the clouds relevant to study aerosol indirect radiative forcing. So the tests with both Th=0.001 mm/day and Th=5 mm/day are less relevant to the question we are interested here. This distinction is important to make, as results from DYCOMS II RF02 showed that lameta-Spop relationship and lameta-So relationship depends on Th threshold. A good predictability of lameta is only found for So with Th=0.001 mm/day, while Spop shows reasonable predictability of lameta for all three Th values and shows very good predictability of lameta for Th=0.5, arguably the most realistic one.

We reiterate our comment above: The use of various T_h values is purely to demonstrate how the analysis changers as a function of the chosen rain rate threshold. We are not being prescriptive. We simply feel that it is important to get a sense of how this threshold might influence the results.

1.2. **Specific comments.**

(1) Abstract. I agree that lameta-Spop relationship is not unique for different cloud types. But Wang et al. (2012) did not make the argument that this should be unique, and nor is that the goal of Wang et al. (2012). As detailed in the major comment #1, the goal of Wang et al. (2012) is to provide a global constraint on lameta. So that relationship is established for all large-scale clouds treated in climate models over global oceans.

We have removed the "uniqueness" wording. However, to truly provide a global constraint, it is necessary to resolve the relevant clouds and aerosol-cloud-precipitation interactions, and aggregate the findings up to the global scale. This point has been made more clear in the revised manuscript.

(2) Page 13235, line 2: See Penner et al. (2011) for issues using satellite observation to constrain albedo effect.

We are familiar with the PD-PI issue from Penner et al. (2011). However, of course, LES is not a means for addressing this question. Our own work (McComiskey and Feingold, 2012) has shown just how uncertain aerosol-cloud relationships can be when this and other issues, e.g., spatial aggregation, are not considered.

(3) Page 13235, line 24-25: Spop and So are different (see the major comment #2). So it is not appropriate to compare Spop with So in Mann et al. (2014). Spop was also derived in Mann et al. (2014). I would suggest to compare Spop from wang et al. (2012) to Spop in Mann et al. (2014).

Mann et al. (2014) does not provide a specific value of S_{pop}; instead, they looked at S_{pop} as a function of the LWP. However, the authors do provide a value of $S_\circ$. Again, we are not being prescriptive. We are simply using two observed susceptibility values as "anchor points"

(4) Page 13236, line 3: Even though the intercept is small, a lameta of 0.01 is still not that small, as this means 1% change in LWP over global ocean.

If the intercept of the $\lambda - S$ relationship is small and S_{pop} is small (0.12 over the global ocean according to Wang et al., 2012), then λ is also small. A 1% change in the LWP over the global ocean could not be easily discerned with satellite-based microwave radiometers, e.g., AMSR-E or even the MODIS LWP product.

(5) Page 13236, line 9: It is not clear to me why the authors want to emphasize that the intercept is near zero. As long as Spop from satellite observations leads to a small lameta, that is what matters.

First, we are interested in the physical understanding of the relationship. In other words, what is the "physical meaning" of an intercept at the origin. Second, if the intercept is not zero, then even a small susceptibility can translate into larger values of λ (both positive or negative).

(6) Page 13236, lines 13-15: I think the goal of this study is clearly stated here. As this has not been examined in LES before, this study can make a unique contribution to the literature. However, the lameta-Spop relationship examined here for two cloud cases are not the same as lameta-Spop relationship examined in Wang et al. (2012) (see the major comment #1). So it would be a stretch to use the Spop-lameta relationship derived in this study to make general comments on the Spop-lameta relationship derived in Wang et al. (2012).

Please see our responses to the major comments above.

(7) Page 13236, line 14: I do not see how the scale-dependence issue is addressed in this study.

The GCM-based results are based on much coarser resolution simulations, even when it comes to the MMF simulations. Granted, two issues are conflated: coarse resolution and global ocean averaging. Instead of specifically address scale issues in the introduction, we have changed the wording in the revised manuscript to simply state that the intention of the this study is to examine the potential generality of the $\lambda - S_{pop}$ relationship.

(8) Section 2.1: Unfortunately, there are not many studies available that examine Spop and lameta relationship. There are more about So and lameta. However, Spop and So are different (See major comment #2).

