# Aerosol indirect effects on temperature-precipitation scaling

*By N. Da Silva, S. Mailler and Ph. Drobinski*

Response to the comments of Anonymous Referee #2

Dear Anonymous Referee #2,

We are grateful for your careful reading of our manuscript and for pointing out points such as the second indirect effect. We have taken into account all of your suggestions and modified the text accordingly. In the text below, your comments are in italics, our answers in straight black fonts, and the text in blue describes (and generally reproduces) the changes that have been brought to the manuscript.

The Authors.

*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 sampling 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 ClausiusClapeyron-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.*

We do not deny any contribution from large scale dynamics changes in our temperature-precipitation relationship which is indeed spread over several seasons as many other studies of the temperature-precipitation relationship (Lenderink, 2008; Hardwick, 2010; Utsumi, 2011; Drobinski, 2016). The Clausius-Clapeyron scaling is a proxy of the expected precipitation change with constant weather regimes, relative humidity and precipitation efficiency. Comparisons with the CC-law, thus inform us on the validity of the latter hypotheses accross the temperature range covered by these 2 seasons. We found that the CC scaling is quite similar to the scaling of median convective precipitation even across two seasons. The explanation of this scaling is not the main topic of the article, that is why it is not investigated. For median total precipitation, we found a negative slope. Indeed, although not specified in our article, it is most likely due to changes in large scale dynamics between spring season (cool temperatures) and summer season (warmer temperatures).

We believe that adding some possible explanation of the scalings that we observe would clarify the text. It has been done modifying the first paragraph of the result section.

*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.*

If the goal of the suggested reanalysis is to select events with similar large scale dynamics, it does not appear to be necessary in the scope of our article which is more focused on the study of precipitation differences between each simulation. The question of convection type impact on the temperature-precipitation scaling is obviously an interesting question, but it needs to be treated in an entire study.

*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 sensitivity test with the droplet number-autoconversion link disabled or fixed.*

This is indeed a point that seems to be missing in our study, but several issues forced us not to do these additional simulations. The «all things being equal» assertion would not be respected when running such a sensitivity test. In particular, when one would modify the autoconversion rate it would also have an effect on the averaged mass of precipitating clouds and then on cloud albedo. Thus, accelerating the autoconversion rate in the MAX simulation may as well reduce the radiative background effect in such 6-months simulations. A potential solution was to initiate each day of this new MAX simulation by the old MAX simulation, considering that the radiative effect is an effect that forms only trough several days of simulation (which might not be totally true). The problem of such a setup is that the same water may be precipitated several times in the simulation, which would overestimate the second indirect effect.

In our study, the precipitation extreme scaling budget clearly shows that changes in convective/extreme precipitation are similar to changes in vertical velocity and are not very sensitive to changes in thermodynamics, which indirectly discard an important effect of precipitation efficiency. Another argument is the similarity of the curves (fig. 6b vs fig. 7b) with and without convective parameterization.

*Other points:*
*p1 line 2-3. Indirect effects.... are these effects caused by increasing aerosol?*

These effects are indeed caused by an increase in aerosol concentrations used in the microphysics scheme.

*p1 line 8. Is this surface temperature or aloft?*

It is precisely the temperature at the first vertical grid level. The term « surface » has been added in the abstract of the new version of the manuscript.

*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?*

Indeed, as shown in figure 3c and 4a, mean precipitation does not follow the C-C law. However the abstract mentions convective precipitation, which are displayed in figure 3a (medians) and 3b (extremes), and which do follow the C-C law.

*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?*

It is expected that extremes would follow the CC law since extremes are supposed to remove all the vapour content of the atmosphere. On the other hand, mean precipitation are constrained by an energetic budget between atmospheric radiative cooling and surface sensible and latent fluxes (Allen and Ingram 2002; Held and Soden 2006; Muller and O'Gorman 2011; Muller et al 2013). As a result, the increase of mean precipitation is expected to be lower than the one of precipitation extremes with temperature. This discussion has been added in the first paragraph.

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

99th and 99.9th for hourly precipitation and only 99.9th for daily precipitation.
It has been added between parentheses in the text.

*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?*

'Hook shape' has the same meaning than in the Drobinski et al. (2016) study. It refers to the shape of the temperature-precipitation scaling which displays an increase slope at low temperatures, a precipitation peak at middle temperatures, and a negative or weaker slope at high temperatures. The definition has been added in the text.

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

The text has been modified accordingly.

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

The text has been modified accordingly.

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

The text has been modified accordingly.

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

The second aerosol indirect effect is discussed in the core and in the conclusion of the new manuscript (cf other points).

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

The text has been modified accordingly.

*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?*

Extreme aerosol concentrations were taken to avoid noise, that we observed at first when realizing a pair of simulation with a factor 2 in aerosol concentrations. As indicated in the table of Da Silva et al. (2018) study, there are some unrealistic values in terms of drop number, liquid water content, cloud optical depth. But these values only affect a little the radiative budget at the surface (while being sufficient to decrease surface temperatures by 0,5 K in the MAX simulation). Thus the extreme change of aerosol concentrations does not result in a drastic change in the radiative balance of the simulations. The reason is that the parameterization of cloud condensation only depends on sursaturation and not on aerosol concentration. It means that an increase in aerosol concentration does not explicitly favors condensation. It leads to very thick anvils but not necessarly larger. The precipitation time serie of one grid point for both HR and LR simulations is also shown in the Da Silva et al. (2018) study and seems realistic.

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

This sentence indeed refers to MR simulations. The meaning of MR has been added to the manuscript.

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

Results were found similar when using values computed at the same hour or 2 hours before, with sometimes higher variability. The 1 hour earlier was chosen arbitrary, and considering that the output frequency is hourly.

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

It is taken at the first grid vertical level that is around 28 m above the surface for oceans. For lisibility, we used 'surface' to designate this level. A parenthesis has been added in the method section : « (centered around 28 m above the ground, hereafter referred to as surface) »

*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.*

Indirect effects were found to reduce surface temperatures and thus water vapor availibility for precipitation. In the hypothesis of constant relative humidity, convective instability and precipitation efficiency, the change of convective precipitation would be similar to the one expected by the CC law.

Instead, the change in surface temperature is accompanied by a change in convective instability which has much more impact. For precipitation efficiency, note that it can only be impacted indirectly in the LR simulation, since the convection scheme does not explicitely take into account aerosol concentrations. In the HR simulation, precipitation efficiency, while hard to evaluate, is indeed an explicit function of aerosol concentrations. However for extremes, we can see that the relative changes in precipitation are very similar to the one of vertical velocity, suggesting that the contribution of precipitation efficiency is also low.

*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*

A discussion was added at the end of this paragraph with justification on discarding the second indirect effect of aerosol (as stated above).

*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).*

Figure 14 is a snapshot of the bigger scheme presented in figure 1. The conclusion has been enlarged to discuss the second indirect effect.