The manuscript by Lenderink et al presents a comprehensive evaluation of convection-permitting model (CPMs) performance for simulating rainfall (and extreme rainfall) relative to regional climate models (RCMs). The distributions of the rainfall, the rainfall-temperature relationship, and the rainfall-relative humidity relationship, and the differences between climatic regions are all better respected in CPMs as compared to RCMs. Moreover, the authors discuss in a very transparent manner the possible short comings of CPMs which will surely lead to improved model performance in the future as these short comings are addressed in future research.

As we move to a world where planning decisions are made on the basis of including more and more climate model results (rather than historical observations) building trust and confidence in the model results, as well as improving model results, is paramount. This manuscript addresses both these aims making it an important and impactful manuscript. This is a very thorough evaluation and hence I unreservedly look forward to this manuscript's publication.

I have comments below which I hope the authors will consider, but I should note, apart from errors I spotted in the supplementary figure numbers, the rest of the comments should be treated as suggestions. Although I feel it would be remiss of me to not suggest references that I am aware of, the authors should at no point feel obliged to cite them unless they feel they would enhance their manuscript. So again, I reiterate my comments should be treated as suggestions, not prescriptions.

Thank you very much for your comments. Below, I will briefly answer to your questions and comments in red.

General comments:

The robustness of the super Clausius-Clapeyron scaling wasn't explicitly stated in the abstract or conclusions and this could be added if the authors wish.

We will add a few lines on this in the conclusions.

The interesting result of rainfall being more intense at lower RH appears is well discussed. I had a thought that this could also be affected by the drop in temperature across events which becomes greater with rarer events (Figure 4 in Barbero et al) but I assume that picking the temperature before the rainfall event negates this effect? The authors may wish to comment on this in the manuscript but shouldn't feel obliged to. Apologies if this was already mentioned and I missed it.

That's an interesting thought, and thank you for pointing out Figure 4 in that paper. Indeed, more intense events are associated with stronger temperature drops. There could be several causes of that. One is that stronger temperature drops are associated with stronger large-scale disturbances, perhaps with colder higher level temperature (and therefore higher CAPE). The other is that stronger temperature drops could be well related to stronger cold pools, and since these are (partly) driven by evaporation of rain boundary layer relative humidity comes into play. In fact even the temperature drop is a rather good measure to distinguish events satisfy 2CC relation versus lower scaling events. Below, you will find scaling derived from sub-selecting on the temperature drop between 2 hours before the rainfall event and one hour after. It is clear that larger temperature drops are associated with stronger rainfall extremes. But also sub-selecting on negative temperature drops most of the super CC behavior disappears. We will add this plot to the supplement.

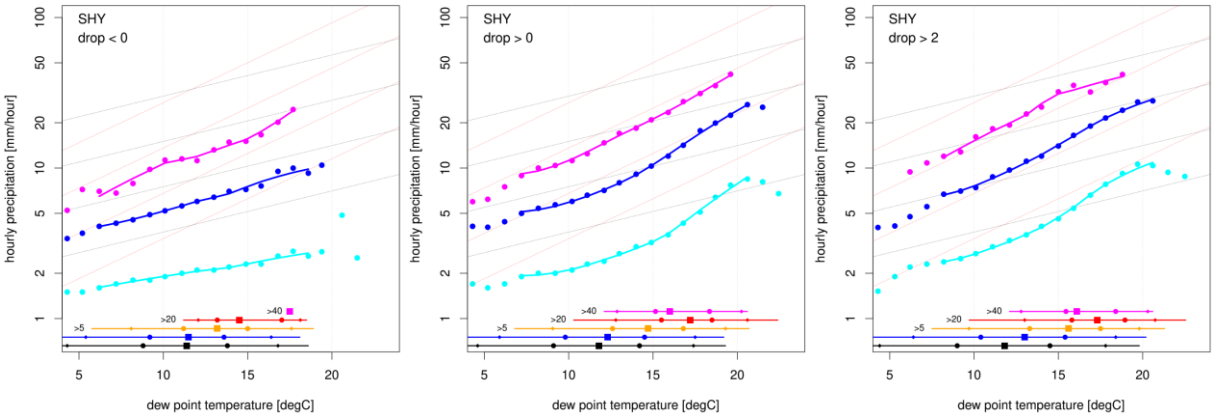

Figure 1. Scaling on sub-selections of data characterized by the temperature drop between two hour before and one hour after the rainfall measurement. From left to right, a temperature rise (negative drop), temperature drop, and temperature drop exceeding 2 degrees.

Barbero, R., Westra, S., Lenderink, G., Fowler, H.J., 2017. Temperature-extreme precipitation scaling: a two-way causality? Int. J. Climatol. 38, e1274–e1279. https://doi.org/10.1002/joc.5370

Line by line comments:

Line 45: The reference below showed RCM parameterisations fail to capture spatial dependencies of rainfall.

Li, J., Wasko, C., Johnson, F., Evans, J.P., Sharma, A., 2018. Can Regional Climate Modeling Capture the Observed Changes in Spatial Organization of Extreme Storms at Higher Temperatures? Geophys. Res. Lett. 45, 4475–4484. https://doi.org/10.1029/2018GL077716

Thank you for this reference, which will be added.

