

Resolution Dependence and Biases in Cold and Warm Frontal Extreme Precipitation over Europe in CMIP6 and EURO-CORDEX Models

Response to the Editor

Dear Heini Wernli,

We again thank you for your constructive feedback. Below we address each of your comments in detail and highlight the changes to the manuscript.

- 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.

We moved Figure A2-A11 to a separate Supplementary Information file (Figs. S1-S10).

- L9: not sure that “underrepresentation” is the best term here, I would write “misrepresentation” or “inadequate representation”.

We agree and change the wording to “inadequate representation”.

- L13: maybe better “larger horizontal scales” are WF are typically shallower than CF.

We agree, “larger horizontal scales” is more accurate description of the fact that warm fronts are shallower and thus extend further.

- L30: please put references in chronological order.

We changed this and one more citation to be in chronological order.

- L33: I would use here past tense “we examined ... and analyzed ...”.

We adapted the sentence accordingly.

- L45: maybe better “... in a study based on climate model simulations.”

We agree. This makes the sentence more readable.

- 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?

We already addressed this point in a reviewer comment. The levels 725, 675, 625, ... 325, 275 hPa (10 levels) do not have geopotential data. We used CMIP6 model levels to generate highly resolved pressure level output, but geopotential is not available on model levels on the ESGF servers. To make this even more clear, we changed the footnote to: “1000-200 (25 hPa steps), **with 10 levels missing geopotential data (725, 675, 625, 575, 525, 475, 425, 375, 325, 275 hPa)**”

- L61: correct formatting of references (years should not be in (...)).

We corrected the citation style.

- 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 à 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).

We included a more detailed description of TFP: “The final frontal points are identified as local maxima in $\nabla\theta_e$ where the Thermal Front Parameter (TFP) is closest to zero. **TFP is defined as: $TFP = -\nabla|\nabla\theta_e| \cdot \nabla\theta_e/|\nabla\theta_e|$ and measures the rate of change of $\nabla\theta_e$ in direction of the gradient.**”

Our front detection method combines multiple methods from different studies with the most important ones being Hewson (1998) & Jenkner et al. (2009) and was developed by multiple people in our institute (e.g. Ritter, 2014). We discussed the choice of method in detail in our previous paper. This is why we did not include this discussion in the current manuscript. However, we now acknowledge the groundwork these studies performed by starting the section with: “**The front detection scheme employed in this study follows established approaches in the literature (e.g., Hewson, 1998; Jenkner et al., 2009), in which regions of strong thermal gradients are identified and a wind-based threshold is subsequently applied.**”

- 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 ...”.

As far as we know the terms “back-bent occlusion”, “back-bent front” and “back-bent warm front” are describing the same phenomenon. “back-bent front” seems to be the most widely used. We changed the text accordingly.

- L93: not clear to me what you regard as the warm conveyor belt here.

In this context, we refer to the warm conveyor belt in the sense of Browning (1973, 1985), i.e., the warm and moist airstream ahead of the cold front in extratropical cyclones. When applying our front detection, we occasionally detect humidity gradients on both flanks of this airstream. The gradients on the warm-side boundary are typically not associated with precipitation and are therefore not considered synoptic fronts for the purposes of our study. To address this, we apply a post-processing filter to remove these cases as much as possible.

- 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.

As noted in our response to the reviewer, our initial definition followed Catto & Pfahl (2013), where “extreme” refers to 6-hourly precipitation above the 99th percentile. However, we agree that “heavy precipitation” is a more appropriate description for 50-day return period events in our context. We have therefore replaced “extreme precipitation” with “heavy precipitation” throughout the manuscript, including in the title: “Resolution Dependence and Biases in Cold and Warm Frontal **Heavy** Precipitation over Europe in CMIP6 and EURO-CORDEX Models”.

- L107: this reads complicated, can you explain this better such that others could reproduce your approach?

We rewrote this paragraph and added an example to make it easier to understand: “Following Henin et al. (2019) precipitation is **split between cold and warm frontal when it falls within the 300 km radius** of both front types. In such cases, precipitation is partitioned **proportionally** to the number of grid points associated with each front **type (e.g., if 6 cold and 4 warm frontal points are within the radius, 60 % of the precipitation amount is classified as cold and 40 % as warm frontal).**”

- L113: not clear what “standardized” means here.

We added “...standardized precipitation values (**normalized by its mean and standard deviation**), ...”, to make our method clearer. We used standardized precipitation to minimize the effect of e.g. orography or coastlines, as described in our previous study.

- 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.

We added the exact definition of the regions: “The regions used to evaluate these fronts **are defined as: Northwestern Europe (NWEUR, 48°N-61°N, 12°E-3°W), Southwestern Europe (SWEUR, 36°N-48°N, 11°E-4°W), and Central Europe (CEUR, 48°N-58°N, 3°W-25°W). These regions are selected due to their high front frequency and fraction of heavy frontal precipitation.**”

- 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.

We fully agree and changed the layout of all cross-section Figures accordingly. The panels still could be larger, but to do that we would have to split the two Figures into four, which we believe would clutter the manuscript.

- L224: please give some indication how these extreme fronts have been selected.

