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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

Anonymous Referee #1

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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.

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 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.

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 non-microphysical factors, and is not able to constrain the dependence of autoconverion 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).

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.

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.

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

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).

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.

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.

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).

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

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).

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

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)

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?

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

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.

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

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).

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.

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.

18. Page 13248, line 5: "lower detection limits" → "higher detection limits"?

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).

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).

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

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

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.

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)

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).

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?

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

28. Page 13254, line 15: "lameta-Spop relationship are universally applied". Again, the goal of Wang 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.

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.

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