We are not aware of studies that have explicitly examined the $\lambda - S_{\circ}$ relationship. There are numerous studied that have looked at $S_{\circ}$ as a function of LWP. However, these two types of studies are not directly comparable.

(9) Page 13238, line 23: decorrelation time. This needs some further elaboration.

We use the decorrelation concept in the correct way. It would take more than 1 min for cloud fields to begin exhibiting substantial differences from one another. However, to capture high rain rates, we must include fields that are correlated in time because these events are much rarer than weakly precipitating events.

(10) Page 13239, line 16-17: LWP in Wang et al. (2012) is the grid mean value (cloud fraction * in-cloud LWP) (see Section 3 in Wang et al., 2012)

There is no "cloud fraction" in the LES. Because a grid box contains either all cloud or no cloud, we do not believe that this point is relevant for the current study.

(11) Page 13240, Spop calculation: It is still not clear how POP and Spop is calculated. Is POP calculated as the precipitation fraction of all grid points over the studied domain or only the precipitation fraction of cloudy grid points over the studied domain? The latter is what was used in Wang et al. (2012). Also, to isolate dynamical influences, POP and Spop were calculated on individual LWP, and then a LWP-weighted Spop was derived. In the current study, Spop is calcucated from a pair of study. This is also different from Wang et al. (2012), where Spop is calculated from the present-day simulation through linear regression of ln(POP) and ln(AI). In calculating lameta and Spop, why is the prognostic aerosol number concentration not used in the calculation?

In the current study, POP is computed as the precipitation fraction of all grid points using the difference between two aerosol scenarios, i.e., a low and high aerosol concentration. Without a clear understanding of the method used in Wang et al. (2012), it is challenging to comment on the differences in the methodologies. Instead, we can comment on the fact that the method used in the current study is self consistent and is a viable approach to analyzing susceptibilities in an LES-framework.

(12) Page 13243, line 4: If I remember correctly, Man et al. (2014) also calculated Spop.

However, Mann et al. (2014) determined S_{pop} for a variety of cases; they did not provide a single value of S_{pop} in their paper.

(13) Section 3.1: See the major comment #2. I do not think it is fair to compare So lameta in literatures with Spop-lameta in Wang et al. (2012). Suggest to remove this section, as this adds little.

As noted above, the dashed line corresponding to Wang et al. (2012) has been removed from the figure in the revised manuscript.

(14) Section 3.2.1: See the major comment #3 for rain rate thresholds

Please refer to our response above regarding this point.

(15) Page 13246, lines 1-2: The dependence of Spop-lameta on Th. A small sensitivity was found in Wang et al. (2012) when 0.12 mm/day instead of 0.6mm/day is used. I would argue that Th=0.001 mm/day and Th=5 mm/day are less realistic and less relevant to aerosol indirect radiative forcing we are interested here (major comment #3).

We are interested in understanding how the results differ for a range of T_h.

(16) Page 13246, lines 15-16: not sure how useful the discussion of the asymptotic behavior is. Spop-lameta does not show this behavior with Th=0.5 mm/day, which is arguably more realistic threshold.

We feel that this discussion is relevant in the context of the current study because we are not being prescriptive. Instead, we want to examine how the relationships may differ as a function of the rain rate threshold (and other factors, e.g., different cloud regimes). If we hadn't explored different thresholds, we would not have known whether this was an important factor in determining the $\lambda - S_{pop}$ and $\lambda - S_{\circ}$ relationships.

(17) Page 13247, line 21: Spop=0.12 is derived over global ocean with a threshold radar reflectivity of 0 dBZ. So this does not make sense to apply Spop here to different lameta-Spop relationship with different Th.

Without the data, we are unable to redefine S_{pop} for different thresholds. We have noted that the line in the current manuscript corresponds to data thresholded at 0 dBZ. $S_{pop} = 0.12$ is used as a reference point in the current study. Further studies might provide different results in different situations.

(18) Page 13248, line 5: lower detection limits $--$ > higher detection limits?

The wording has been changed in the revised manuscript.

