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Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #2

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The paper uses a spring and summer of regional high resolution simulations over Europe to explore the link between precipitation, temperature and aerosol effects. A precipitation temperature-bin approach is used to order the data for high and low aerosol loadings.

I have a couple of main issues with the paper as it stands, outlined below. Until these main points are dealt with i do not think the paper is ready to be published in ACP.

Main points:

1. The results show a statistical relationship between precipitation and temperature, which is fine, but the subsequent comparison with a Clausius-Clapeyron expected increase needs some clarification or modification. The authors are conditionally sam-

pling on precipitation, but across two seasons, this will blend together different weather regimes together as the optical depth vs temperature plot indicates. It seems to me that what is happening here is that different dynamical regimes driven by large scale dynamics are being conflated with the surface temperature. If you isolated one case and increased the atmospheric temperature then i would expect to see the Clausius-Clapeyron-like behaviour, but the negative gradient suggests to me that changes in atmospheric stability driven by the global circulation is the main controller of the T-P relation. I think that this is not a useful comparison between the median observations and C-C as it stands.

I recommend that the data be reanalysed in a way that conditionally samples one type of convection (e.g. 'popcorn convection' only), perhaps by using cloud fraction thresholds. In some ways, the extremes analysis is doing the job of conditionally sampling on strong convection. By focusing on the most intense events the precipitation is probably linked to the strongest convection events in that temperature bin.

2. The title mentions indirect effects. In the introduction the first and second indirect effects are discussed, but observational evidence for the second indirect effect is felt to be inconclusive. That may be true, but the model used in this work does explicitly represent the second indirect effect through modification of the autoconversion process that will lead to reduced precipitation for increased aerosol, all things being equal. Given that one result of this analysis is that indirect effects appear less important than temperature changes it would be simple to confirm this in the model by running a senstivity test with the droplet number-autoconversion link disabled or fixed.

Other points:

p1 line 2-3. Indirect effects.... are these effects caused by increasing aerosol?

p1 line 8. Is this surface temperature or aloft?

p1 line 6-7. I don't follow this sentence. I thought that figure 3c, 4a showed that the mean precipitation did not follow C-C?

p1 line 14. Can you explain more why the first guess is that the extremes are most likely to follow C-C? Is it because you are assuming that these are the most precipitation efficient events that can wring out all of the moisture from an ascending parcel? Can you argue against the means not following C-C?

p1 line 24. What percentiles characterise the extremes referred to here?

p2 line 3. Can you define what is meant by 'hook' shape? Is it anomalously high precipitation for the warmer temperatures compared to C-C?

p2 line 5. ...with respect to ... -> ...in constrast to... ?

p2 line 19-20. ...reduced droplet radius with increased aerosol concentrations for constant liquid water content.

p2 line 25-26 ...through a decrease in evaporation from the surface due... (could be confusion with droplet evaporation).

p2 line 20. Observations may be inconclusive but the model you are using explicitly links aerosol-> droplet number-> autoconversion.

p4 line 4. The MR configuration should also be introduced in this subsection.

p4 line 30. While recognising that this is a sensitivity test - a concentration of 10,000 cm-3 for ice is 1000-100000 times more than typically observed. This is likely to result in large extensive ice anvils that impact the radiative balance of the simulation. If the nudging timescale were longer than 6 hours this might become a problem. What do the cloud fields simulated look like when compared to observations? What does the precipitation time series look like for HR, LR and observed?

p5 line 6 - are these the MR mentioned in the figure 2 caption? Perhaps description should be included in section 2.1?

p6 line 22. How sensitive are the results to the use of values computed 1 hour earlier? How about 2 hours or 30 minutes?

p9 line 8 - 'at the surface' - is this truly at the surface or a screen level value (e.g. 1.5m)?

p12 line 1-2. I don't really follow this. Why should the C-C predict changes in convective precipitation due to indirect effects? Given that the model explicitly represents a suppression of autoconversion due to increased droplet number concentration (from increased aerosol number concentration) the change in precipitation efficiency, to first order, would seem to be more of a predictor of changes in precipitation due to aerosol effects.

p12 line 11-19. This discussion ignores the fact that the microphysical scheme has autoconversion, and related processes, that is directly affected by the number concentration of aerosol and hence droplets. The HR can represent these effects explicitly in the convective clouds whereas the parameterised convection in the LR configuration will not represent aerosol effects. The assertions made here could be tested by disabling the link between droplet number and autoconversion in a sensitivity test of the HR configuration.

p19 conclusions. Figure 14 has no links to changes in microphysical processes directly affected by changes in aerosol. This may be true of the real world, but as far as i can see this was not cleanly demonstrated with the model (see comment about p12 line 11-19).

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