

## **Resolution dependence and biases in cold and warm frontal extreme precipitation over Europe in CMIP6 and EURO-CORDEX models**

by Armin Schaffer, Tobias Lichtenegger, Albert Ossó, and Douglas Maraun

Dear Armin Schaffer

Thank you for already addressing the reviewers' comments in your final author comments. Both reviewers are very positive about the merit of your work and addressing their comments will make the paper even clearer and stronger. I have few additional points, which I invite you to consider when preparing the revised version of your paper:

- General remark: you currently place many figures in the Appendix, and some paragraphs discuss mainly figures that the reader finds in the Appendix. I wonder whether this is ideal for the reader, because they have to flip many pages back and forth when reading the text and looking at the figures. I wonder whether it would not be better to put these additional figures into a Supplement. It is easier, in my view, for the reader to have two documents open, the main paper and the supplement than to navigate between text and appendix. Maybe Fig. A1 is an exception, this figure is fine in the appendix.
- L9: not sure that “underrepresentation” is the best term here, I would write “misrepresentation” or “inadequate representation”.
- L13: maybe better “larger horizontal scales” are WF are typically shallower than CF.
- L30: please put references in chronological order.
- L33: I would use here past tense “we examined ... and analyzed ...”.
- L45: maybe better “... in a study based on climate model simulations.”
- P3 footnote 4, I don't understand “with geopotential missing 725-275 (50 hPa steps)”. Do you have no geopotential data between 725 and 275 hPa? What is meant then by 50 hPa steps?
- L61: correct formatting of references (years should not be in (...)).
- Section 3.1: I appreciate that you try to be brief here and not repeat too much information from your previous paper. However, currently it is unclear what the TFP is → either explain or skip entirely. Also, I recommend that you reference the origin of this front detection method (I assume it goes back to some studies by Tim Hewson).
- L92: not sure that the term “back-bent occlusion fronts” is standard. First, I would write only “occlusions” instead of “occlusion fronts”, and, more importantly, I think the original idea by Shapiro and Keyser when introducing the concept of the bent-back (warm) front was that this type of front is not an occlusion. I would write “exclude bent-back fronts from ...”.
- L93: not clear to me what you regard as the warm conveyor belt here.

- L106: consistent with a reviewer comments, I would not call a return period of 50 days as “extreme”. Maybe “intense/heavy precipitation events” would be more appropriate.
- L107: this reads complicated, can you explain this better such that others could reproduce your approach?
- L113: not clear what “standardized” means here.
- L116: please remind the reader what these regions are – it is not convenient if the reader has to search for important details in another paper.
- Figures 7 and 8: this is, in my view, the highlight and main novelty of your study. It is therefore a pity that the panels are so small. You currently use a lot of space for the headings (e.g., “ERA5 2deg”). I suggest that you place the labels next to the panels (instead of on top) and thereby you can enlarge the vertical dimension of your panels.
- L224: please give some indication how these extreme fronts have been selected.
- L230: my view on these interesting results is that the differences between ERA5 and the climate models are substantial, in particular for vorticity in cold fronts (Fig. 7). Please discuss more whether this could be an effect of vertical resolution. How do the vorticity cross sections look like for ERA5 if you only use the limited vertical levels available for some of the climate models? Why is vorticity particularly “wrong” near 500-700 hPa?
- L254: I find this concluding statement a bit too positive, given the results shown in Fig. 7.
- I don’t think that Figs. A8-A11 are discussed in the text. In particular, I have a hard time understanding Figs. A10 and A11 in comparison with Figs. 7 and 8: what are these “synoptic” vorticity and convergence fields? I suggest removing them from the paper, as they were not discussed in the text and therefore not considered by the reviewers.

Looking forward to receiving the revised version of your paper.

With best regards,

Heini Wernli