(19) Page 15, lines 14-16: Again, to apply So,mod from Mann et al. (2014) to the different So-lameta relationship with different Th, you need to calculate So with the corresponding Th using data from Mann et al. (2014).

Without the data, we are unable to redefine $S_{\circ}$ for different thresholds. We have noted that the value of $S_{\circ}$ that is used in the text was originally derived for a specific threshold.

(20) Page 13250, line 9: The fact that lameta is not necessarily positive has been found in many previous studies (e.g., Ackerman et al., 2004).

This point is noted earlier in the paper (and in the revised paper). This point was also noted in our own LES for cumulus clouds and in observations of cumulus Small et al. (2009).

(21) Page 13251, line 11-12: the relative droplet number concentration increases. This is not clear to me.

We have included a definition for this variable in the line in the revised manuscript.

(22) Page 13251, lines 21-22: lameta decreases more rapidly with increased aerosol loading. So you mean more rapidly with increased Spop?

We choose to frame this result with respect to the aerosol loading because it is considered to be an independent variable.

(23) Page 13251, lines 25-27: the discussion about Wang et al. (2012). Again, I want to point it out that the lameta-Spop relationship in that study is based all large-scale clouds over global oceans. The focus of Wang et al. (2012) is certainly not just about shall cumulus clouds, like RICO discussed here. So I think the discussions the authors made regarding the lameta-Spop relationship in Wang et al. (2012) based on their RICO results is confusing, and can be even misleading.

Please see our responses to the major points above. We hope the reviewer will find the revised language clearer. We have addressed only two cloud types and only two soundings; therefore, we cannot claim that the results are generally applicable. However, what we do see in our results is that stratocumulus and cumulus exhibit different responses, which is in line with our understanding of aerosol-cloud-dynamical feedbacks from a host of other studies. It is important to note that it is prudent to understand the relationships at the cloud scale and then aggregate the findings up to the global scale to attain an accurate relationship. (see e.g., the concluding remarks.)

(24) Page 13252, lines 1-4: Again, Spop=0.12 and So=0.66 in Wang et al. (2012) and Mann et al. (2014) were derived at a certain rain rate threshold (see specific comments #17 and #19)

Please refer to our responses above.

(25) Page 13252, lines 9-13: The authors made it clear that Wang et al. (2012) examined Spop-lameta relationship on a global scale, while this study examined this relationship at the large eddy scale. This distinction in cloud populations in two studies (including cloud types, sample sizes, spatial coverage) needs to be acknowledged when the authors use their results at the large eddy scale to make general comments regarding Wang et al. (2012).

We have changed the wording at the beginning of the conclusions to better reflect the intention of the current study.

(26) Page 13252, line 24: the non-zero intercept. First, the intercept in Wang et al. (2012) is not zero, but 0.01 (with -15 dDBZ as rain rate threshold, the intercept is 0.02), which is not insignificant and means 1% change in LWP over the global ocean. Second, Wang et al. (2012) is based on all large-sale clouds over global oceans. Third, I do not see why the intercept is likely larger than 0 in the current study. For DYCOMES, it is larger than zero, based on 4 Na perturbation examined in this study (I would expect the minimum lameta of 0.3 will change if we have a large number of simulations with a more gradual changes in Na), but how about RICO? How about if you combine both RICO and DYCOMS II RF02?

The relationship presented in Wang et al. (2012) shows a very small intercept. Without a statistical analysis, one cannot conclude that the value of λ is in fact significant or not. The LES results clearly show larger intercepts. Regarding combination of RICO and DYCOMS-II points, it's clear that we would see a distinct separation of two "populations" of points. One might consider performing a weighted areal average of these points; however two soundings would not be adequate for such an exercise.

(27) Page 13254, line 9: As for the data aggregation, see the discussion in Wang et al. (2012) (their Section 4)

We have noted this point in the conclusions of the revised manuscript.

(28) Page 13254, line 15: lameta-Spop relationship are universally applied. Again, the goal ofWang et al. (2012) is to constrain changes in LWP in response to anthropogenic aerosol perturbations on GLOBAL SCALE (over ocean), but not to provide a uniform Spop-lameta formula for all cloud types.