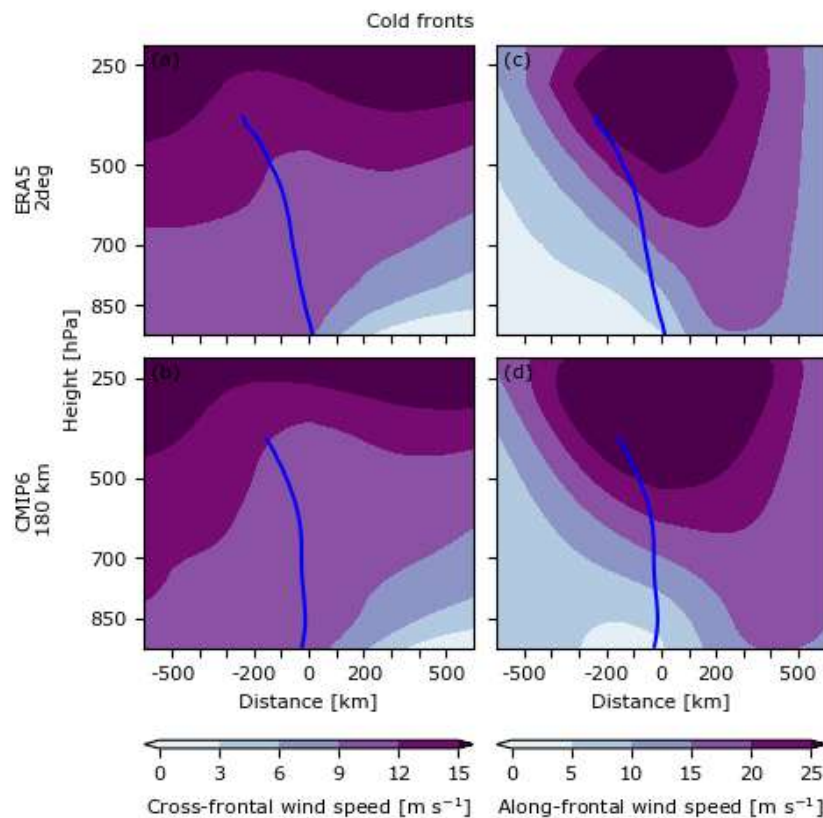
With this sentence we wanted to highlight that the composite and the heavy precipitation analysis are two independent analyses. The composite method is described in Section 3.3. We made this clearer by changing the citation to a reference of the section. We further changed the term “extreme fronts” to “strong fronts” to be more in line with the adapted definition of “heavy” precipitation: “For this purpose, we analyze cross-sections of composites **of strong** cold (Fig. 7) and warm fronts (Fig. 8). Note that these composites are based on fronts selected independently from those associated with the heavy precipitation discussed in the previous section, but by following the method described in **Section 3.3.**”

- 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?

We do not believe that the mesoscale vorticity differences are primarily driven by vertical-resolution effects. CMIP6 data have higher vertical resolution than the ERA5 fields we use here, and one CORDEX model with relatively few vertical levels still reproduces the sloping vorticity structure more closely to ERA5 than the coarser CMIP6 simulations.

Instead, the contrasting vorticity patterns are most likely linked to differences in the representation and position of the low-level jet. In the CMIP6 simulations, the low-level jet on average tends to be displaced farther ahead of the cold front (~ 450 km ahead of the cold front in CMIP6 vs ~ 300 km in ERA5), which shifts the upper- and lower-level vorticity maxima apart and produces a dipole-like vertical structure. In contrast, ERA5 composites show a smoother vertical transition because the low- and upper-level jets, and thus their associated vorticity maxima, are more closely aligned. The ERA5 data is the output of the high-resolution IFS model and therefore captures the low-level jet location more accurately than the CMIP6 models. Remapping ERA5 to coarser resolutions weakens the low-level jet but does not substantially change its mean position. We added the following to the text: “Mesoscale circulations, however, show some biases. **Vorticity in cold fronts (Fig. 7i – p) exhibits the biggest differences, with all CMIP6 sub-ensembles showing a split between the upper- and lower-level positive vorticity regions. In contrast, ERA5 has a continuous backwards-sloped area of high vorticity. This is due to the low-level jet position, which in CMIP6 on average is located further ahead of the cold front than in ERA5.**”

To further illustrate this, we here provide cold front composites of the cross- and along-frontal wind speed for ERA5 2° and CMIP6 180 km:



- L254: I find this concluding statement a bit too positive, given the results shown in Fig. 7.

This sentence refers to the large-scale fields (Fig. A8–A11 / Fig. S7–S10), not the mesoscale circulation. To avoid confusion, we clarified the wording as follows: “The composite analysis of frontal cross-sections reveals that the **large-scale fields and synoptic circulation** are well captured.” With the additional figure references in the results section (see reply to the next comment), we believe the revised text now reads more intuitively.

- 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.

The content of the figures was already described in the text, but we had not included explicit references. We now added: “At the **large scale**, the composite fields of temperature, humidity (**Fig. S7–S8**), and circulation (**Fig. S9–S10**) show good agreement between ERA5 and all model ensembles.”

We believe these plots are valuable in the Supplementary Information, as they provide additional insight into the thermodynamic conditions and large-scale frontal circulation. As noted, there are no substantial differences between the reanalysis and the simulations, demonstrating that the large-scale environment of frontal systems is well represented even in coarse-resolution GCMs. Since the focus of our study is on the differences, we do not discuss the general frontal structure in detail here. A more detailed discussion of the cold frontal structure can be found in Schaffer et al. (2024). In response to a reviewer suggestion, we also expanded the description of the synoptic and mesoscale decomposition in Section 3.3: “**To analyze composites of front-relative circulation, the dynamic variables are separated into synoptic and mesoscale components using a spectral filter, with wavelengths longer than 1000 km representing the synoptic scale and shorter wavelengths the mesoscale.**” Finally, we further added the approximate location of the frontal surfaces to all cross-section figures.

We believe that our revisions enhance the manuscript’s clarity and provide readers with a clearer insight into our study.

Best regards,
Armin Schaffer