

Anonymous Review of the manuscript “Regional Climate and Convection-Permitting Modelling of heavy precipitation in decadal simulations of the greater Alpine region with COSMO-CLM” by Caldas-Alvarez et al.

The manuscript has changed substantially since the initial submission, and obviously a lot of work went into it. I appreciate that my extensive comments from the last review round were carefully addressed. I have a few remaining issues that, whilst still important, can be solved rather quickly.

Major Comments

I do not fully agree with the statement about the overestimation of precipitation over the Alps (L22/L542). For this argument, the authors primarily refer to studies that apparently support their claims. However, the authors of these studies are more careful about that statement, and typically refer to uncertainties related to the gridding procedure, sampling biases due to the gauges being primarily located in valleys, and the prominent under-catch issue by gauges during HP events. I agree with these studies that, while generally useful, the current datasets/observations are not fit for making the “HP overestimation in CPMs” claim in the Alps.

I am also a bit concerned about the discussion of IWV and its relation to HP (L30 - 35 and L565 – 587). In particular, I did not fully comprehend the presented argument. That is because the authors primarily present results, rather than their idea, and leave most of the interpretation to the reader. It would be helpful if the assessed hypothesis were concisely stated in the introduction, and then assessed in Chapter 8. I currently interpret the results as if the authors alluded to IWV (and remote sensible and latent fluxes over the ocean) being an important driver for differences in HP between RCM and CPM. I don't think that the presented analysis would convincingly outline how such a mechanism would work.

Minor Comments

L75ff: The argument presented by Hohenegger et al. (2009) has meanwhile been augmented and better understood. In particular, the sentence starting on L77 is now outdated. More recent hypothesis actually involve a spatial scale dependency ;-) (e.g., Taylor et al. 2012), that is actually represented in kilometer-resolution climate simulations (e.g., Leutwyler et al. 2021).

L181/L252: In the revised version of the manuscript the 80th all-day percentile is used, right? Maybe thus mention “all-day” explicitly in the text?

L303: The text reads as if the authors are talking about the probability of exceedance. Why not show this metric instead (Figure 5)?

Section 6/Figure 9: The authors explain what they did, but I do not fully comprehend what they want to demonstrate. For unfamiliar readers (like me), it might be worthwhile to add a sentence or two explaining the intent at the beginning of the Section (same for L440ff), and a few words at the end summarizing the findings.

L485: I do not understand the role of the green shading. Is there something awkward with “negative” sensible heat fluxes? I think the authors need to explain better what the problem is. Also, I am not familiar with the term “surface directed fluxes” used in the caption of Fig 12, and I do not immediately grasp what it means. Please explain it in the caption.

L497 – L507: I think these paragraphs need to be rewritten. They read like notes rather than actual paragraphs, also I am confused what the underpinning message is.

L572: “overestimates” and “overestimation”. I think these words can only be used when comparing against observations. What is wrong with the word “larger”?

L576: “The wind transports the moisture excess in RCM inland.” How do you know that? I think such a statement would require a moisture budget, possibly even including a trajectory analysis.

L595: Maybe the original studies merit citation instead? Chubb et al., (2015) provide a nice summary.

Suggestions

Title: “Regional Climate and Convection-Permitting Modelling of heavy precipitation ...”
Maybe better: Convection-Parameterizing and Convection-Permitting Simulations of Heavy precipitation [...]. The term RCM usually refers to the limited-area extent of the computational domain rather than to the parameterization of convection.

L97: Maybe add a sentence outlining how the method will be exploited in the presented study?

L335: Maybe explain what you mean with “event scale”?

L404ff: One might summarize the surface T and QV verification by stating that the event scale verification is consistent with the well-known too dry too hot bias of CPMs. In particular because most of the assessed events are in summer.

L566: I thought you wanted to remove the term “moisture excess” from the manuscript.

References

Chubb, T., Manton, M. J., Siems, S. T., Peace, A. D., & Bilish, S. P. (2015). Estimation of Wind-Induced Losses from a Precipitation Gauge Network in the Australian Snowy Mountains, *Journal of Hydrometeorology*, 16(6), 2619-2638.

Leutwyler, D., Imamovic, A. and Schär, C. (2021). The Continental-Scale Soil Moisture–Precipitation Feedback in Europe with Parameterized and Explicit Convection, *Journal of Climate*, 34(13), 5303-5320.

Taylor, C. M., R. A. M. de Jeu, F. Guichard, P. P. Harris, and W. A. Dorigo, 2012: Afternoon rain more likely over drier soils. *Nature*, **489**, 423–426.