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# *Interactive comment on* "Precipitation Susceptibility in Marine Stratocumulus and Shallow Cumulus from Airborne Measurements" *by* E. Jung et al.

**Anonymous Referee #1**

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Review of Jung et al. "Precipitation Susceptibility in Marine Stratocumulus and Shallow Cumulus from Airborne Measurements"

General comments

This study examines the precipitation susceptibility metric using consistent measurements from a variety of field campaigns to ask whether the qualitative behavior of the susceptibility varies with cloud type. Whereas previous studies appear to disagree on whether susceptibility should increase and then decrease with cloud thickness or whether they should decrease monotonically, different retrieval methods were used in these previous studies, so it has not been clear whether the differences are due to cloud types, retrieval methods, or analysis methods. This study does not suffer from



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many of the same issues, because the measurements are made from the same aircraft, using the same instruments and sampling strategy.

The authors show that the precipitation susceptibility increases, and then decreases, regardless of whether cumulus or stratocumulus clouds are examined. After presenting their susceptibility estimates, they provide possible explanations for the why the results of Terai et al. (2012) do not capture the increase in susceptibility at lower cloud thicknesses.

The study addresses an existing disagreement in the qualitative behavior amongst precipitation susceptibility estimates and provides valuable observations to add to the existing observed estimates and to try to reconcile the disagreements. However, there some issues that need to be addressed before I recommend publication. In particular, issues of Nd and H covariability and the statistical independence of the 1-second data should be addressed.

Major comments 1) In the study, it appears that a good amount of consecutive data are included in the log(Nd) vs. log(R) regression slopes. Given that N and R estimates from every second are used, there is the possibility that covariability between N and H at those smaller spatial scales might affect S0 estimates. For example, even within the same cloud thickness bin, the N and H can covary in a flight leg due to updraft/downdraft organization. In other words, where there are stronger updrafts, Nd will likely be higher, as well as H. This can impact S0, because H also controls R. Therefore, I would like the authors to examine the extent to which the covariability between N and H exist and how they might affect S0. Do data need to be averaged over longer timesteps to reduce the covariability?

2) Similarly, I would like to see the authors demonstrate whether 1-second of data (N, R) is statistically independent from one another. For example, Leith et al. (1973) provide a method to determine the e-folding time scale, which will help determine whether using the 1-second data is indeed appropriate.





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Leith, C. E. (1973), The Standard Error of Time-Average Estimates of Climatic Means, J. Appl. Meteorl., 12, 1066–1069.

3) Whenever a slope is calculated, the statistical uncertainties should also be reported, since the relationship does not appear to be linear in many of the cases (Fig. 2).

4) Possible explanations are presented as to why the results in this study disagree from what is presented in Terai et al. (2012) but are not tested. I believe some of the issues can be tested using the data analyzed this study. For example, the authors can test whether the method used in Terai et al. (2012) gives a different behavior than when a linear regression is used.

5) Many times, in comparing with the results of Terai et al. (2012), their SR is compared with the S0 in this study. Is this the right comparison to make? Or should SI be compared with S0 in this study, since SI captures the effect of aerosols on measureable precipitation rates.

Minor and specific comments

P1 L23: "R" and other variables (e.g. Nd) should be italicized throughout the manuscript

P2 L26-27: "S0 is insensitive to aerosol perturbations where clouds do not precipitate": S0 should be undefined where clouds do not precipitate, not zero.

P2 L30: Please write out what VOCALS stands for.

P3 L6: (and subsequent uses) Replace "GCCM" with "GCM"? If it is supposed to be GCCM, please state what it is an acronym for.

P3 L12: Please define the acronym ARM.

P3 L16: For completeness, at some point in the paragraph the study of Hill et al. 2015 should be mentioned. There are a number of other instances throughout the study where comparison with results of Hill et al. (2015) would also be good to make.

Hill, A. A., B. J. Shipway, and I. A. Boutle (2015), How sensitive are aerosol-precipitation interactions to the warm rain representation?, J. Adv. Model. Earth Syst., 7, 987–1004, doi:10.1002/2014MS000422.

P4 L12: “LCL varied little for Cu” Can the authors attach some numbers to this statement?

P4 L18-26: How long were these cloud base level flights? In other words, over what length scales are cloud thicknesses assumed to be constant, and is this a good approximation?

P4 L28-29: Is using 1 second data appropriate? Given the sampling volume rate and scarcity of drizzle drops I wonder how statistically robust the R retrievals are. Just based on counting statistics, what are the measurement uncertainties in R? What is the theoretical minimum threshold on R given the sampling rate of 1 second?

P5 L3-6: Same question can be applied to z.

P6 L6: What are the 95 or 99% confidence intervals on this estimate? The scatter in Fig. 2 appears rather large.

P7 L2: A measure of the uncertainty will be helpful here as well.

P7 L8: How were the H intervals chosen? Do the results vary with larger or smaller H bins?

P7 L14-15: “We noted that S0 tended …. (not shown).” Why do the authors think that when the Nd variations are small, S0 is high? Are N and h covariations leading to unrealistically high S0? Do uncertainties in the slopes increase in this case?

P7 L16: “if dlog(Nd) spans at least 2.2” Is the natural log used here? If so, what is the threshold of max(Nd)/min(Nd) used here? Perhaps around 10?

P7 L16-17: “exceeds six for a given H” – this seems like a very small sample size for calculating slopes. Uncertainties in the slopes should be shown.

P7 L20: “statistically significant at the 99% confidence level” - I suspect this means statistically significant with a comparison with a slope of 0. This is only the case if the each 1 second of data is independent of another. The authors need to demonstrate that this is the case, perhaps using the method of Leith (1973) or Bretherton (1999).

