Atmos. Chem. Phys. Discuss., 14, C5150–C5153, 2014
www.atmos-chem-phys-discuss.net/14/C5150/2014/

Atmospheric Chemistry and Physics Discussions

Open Access

ACPD

14, C5150–C5153, 2014

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

Anonymous Referee #2

Received and published: 21 July 2014

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.

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.

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.

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?

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.

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.

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

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.

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).

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").

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?

Figures:

some of the figures are impossible to read.

Fig. 2: I could not read the 3 rain rate thresholds or distinguish their lines. also mention

ACPD
14, C5150–C5153, 2014

Interactive Comment

these are DYCOMS-2 in the caption.

Fig. 3: even more illegible than Fig. 2

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.

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

ACPD

14, C5150–C5153, 2014

Interactive Comment