Atmos. Chem. Phys. Discuss., 14, C6095–C6102, 2014
www.atmos-chem-phys-discuss.net/14/C6095/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the relationship between responses in cloud water and precipitation to changes in aerosol" *by* Z. J. Lebo and G. Feingold

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Received and published: 20 August 2014

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, 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. It's 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 13233, 2014.