Leith, C. E. (1973), The Standard Error of Time-Average Estimates of Climatic Means, J. Appl. Meteorl., 12, 1066–1069.

Bretherton, C. S. et al. (1999), The Effective Number of Spatial Degrees of Freedom of a Time-Varying Field, J. Clim., 12,7, 1990-2009.

P 7 L29-30: “S0 tends to be overestimated…” Based on what has been shown so far, it doesn’t appear that S0 in necessarily overestimated when a larger ‘averaging lengthscale’ is used. It can be that S0 is underestimated when every second of data are included.

P7 L32: “H is estimated from the vertical structure of LWC for each day” - If daily mean H is used, then the sub-scale covariance between N and H should be examined, based on the other measurements. To what extent are H and N covarying and how can that potentially affect susceptibility estimates?

P8 L1: “H of 9 Nov. (164+/- 18m)” To what extent is using daily mean H to group data appropriate? What is the true range of H from each day of flight? Are there cases where data from one day could potentially lie in a different bin?

P8 L3: “as the each” – remove “the”

P8 L9: “no data were available between 800 m and 1500 m that satisfied the data analysis” – were there not enough data points that existed in this range to calculate the regressions or was the range of N too small? Could bins have been combined to get an estimate? In Fig. 5, it is difficult to make out much of a trend based on three susceptibility estimates.

P9 L17-19: “The negative values of S0 in the largest… in that category” – based on the open circle designation (Fig. 4b), it appears that the susceptibility is statistically indistinguishable from zero, so there is no need to explain why it has a negative value.

P9 L20-22: "… if the H varied substantially during the cloud-base level-leg flight on the day which S0 was calculated with a daily mean H." I wonder how the susceptibility estimates from the thinner clouds are similarly affected from the VOCALS flights. Can the authors point to any other data or references, which show that the cloud thickness variability in a flight day are smaller than the variability from flight to flight?

P10 L5-7: "probably show the impacts of meteorology on S0 within the fixed H, because the cloud data points close to each other with similar H are more likely to experience the same meteorology": Although the authors appear to argue that using larger averaging lengths lead to an overestimation of S0, can you not argue that S0 can be underestimated with a shorter averaging length due to covariance between N and H and smaller spatial scales?

P10 L7: The authors have addressed the similarity in the qualitative behavior of susceptibility between Cu and Sc clouds. Can they comment on how they agree in terms of absolute values? And at which thicknesses the peaks occur? Based on previous studies, is there a prior expectation of whether the peak should occur at the same location (H-value)?

P11 L3-5: "… indicating a longer tc for the clouds sampled at mid-cloud level compared with those sampled at cloud-base." - In the developing stages of precipitation, this may be true, but if the drops start to fall out, they will eventually fall through bottom of the cloud, which means they will have a longer tc at the bottom of the cloud. One would expect the cloud base measurements to be a combination of parcels with short tc and with long tc.

P11 L19-20: Include comparison and references to Mann et al., 2014 and Hill et al., 2015.

P11 L27: "Note that not all of the data shown in Fig. 1 in Terai et al. (2012) are used for the S0 calculations in their study." Because their SR included a component coming from Sf, which took into account non-precipitating clouds, all of the data was used to calculate SR. Not all of the data was used for SI.

P12 L1-3: "As an example, this R threshold rejects all the data in Fig. 2a.." How about the H in the two datasets? Are there overlaps? Do the H and R values agree between the two studies?

P12 L15-16: This is not the case. Their reflectivities were mainly taken from cloud-base retrievals and their N was the accumulation aerosol concentration, not the cloud droplet number.

P12 L25: Would the authors summarize the main conclusion of the paragraph? Is it that Terai et al. (2012) examined relationships in mid-cloud level, where accretion rates are high, and therefore, examine only the downward tail of susceptibility?

P13 L4-5: "This procedure can overestimate precipitation for a given Nd" – please elaborate on why this is the case. In many heavily precipitating clouds, the reflectivity is highest at cloud base.

P13 L28-29: Although the use of sub-cloud aerosol concentration to calculate the susceptibility in Terai et al. (2012) might explain some of the differences between the susceptibility in this study and the susceptibility in their study, they did not make the assumption that the sub-cloud aerosol concentrations and cloud-base Nd were linearly related (see their Fig. 2 and the corresponding discussion).

P14 L4: replace "Terei" with "Terai"

P14 L6-7: "This method possibly could affect/change the slope. . ." One way to test this is to apply their method to the data in this study to determine whether susceptibility values using the data in this study are indeed overestimated when using their method.

P14 L24-25: "We also note that Z increases with height that is consistent with the Hdependent. . .” The Z will also increase with height just from an increase in condensed water that you would get from an adiabatic increase with height.

P15 L26: “the lower R minimum threshold is desirable to use” – what threshold should the minimum be? Should sedimentation of cloud droplets be included? The appendix in Hill et al. 2015 suggests that the asymptotic limit of S0 for small LWP, where ‘precipitation’ is dominated by cloud sedimentation, is 2/3.

Table 1: In the captions please specify what the numbers in parentheses mean.

Figure 2: What does the dashed line indicate?

Figure 3: Please provide the uncertainties in the slopes.

Figure 4: What do the lighter pink colors indicate? As in my previous comment, the uncertainties in the slopes should be shown.

Figure 5a: What do the horizontal bars indicate?

Figure 6: What do the horizontal bars indicate?

Figure 7a: What do A and B indicate?

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