The concluding remarks have been changed. They make it clear that Wang et al. (2012) and the current study address somewhat different exercises in addressing the $\lambda - S_{pop}$ relationship. We feel that it is essential to understand how these relationships work at the small/cloud scale and aggregate those findings to larger scales. If not, we are destined to venture down the same confusing path that the albedo effect studies did.

Penner, J. E., L. Xu, and M. H. Wang (2011), Satellite methods underestimate indirect climate forcing by aerosols, Proceedings of the National Academy of Sciences of the United States of America, 108(33), 13404-13408.

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RESPONSES TO REVIEWER #2

Z. J. LEBO AND G. F. FEINGOLD

1. Reviewer #2

This manuscript examines the relationship between LWP susceptibility (lambda) and the susceptibility of probability of precipitation to changes in aerosol concentration (S_pop) from a range of LES simulations of DYCOMS-2 stratocumulus and RICO simulations. The motivation is the Wang et al. 2012 paper, which used a range of GCM/MMM simulations to define lambda as a function of S_pop. The current study applies a similar analysis to simulations done at the cloud scale. In so doing a more nuanced understanding of the lambda-S_pop relationship emerges, with a variety of relationships possible depending upon the microphysical regime. The study is interesting and a useful addition to the literature. I have comments I would like to see addressed, after which I would recommend the manuscript for publication as an ACP article.

Thank you for providing useful comments and suggestions. Please find our responses below.

1.1. General Comments.

(1) Abstract: I assume the sentence beginning with A satellite-based measurement: : :.. refers to the Wang et al. 2012 paper. If so it is a surprisingly specific statement to be placing in an abstract. I would suggest removing it. You might also consider adding an additional sentence summarizing your Fig. 9 schematic.

We have removed the specificity from the abstract. Without referring to the schematic itself, we now distinguish between stratocumulus and cumulus responses.

(2) Introduction: The writing could be improved here in several ways. The survey of observational results appears to be cursorily done, with Christensen and Stephens,

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2011 not mentioned until p.13248 line 15, and Terai et al. 2012 described as a S_pop analysis (they examined both rain intensity, or S_o, and rain fraction, or S_pop). Why not a more thorough review of the observed values? Later on it is stated in sec. 2.1 there are so few S_pop observational values that they can be ignored, but I do not see a careful review of the observational literature being done here. Along with this, a better justification of why the Mann et al 2014 S_o value is selected as the observational reference and not others is desired.

We delayed referencing these studies because they address the LWP – S relationship rather than the $\lambda - S$ relationship, which is the focal point of the current study. The value of $S_\circ$ from Mann et al. (2014) is chosen because the authors provide an observed value with large sampling statistics based on their analysis of ground-based remote sensing data. Wang et al. (2012) and $S_\circ = 1.0$ (or equivalently, $S_{\circ,mod} = 0.66$) from Mann et al. (2014) as guidelines.

(3) p. 13235 lines 18-20: it surprises me that cloud type/microphysical regime is not mentioned in this list, since that is the variability that is considered within the manuscript. It might also be worth mentioning that all results are domain-mean in this analysis, whereas some of the observational results may not be. Do the Mann et al. 2014 results incorporate an averaging scale?

This is a good point. We have added the dependence on cloud regime in the revised manuscript.

Moreover, we have added a note regarding the difference in domain versus ocean-only averaging in the discussion regarding the method used to compute λ.

Regarding Mann et al. (2014), the data are averaged for only single-layer warm clouds with bases above 170 m and tops below 3 km. Single-layer warm clouds with high cirrus (cirrus cloud base temperatures below -40°C) are also included in their analysis because these clouds contain no liquid water. The measurements were obtained *in situ* at two ground-based locations, i.e., the ARM mobile facilities in the Azores (June 2009 to December 2010) and the Black Forest, Germany (April 2007 to December 2007). Moreover, Mann et al. (2014) averaged the LWP observational data over 20-min periods such that the spatial scale was approximately 12 km (assuming a nominal wind speed of 10 m s^{-1}).

(4) p. 13236 line 17: S_o is introduced here. It is not apparently part of the Wang motivational analysis. As I understand it S_o is considered because it is better observed (?) and because it is easily done with the LES simulations at hand. Please devote a paragraph discussing how S_o fits into the motivational framework.

