

Aerosol indirect effects on temperature-precipitation scaling

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

Response to the comments of Anonymous Referee #1

Dear Anonymous Referee #1,

We are grateful for your careful reading of our manuscript and for pointing out points such as the fact that precipitation extremes were dominated by convective precipitation. 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.

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 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.

We used 2 different aerosol climatologies in this setup, one for the microphysical scheme and one for the radiative scheme. In our sensitivity experiment, only the climatology of the microphysical scheme is modified. Therefore, aerosols do include shortwave absorption, but this absorption is identical in both simulations since the aerosol radiative climatology is the same.

This explanation is in the text on line 16 of page 4:

« Another climatology of aerosols from Tegen et al. (1997) is used in this radiative scheme and therefore is not affected by any changes in the microphysical aerosol climatology, which enables us to perform sensitivity experiments of the indirect effects of aerosols with fixed aerosol direct effect. »

A reference to the study of Stjern et al. (2017) has been added in the introduction.

«They have shown that the consecutive surface cooling not only reduces the water content but also stabilizes the atmosphere as suggested by Fan et al. (2013); Morrison and Grabowski 2011; Stjern et al. (2017) ...»

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

It refers to the previous literature, and especially the Da Silva et al. (2018) study, for which the present manuscript can be seen as a follow-up. The first sentence of the abstract has been modified in order to be explicit:

«Convective precipitation are known to be negatively affected by aerosol indirect effects through reduced precipitable water and convective instability, as stated in the previous literature.»

* 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.

This term has been explicited in the new version of the manuscript:

«Although less documented than extremes, a "hook shape" of the temperature-precipitation relationship, that is a positive slope at low temperatures and a negative slope at high temperatures, is also suggested for mean precipitation (Zhao and Khalil, 1993; Madden and Williams, 1978; Crhova and Holtanova, 2017; Rodrigo, 2018) as well as differences between land and sea areas (Adler et al., 2008; Trenberth and Shea, 2005).»

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

This citation was added in the manuscript.

* 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.

These values maximize the potential effect of aerosols and correspond to the lowest and highest values that the microphysical parameterization tolerates. They are too extreme compared to observations and therefore do not have any physical meaning. The ranges found later in the study should be interpreted as upper bounds. The following sentence has been added in the 'simulation experiment' section: « It is however important to keep in mind that the ranges that will be found in this study should be interpreted as an upper bound of aerosol indirect effects. »

* 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?

The choice of the daily averaged temperature has also been done for consistency with previous literature. However we have not tested if this temperature is indeed representative of the air mass. Days with strong diurnal cycle (sunny days) versus days with weak diurnal cycle (cloudy days) are indeed confused in our analysis. We think that the daily averaged value is not perfect, but might be more representative of the airmass than an instantaneous value which is might be affected by rain for example.

* P6L20: most -> more?

It has been corrected in the new version of the manuscript.

* 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?

SBCAPE is the usual abbreviation to state for the convective energy that would acquire a parcel that is raised from the surface. It is used by several prediction centers like the Storm Prediction Center and the European Severe Storm Laboratory. It might be subjective, but we think that SBCAPE is

more intuitive than E_C which is less used in the literature. However, we admit that this term is a bit long to be used in our study. Since the fact that it is a 'surface-based' CAPE is not discussed in our article (with respect to other possible CAPE calculations such as 'Most Unstable' CAPE), we propose to remove SB and keep CAPE. [It has been modified in the new version of the manuscript.](#)

** 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.*

[The new version of the manuscript include ranges for these figures. The ranges chosen are the 95% confidence intervals, as in figure 6 and 7. Descriptions of these errobars have been added in the caption of the corresponding figures.](#)

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

It is indeed a partial derivative decomposition done from the logarithm of Eq. 2:

$$\ln(Pr) = \ln(\varepsilon) + \ln(W) + \ln(Q) + C$$

where C is a constant.

$$d(\ln(Pr)) = d(\ln(\varepsilon)) + d(\ln(W)) + d(\ln(Q))$$

which gives:

$$\frac{dPr}{Pr} = \frac{d\varepsilon}{\varepsilon} + \frac{dW}{W} + \frac{dQ}{Q}$$

Assuming small changes between both simulations, one can write the approximate equation by replacing infinitesimal differences (d) by differences between both simulations (Δ):

$$\frac{\Delta Pr}{Pr} \approx \frac{\Delta \varepsilon}{\varepsilon} + \frac{\Delta W}{W} + \frac{\Delta Q}{Q}$$

[In the manuscript, the equal sign has been replaced by an approximately equal sign.](#)

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

This sentence was written in order to explain why the scaling seems to work better for extreme total precipitation than in median total precipitation. The argument is that the proportion of convective precipitation is higher in total extreme precipitation than in total median precipitation. Since the scaling ($Pr = \varepsilon W Q$) is more adapted to convective precipitation, one can expect a better fit for extreme total precipitation than for extreme median precipitation, which is actually the case. Loriaux et al. (2013) stated that "On an hourly time-scale, precipitation extremes are predominantly stratiform at low temperatures, while at high temperatures convective extremes become dominant." In our LR simulations, we found that convective precipitation start to dominate precipitation extremes from 10°C (not shown), which corresponds to almost the whole range of temperature in our case. On the contrary, we found that stratiform precipitation dominate median precipitation for the whole range of temperature. It is therefore conform with our explanation.

We added a short explanation and a reference to the work of Loriaux et al. (2013).

** 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*

It indeed illustrates the concept of partial derivative. This figure was done to introduce variables that are discussed in the text, so that the text is easier to read and the reader can refer to this figure if needed.

** 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.)*

We expanded a bit the conclusion in order to reveal the main limitations of our model setting. A deeper discussion is made on the previous companion paper (Da Silva et al., 2018).