

Future Projections of Extreme Wind and Precipitation Associated With Extratropical Cyclones Over North America Using Regional Climate Simulations

by De Meyer et al.

General comments:

Using a set of regional climate model experiments, this manuscript quantifies uncertainties and projected future changes in two hazards associated with extratropical cyclones: extreme precipitation and wind. The authors propose a decomposition of the biases and changes in terms of intensity, threshold selection, and occurrence. Overall, a clearer picture emerges for precipitation, for which the biases appear to be primarily driven by intensity, whereas the projected future increases appear to result from changes in both intensity and occurrence. The manuscript is clearly written and easy to follow, and it contains an appropriate number of figures in the main text, complemented by additional supporting figures in the Supplement. I believe the manuscript is of interest to the weather and climate community and is suitable for publication in *Weather and Climate Dynamics* after the authors address the minor revisions outlined below.

Specific comments:

Line 74: You motivate the use of RCMs partly based on the relatively coarse resolution of global climate models. Could you briefly discuss how this motivation relates to the emergence of global storm-resolving or kilometre-scale models, for example those developed within nextGEMS project? Although these simulations may still have limitations in terms of ensemble size and simulation length, they seem relevant in this context.

See for example: Segura, H., Pedruzo-Bagazgoitia, X., Weiss, P., Müller, S. K., Rackow, T., Lee, J., ... & Stevens, B. (2025). nextGEMS: entering the era of kilometer-scale Earth system modeling. *Geoscientific Model Development*, 18 (20), 7735-7761.

Line 134: I think it would be useful to include a figure illustrating the model domain and the differences in orography among the RCM, the driving AOGCMs, and ERA5. This could also help interpret some of the regional differences discussed later in the manuscript.

Line 153: Please explain why these four CMIP6 models were selected.

Line 156: The other model simulations extend to 2100, whereas the MIROC6 simulation ends in 2098. Why does this simulation end earlier?

Line 156: How was the September–December spin-up period selected? Please clarify why this spin-up duration is considered sufficient for the variables analysed here.

Line 158: Please explain the rationale for selecting the SSP3-7.0 scenario.

Lines 160–161: Is there a reference or additional analysis that supports this statement?

Line 166: Please spell out UQAM at first use.

Line 173: The criterion that cyclones must “be located more than 5° from the equator” seems rather permissive for identifying extratropical cyclones. Could you explain the rationale for this threshold and whether additional criteria are used to distinguish extratropical from tropical systems?

Line 176: How are cyclone-track merging and splitting events handled by the tracking algorithm?

Line 177: How is the influence of orography handled in the cyclone detection? Mean sea-level pressure fields over regions of high topography can contain artificial or otherwise problematic pressure minima. Do you apply an elevation threshold, mask, or another criterion to avoid identifying such features as cyclones?

Line 182: “Tropical cyclones form an integral part of the analysis,...” I am not fully clear on the rationale for retaining tropical cyclones in an analysis focused on extratropical cyclones. Could you explain this choice? It would also be useful to clarify how tropical cyclones and systems undergoing extratropical transition are treated.

Lines 229–230: How is seasonality accounted for in this analysis? Given the strong seasonal cycle in both extratropical cyclone activity and the associated wind and precipitation extremes, could seasonality influence the results presented here?

Line 292: An overestimation by CRCM6 driven by MIROC6 during winter in the eastern part of the domain appears quite clear in the figure.

Figure 1: Could some indication of robustness or statistical significance be provided? For example, this might be based on ERA5 interannual variability, uncertainty across the simulations, or agreement among the different model experiments. A similar consideration may also apply to some of the subsequent figures.

Figures 3–10: Their current placement after the references makes the manuscript more difficult to read. I suggest moving these figures closer to the corresponding discussion in the main text.

Line 313: It would also be useful to express these differences as percentages, i.e. as relative biases with respect to ERA5, where appropriate. This could make it easier to assess the magnitude of the biases across regions with different climatological values.

Lines 315–316: “Despite relatively strong intrinsic intensity biases in the tropics (Fig. S11a), ...” This sentence is somewhat difficult to interpret and could be rephrased for clarity.

Line 543: How is the ocean represented in the RCM simulations? For example, are SST and sea-ice conditions prescribed from the driving AOGCMs, or is some form of coupled ocean representation used? Please clarify this aspect of the experimental configuration.

Technical corrections:

Line 133: MSLP is not italicized here, whereas the other acronyms in the manuscript appear to be in italics. Please check for consistency.

Line 150: Missing reference (?).