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Interactive comment on "Contrasting the Co-variability of Daytime Cloud and Precipitation over Tropical Land and Ocean" *by* Daeho Jin et al.

Anonymous Referee #2

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In "Contrasting the Co-variability of Daytime Cloud and Precipitation over Tropical Land and Ocean", Jin et al. analyze satellite datasets of cloud and precipitation retrievals with the aim of determining the relationship between cloud and rain.

The topic is highly relevant for understanding the behavior of the atmosphere, both in physical reality and in parameterized cloud and precipitation in models. To my knowledge, the authors are the first to use this particular technique of regime-based cloud classification to analyze the relationship between cloud and precipitation, and I therefore recommend the analysis be published.

The authors have chosen a fairly non-straightforward analysis method, and I hope the comments below will help them clarify a few points for the reader.

The main potential weaknesses of the analysis are the following:

1. As the authors themselves point out, using cloud optical thickness and cloud-top pressure to define cloud regimes is an essentially ad-hoc classification based on arbitrary choices. In the conclusions, they then describe the regime classification as "widely accepted". It is true that these regimes are widely used, subject to the known caveats that the authors correctly state (e.g., that the regime names are not to be taken to correspond literally to actual cloud types); however, this acceptance is based on the regimes' usefulness having been demonstrated for each particular application, for example by showing that susceptibilites to aerosol are very different across regimes (e.g., Gryspeerdt et al, ACP 2014). In my opinion, this paper provides some interesting indications that the regime classification does indeed differentiate between cloud type of very different behavior regarding precipitation, but this is the case for only three (out of nine) regimes, so I think it will take some extra work (perhaps the unpublished paper referred to in the Conclusions) before the field will "widely accept" the use of these regimes for precipitation studies.

2. Many of the conclusions are based on regime-composite Pearson correlation coefficients between cloud area fraction and precipitation intensity percentiles. The Pearson correlation coefficient is fraught with pitfalls. The authors would greatly assist the reader in his or her assessment of the robustness of the conclusions by providing:

   (a) a representative scatter plot of the correlated variables in the case of a strong positive correlation and a strong negative correlation and

   (b) a geographic map of correlation strengths for the strongly positively and negatively correlated cloud/precip categories to see, e.g., whether the subsidence regions, ITCZ, warm pool, SPCZ, and maritime continent contribute as expected to the global-mean positive and negative correlations.

3. According to the authors, the TMPA precipitation dataset uses cloud-top temperature to fill in precipitation information where radar is not available. Since cloud-top height information is also used in the regime definitions, I would expect some amount of potentially spurious correlation. Discussion of whether this effect has been considered would be appropriate in the text.

Minor comments:

- Section 2.2, “If the number of bins in the histogram is chosen to be also 16, each bin value falls between 0 and 1 in multiples of 1/16, the sum of all histogram bins at 1° grid cell is equal to 1, and sub-grid precipitation rates are thus converted to areal fractions of specific ranges of precipitation rates”: I don’t quite follow the why 16 is a magic number in the link between the number of bins and area fraction; since we end up with 6, not 16, bins in Sec. 2.3, are those not area fractions anymore?
- Fig. 5: define what is meant by “climatology”.
- p. 11, first paragraph: I find the claimed link between P4, P5, and MCS tenuous; for example, if P5 indicates MCS (where we expect clouds at all levels), why are both Cb and Cs anticorrelated with low- and mid-level clouds?
- Anticorrelation in the Cu case: I am surprised that Cu is so anticorrelated with rain; I always thought (perhaps my thinking is guided by the regime name, which the authors caution against) that this would be the regime that clouds with high in-cloud water content but low area fraction (hence low grid-scale optical thickness).
- Anticorrelation in the Cu case (still): It would be interesting to get to the bottom of whether this is a real effect (CAPE/stability) or shadowing artifact, and I think the authors could easily do it by looking at CloudSat profiles (since they are already

using MODIS data, not much additional co-location would be needed). If it is an artifact, does that mean all of Fig. 8 could be simplified to just the first row of every 3×3 plot? (By the way, I think the matrix of additive/subtractive $P_n > 0$ subsets in Figs. 8 and 9 is brilliant plotting strategy.) Anyway, my first guess at the source of the anticorrelation was open vs closed-cell stratocumulus, and it was interesting to learn that that was not the reason.
- The other surprise for me in Fig. 8 is that cor(CF, f_{prec}) never goes above 0.6. All CBs precipitate, so I would expect the Cb CF should correlate much more strongly with f_{prec}. What am I missing?
- Sec. 4: it should be clarified that the first paragraph is an aspirational statement about the cloud-physics field as a whole, since this study is an incremental advance
- p.16 l.16: if "once detection of low clouds in the presence of high clouds and of warm rain over land improves" refers to the use of active rather than passive satellite sensors, the authors may be interested in Field and Heymsfield or Mulmenstadt et al (both 2015, GRL)
- p.16 l.20: The authors chose not to use L3 instead of L2 data, presumably for reasons of data management complexity. I don’t think anyone would fault them for this choice, so the defensive tone of this sentence is out of place. Either that, or I misunderstood something about it.
- p. 16 l.25: No objection to citing unpublished work, but why not also some published references that show the same thing, e.g., Suzuki et al (2015, J Atmos Sci), Jing et al (2017, JGR)

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