Line 52: Where you say "when future changes are dominated by simple thermodynamics" when is this exactly? I think it is for long duration (daily) rare rainfalls, but it would be nice to state this explicitly. Having said that, if a short duration event is embedded in a long duration event, then convection permitting modelling remains crucial.

This is indeed the case. But we agree that the distinction between the two situations is not so clear. We can add there if two conditions are met: "large-scale dynamics control the event and these large-scale dynamics do systematically change" . But to be fair, we think that is more a statistical statement, and it will be hard to proof this to be the case for an event.

Line 72: "depend on the temperature measure used" – I wonder if something could also be said for the need to make sure the temperature is the one driving the event by making sure rainfall events are independent when captured. When this is done this gives reduced variability in the rainfall-temperature scaling. This conclusion in Visser et al is ultimately the outcome of these three manuscripts.

Barbero, R., Westra, S., Lenderink, G., Fowler, H.J., 2017. Temperature-extreme precipitation scaling: a two-way causality? Int. J. Climatol. 38, e1274–e1279. https://doi.org/10.1002/joc.5370

Schleiss, M., 2018. How intermittency affects the rate at which rainfall extremes respond to changes in temperature. Earth Syst. Dyn. 9, 955–968. https://doi.org/10.5194/esd-9-955-2018

Visser, J.B., Wasko, C., Sharma, A., Nathan, R., 2021. Eliminating the "hook" in Precipitation-Temperature Scaling. J. Clim. 34, 9535–9549. https://doi.org/10.1175/JCLI-D-21-0292.1

We indeed made no effort to distinguish between events. We will mention that in the discussion. We also note that in this paper Lenderink G, Barbero R, Loriaux JM, Fowler HJ (2017) https://doi.org/10.1175/JCLI-D-16-0808.1 we clustered rainfall into events, and found similar scaling.

Line 95: it might be worth mentioning that such reductions in relative humidity have also been observed and not just modelled?

Denson, E., Wasko, C., Peel, M.C., 2021. Decreases in relative humidity across Australia. Environ. Res. Lett. 16, 074023. https://doi.org/10.1088/1748-9326/ac0aca

This is definitely true, and we will mention that. Unfortunately, we are unaware of such a paper for Europe.

Line 170: I could be wrong here, but was Pmean used at all in the manuscript? I got the impression that only Psample was used so I was a bit confused to why these were both defined.

Indeed, the paper uses the sample value. But we did all analysis also with the mean value. We will add a figure to supplement to show the difference between sample and mean value.

Line 226: Is the "all time-steps" including zero rainfall time-steps or just the wet time steps?

This is including dry hours; we will add that.

Section 3.1: There was a really comprehensive discussion of why some models were better/worse for the RCMs but for the outlier for the CPMs (UKMO-UM), it wasn't discussed as to the reason why this might be the case – this could be added around lines 235-240.

If this is known by the Met Office I will add this, but due to holiday time no information yet from them.

Figure 3 caption: The parentheses for the last line are probably not required.

OK

Line 251: The higher intermittency I assume may lead to drier soils and less humidity? The link to the above sentence that discuss model performance was missing because the above sentences talk about the model performance in terms of simulating temperature.

Lines 252-255: I wasn't really sure what figure I was supposed to be looking at, so I didn't really understand this part I am sorry. I think Figure 3 was introduced but then the discussion shifted to discussing Figure S2 and this wasn't clear.

We will clarify this paragraph, better explaining that we look at humidity (dew point temperature as dew point depression), and the link to soil moisture. We will also add the reference to the appropriate Figure to the text.

Lines 266-274: I am not sure the results of the DPD for median and 80th percentile (middle and right columns) were contrasted in the text?

We will add a sentence that it is robust, so both median and 80th percentile showing very similar results

Line 319: “similar to a quantile difference plot” - I think an “a” is missing in this sentence.

OK

Line 302: Again, the link of soil memory and runoff to the temperature simulated is missing.

I am not sure whether I understand this because we do not consider temperature as a scaling parameter. But will clarify this in earlier lines (252-255).

Line 348: Can the figure numbers in Supplementary be listed? I think this sentence needs rewording because when you say “plots for individual models are presented in supplemtnary” this implies that Figure 5 is pooled model results, but they are just observations correct? Maybe saying “plots comparing observations to individual models are presented in supplementary and are summarised in Figure 6?” would be clearer.

Indeed, Figure5 is observations only. We will reformulate this.

Line 349: “For further investigation…” should this say, “For further investigation and comparison to the model simulations”? I think it could be made explicit that you are now shifting from observations to also looking at model output.

Yes, indeed. Thank you

Figure 6: I think the caption is wrong – it says NL is presented, but the text at Line 353 says NL is in the supplement. The caption should also mention the percentile analysed. Finally, I think you mean Figure S10 and not Figure S6?

Thanks for spotting this error. We will correct this error.

Line 350: Although the split of low and high RH was explicitly stated in the methods it could be mentioned here again to remind the reader?

A good suggestion

Line 417 – I think you should mention the figure number in supplementary.

OK

Line 446 – “Area” could maybe be “regional” differences?

A good suggestion