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

Anonymous Referee #2

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The authors prepare an analysis of precipitation susceptibility based on in situ measurements from four field campaigns with nearly identical aircraft payloads, two that sampled cumulus clouds and two that sampled stratocumulus. The authors report robust patterns, and advance hypotheses for the trends that they find, as well as possible explanations for differences between their findings and the results of past studies. The methodology for how to best make such calculations is not clearly settled upon, as evidenced by some of the sensitivity tests presented, but details are sufficient for this work to be reproduced. I rate that revisions to address the following comments can make this into a methodologically sound contribution that is suitable for publication. In particular, I think it needs to be taken into account whether 1-s samples are statistically

independent when evaluating sample size. In addition, the authors seem to conflate sample size and horizontal scale dependence, which appears illogical. I also could not follow the arguments about autoconversion versus accretion using this data set, leaving final statements seeming unsupported by the evidence provided. Comments are enumerated below.

1. There should be some guidance on the spatial scale over which equation 1 applies. A reader must assume implicitly based on this work that it is intended to apply to one second of flight time (100 m in horizontal and full vertical column)? Also a GCM grid cell (100 km in horizontal and full vertical column)? Really both identically? Please offer at least brief guidance for the reader in the introduction.

2. Can you pls comment on whether treating N_d as a proxy for N_a has any relevant consequences? For instance, does that proxy give stronger S_o than using N_a owing to a decreasing fraction of aerosol activated with N_a increasing, all else being equal?

3. I recommend revising the text to reflect the fact that not all current climate models use equation 2, such as those with prognostic precipitation species.

4. Should GCCM be GCM throughout?

5. Using 1-second data, there is a big enough sample volume to accurately calculate Z from dZ/dD? For instance, can you show evidence that your 1-s sample volume is large enough to produce a smoothly continuous DSD? If everyone except me knows that this is possible, perhaps you can just point to a reference or provide a figure outside of the manuscript.

6. Page 6, line 17: It is stated that figure 2 "essentially shows that as N_d increases, R decreases." I would not jump to that conclusion from that figure. The amount of scatter around the trend in figure 3 demonstrates why. I would recommend leaving this statement out of the introduction to figure 2 and focusing instead on the fact that it shows well the range of R and N_d sampled during each experiment.

7. Page 7, paragraph containing line 25: Can the authors present evidence to indicate that sequential 1-s samples are statistically independent? It seems to me that a methodologically appropriate test of sample size for this study would be to randomly resample the data (if consecutive 1-s points are statistically independent) or else randomly resample the flights used (if they are not).

8. Page 8, lines 9-11: Are the authors suggesting to use one LWP profile for each date for Sc and Cu? Unclear if this statement is limited to Sc.

9. Page 10, first paragraph: This logic is not sound as currently written. First the authors state that So decreases with increasing sample size. There must be a limit to that if the system is well-defined and the significance of the results robustly evaluated, right? Then the authors compare such behavior to that found by others when decreasing the averaging length scale, which is a different issue entirely (see comment 1).

10. 2nd paragraph of section 3.2: I really couldn't follow this paragraph. I would remove section 3.2 and figures 5 and 6 if the point of this paragraph can't be significantly clarified. Stating "But it is not discussed here." furthered this reader's impression that the Z analysis did not really add anything to this study.

11. Page 11, line 23: There is no reason to show a figure such as A2. Simply state that results are insensitive. I would be much more interested to see a clear demonstration of a case where the R threshold is very important. It seems clear to the authors, but is not so clear to me how figure 4 would be affected, for instance.

12. Page 14, line 24: I really did not take away the autoconversion versus accretion behavior. There seemed to be a lot of handwaving in section 3.2. I basically feel that this statement is just not supported by the material shown. I think this needs to be much clarified or else removed.

13. I don't understand the last sentence of the paper. The authors call for more studies

on which range of H is most susceptible to preciptiation rate? This is a study on susceptibility of precipitation rate to Nd. Are the authors suggesting another thing? If the authors meant to further study So as defined here, why are further studies needed? All previous sentences in the last paragraph would indicate that the authors have already shown within which range of H Sc and Cu are most susceptible. Are these results somehow uncertain or incomplete? If that could be clarified and its relevance to the conclusions made here (regarding the general behavior of So in Sc and Cu found; is that uncertain?), I think that would better support this closing argument, if I understand it correctly.

14. So many grammatical errors appear here in a paper with so many capable co-authors that I will not take my time to enumerate them, but merely note that this sometimes impacted my ability to evaluate the work (as in comment 13 above).

15. Please label Nd axis units on figures 2 and 3.