We are thinking more generally about how these relationships may differ if the definitions are slightly changed. Our sense is that $S_\circ$ makes more sense in a modeling framework, while S_{pop} makes more sense in an observational framework. However, because we are not prescriptive in the current study, we address both susceptibilities.

(5) Section 2.1 is awkward. Why are observational results not included? The two sentences beginning with The choice of So vs S_pop.... are unintelligible. The information in this section appears to be more motivational and should likely be merged with the introduction.

The text has been changed to clarify these points in the revised text.

(6) Section 2.2, p. 13239, line 12: why is it important that the sounding resemble the 19 January 2005 case?

This sentence has been removed in the revised manuscript because it is not relevant to the current work.

(7) Sec. 2.3-2.7: these sections describe the different calculations and are rather technical. Once I was absorbed with the manuscript the symbolism became familiar, however initially I was often referring back to these sections to remind myself. A table summarizing the different definitions would be helpful for your readers.

A table has been added to the revised text to provide a clear representation of the variables used in the paper.

(8) Sec. 2.7: It is worth mentioning that your A_f definition does not require an actual albedo calculation. Its also worth mentioning the caveat that you are estimating a daytime albedo susceptibility from nocturnal simulations that will not

be including the response of the cloudy boundary layer to shortwave absorption (I would think this would reduce the lambda values).

We have added additional details regarding the exclusion of solar radiation in the simulations. We have also added text to explain that A_f is calculated without knowing the actual albedo.

(9) section 3.1: I find it confusing to have read previously that POP was initially introduced because it was easy to measure, and to read here that it is impossible to determine from previously published results. Is this an observational vs modeling distinction? by previously published results do you only mean modeling results? I also think some of the discussion in the first paragraph belongs in the introduction, possibly the entire section as it is a useful motivator. More physical description of the slopes will help the reader relate to what you show in Fig. 1 (e.g., ...meaning as aerosol concentrations increase, LWPs increase and rain rates decrease).

The point that we intended to make is that POP can be readily measured via satellite observations; however, based on previously published modeling results, it is basically impossible to extract a sufficient amount of information to even make a wild guess at the value of POP and S_{pop} unless the authors specifically provided that information or complete model output. The text has been clarified in the revised manuscript to properly convey that the sentence discusses modeling results and not observations. Furthermore, a few sentences have been added to this section to provide physical descriptions of what the different slopes mean in Fig. 1.

(10) Conclusions: please relate your findings more physically to the results from Wang. How well do you perceive the Wang GCM/MMM simulations captured the two cloud regimes that you examined? Were they also focused on shallow boundary layer clouds entirely?

Wang et al. (2012) examined warm marine clouds (determined by only considering clouds with cloud top temperatures exceeding 273 K). These clouds should primarily be marine stratocumulus, fair weather shallow-cumulus, and trade wind cumulus. However, because we do not know the frequency of occurrence of these different cloud types in the simulations performed in Wang et al. (2012), it is not possible to more physically relate the regime-dependent findings that are presented in the current manuscript to the global ocean-scale findings that are presented in Wang et al. (2012).

1.2. **Figures.**

(1) some of the figures are impossible to read.

We believe this is because the paper was printed from the online format. Once the figures are formatted into the ACP format, this will not be an issue because the figures will appear in a single column.

(2) Fig. 2: I could not read the 3 rain rate thresholds or distinguish their lines. also mention these are DYCOMS-2 in the caption.

We have added this information to the caption. The figures will be easier to read in the final version of the paper (i.e., after being formatted for ACP).

(3) Fig. 3: even more illegible than Fig. 2

Please see the aforementioned responses regarding the figure clarity.

(4) Fig. 4-7: basically illegible. Perhaps try arranging the 3 panels horizontally and playing with the axis labels, removing some and increasing the font size on the outer axes.

We have arranged the panels so that they will appear within a column in the final format.

References

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

Wang, M., et al., 2012: Constraining cloud lifetime effects of aerosols using A-Train satellite measurements. *Geophys. Res. Let.*, **39 (L15709)**, doi:10.1029/2012GL052204.