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Interactive comment on "Aerosol indirect effects on the temperature-precipitation scaling" *by* Nicolas Da Silva et al.

Anonymous Referee #1

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The authors present an investigation into the link between aerosol indirect effects (aerosol-cloud interactions) and convective precipitation, using a set of nested simulations with WRF. The study is part of an ongoing investigation by the author team into a topic of high relevance and broad interest. It is well designed, the analysis is adequately presented, and the results well supported. I have a number of minor comments and questions to clarify some issues, but see no major problems that should preclude publication in ACP. I therefore recommend publication subject to minor revisions, and thank the authors for an interesting manuscript.

My one potentially major comment, depending on the answer, is whether the aerosols included in this setup of WRF (and presented in section 2.1) include any amount of shortwave absorption? If they do, then the added heating rate through the atmospheric

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column will also affect convection and stability (rapid adjustments, or the semi-direct effect), which might affect the results throughout the paper. If not, then this is not an issue - but it should still be noted. For a recent investigation of the rapid adjustments due to strongly absorbing aerosols (BC), see Stjern et al. JGRA 2017; this is potentially a very significant effect in some regions.

Minor comments:

* The abstract opens with "Indirect effects of aerosols were found to weaken..." Where? In the present manuscript, or in the previous litterature it builds on? (Both seem to be the case, but please clarify.)

* P1L18: "a hook shape". This term is used throughout the paper, but never fully explained. Please expand a bit, so the reader won't have to dig it out of the references.

* P2L21: Malavelle 2017, Nature Geoscience, should probably also be cited in this context.

* P3L29-30: How are the max and min values in WRF determined? Do they have any physical meaning, or are they simply the endpoints of the validity of some internal parametrization? This matters, because it affects how we should interpret the ranges found later in the study.

* P6L11: Have you tested that daily averaged temperature is indeed representative? How about days with strong diurnal cycle (which would be predominantly low-cloud conditions) vs weak (prevailing clouds), which could have the same average temperature but quite different convective precip event statistics?

* P6L20: most -> more?

* P6L20: Here and elsewhere, consider replacing "SBCAPE" with another term. It is not an intuitive abbreviation, nor short enough to function as a symbol. This becomes very clear on page 12 and in Figure 11, for instance. Why not just E_C, as the rest of the term is clear from the definition?

* Figures 3 and 4: Here I would have liked to see some ranges in addition to the lines. E.g. 25th-75th percentile for the medians, and 90th-99th for the extremes? This helps in interpreting the difference between the cases. Later figures have ranges shown, which makes them very clear.

* P9Eq3: This would be a partial derivative decomposition, I guess?

* P12L16: "Extreme precipitation are mostly of convective nature" -> add "events" and a reference, perhaps? (Or is it still Da Silva 2018? Not quite clear.)

* Figure 11: Again, this just illustrates the concept of partial derivatives... Perhaps this figure is overly complex? The point is made nicely by figure 8 already.

* Finally: This entire study is performed within WRF. That's OK, but I find little discussion of any possible limitations of that particular model. How broadly applicable do the authors think their results are? Are crucial elements still missing, even for WRF at such high resolution? (There is some discussion in the conclusions, but I would encourage expanding a bit on it.)

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