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Reply on RC1

Kalli Furtado and Paul Field

Author comment on "A strong statistical link between aerosol indirect effects and the self-similarity of rainfall distributions" by Kalli Furtado and Paul Field, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-443-AC1, 2021

We agree with you that to get an idea about the true universality of the result, we should ideally simulate more cases, at different locations, and with different models.

We have added a new section (Section 7.3) that:
1) constructs the universal distribution for 20 case studies of mid-latitude weather, using a convection-permitting (1.5km resolution) regional model over the UK;
2) constructs universal pdfs for three large (17 x 12 degree) regions, covering tropical Pacific, north-eastern Pacific, and Southern Ocean, from 20-years of daily mean precipitation from AMIP simulations with a global climate model.

The inclusion of the UK cases samples a variety of different meteorological regimes. The global simulation samples a different microphysics scheme (a single-moment scheme), and convection-scheme rainfall, in three different climatological backgrounds (tropical deep convection, subtropical stratocumulus, and mid-latitude storm tracks). The results show that the 'universal' distribution is approximately independent of the factors sampled. Please see our revised manuscript for the full details.

We also agree with your suggestion to try and strengthen the statistical robustness of the results. The new analysis of other model configurations, different regions, and more cases, supports the conclusion that the existence of a widely applicable re-scaling is statistically robust.
To physically motivate this statistical property, we have also added a new section (Section 7.4) which gives a theoretical argument for the existence of a universal scaling, based on a stochastic rainfall generator. The generator treats rainfall at each point as a sum of independent precipitation 'events' with durations and intensities drawn at random from underlying probability distributions. We show that this stochastic model also has data-collapse to a universal distribution. Please see the revised manuscript for full details.

You made several, very helpful specific points which are addressed in-line below (and in the manuscript):

Specific comments:

RC: Line 25 to 28: There are also studies that indicate that, on average, the amount of precipitation is not influenced due to the buffering effect of clouds. The idea of buffering is described in Stevens and Feingold (2009) and a study indicating that there might, on

average, be no influence is Seifert et al (2012) (although the latter study is based on an operational NWP model, i.e. the coupling to aerosol is limited).
Response: we've added the following "*Furthermore, Stevens and Feingold (2009) suggested that in some situations the answer to this question* [of whether ACIs increase/decrease precipitation] *is 'neither', because systems of clouds adjust to counteract the aerosol-induced changes in precipitation. This implies that although an individual cloud may have a large response to aerosols, changes in the amount of precipitation average over an area may be much smaller. This was illustrated by Seifert et al (2012) who showed that aerosols had negligible effects on precipitation over a large range of region numerical weather predictions.*"

RC: Hence at this point it is important to state the context more clearly: Do you ask the question for a specific cloud? Do you ask the question for the amount of precipitation averaged over an area?
Response: We now address this explicitly after the bullet-points 'aims' in the Introduction:
"*We will primarily consider these questions in relation to the area- and time-averaged statistics of rainfall over a large domain for a case of typical case of monsoon rainfall over East Asia.*"

RC: See also the paragraph starting at line 90.
Response: we've added a citation to Siefert et al (2012) in this paragraph.

RC: Line 30 to 32: I do have problems understanding this sentence. Not every cloud produces rain and a cloud is an example of a system with unbalanced sources and sinks (otherwise the cloud would not have formed)? Please clarify.
Response: I think there is an implicit steady state assumption in Khain's 2008 framework which implies balanced (i.e., steady-state) systems where sources and sinks are approximately equal over a period of time; it therefore probably doesn't apply well to the formation/dissipation of individual clouds. I've tried to clarify this as follows:
"*If the precipitation rate adjusts over time to a slowly varying state in which sources and sinks of condensate approximately cancel out, then an aerosol change which increases the sources more than it increases the sinks will necessarily lead to an increase in the amount of precipitation.*
Therefore, if we consider a two systems both precipitating at rate, P, and subject one to an aerosol perturbation, the perturbed system will evolve to a new state with a (possibly) different precipitation rate, P+dP, where dP is due to a change in the net source of condensate relative to the
unperturbed system."

RC: Line 120 to 121: You refer to the domain of your simulation by pointing the reader to a plot of radiative fluxes. You should either only state the domain of your simulation by indicating the geographical coordinates or adding a geographical map. I prefer the latter.
Response: I hope it's okay that I've opted to specify the coordinates in the text (17 ◦ –35 ◦ N,97 ◦ –126 ◦ E) because there are already quite a few figures.

RC: Line 127 to 128: To which degree do your results degree on the choice of the lateral boundary conditions for the aerosols?
Response: The new results for a regional NWP over the UK (Section 7.3) which uses a different aerosol model, suggest that scaling of the distributions is independent of LBC details.

RC: Line 296: You refer the reader to a figure in the supplementary material; please include the figure in the main text.

Response: If possible, I'd like to keep this in the supplementary (there are so many figures already)

RC: Line 317: The predictions do not always reproduce the simulated values, e.g. the black circle in panel b is off; also in panel d a more stagnant behaviour is predicted instead of the decrease that is visible in the solid line.
Response: Agreed! The original was too strong – I've revised it to:
"*In most cases, the predictions are able to reproduce the simulated values of the moments reasonably well. The agreement is slightly less good for some values of the single-moment reconstructions and for the highest-order moment tested.*"

RC: Caption of Figure 4, fourth line: The sentence within the brackets appears exaggerated to me. You can only assess the sensitivity to your experiments, which is different from a "universal" sensitivity.
Response: Changed "*universal*" to "*dimensionless*".

RC: Figure 6: Is there a motivation for the thresholds used? Why not use "simpler" values, e.g. 0.5 instead of 0.4 or 29 instead of 28.7?
Response: We deliberately chose cloud-regime boundaries that isolate the three rainfall regimes shown in S3 as well as possible (See discussion on lines 357–360)

RC technical corrections:
Response: Thank you very much for finding all these! All corrected (with the exception of Fig. 9 axis-labels for which I'd rather keep the full labels, if possible, because these are more precise descriptions of the regimes).