

General response to reviewers:

We are very thankful for the constructive and thorough review. You all clearly took meaningful time to help improve the presentation and robustness of this work, and I hope our attempt to incorporate your comments shows that we value it. There are a couple of broad changes to mention, but please see individual responses to each comment below. Our responses are in blue.

General changes:

1. Methodological details have been removed from the Results and Discussion section and moved into the Methods section (Section 2).
2. We have attempted to improve the writing of the Results and Discussion section as each reviewer brought up that this section was a bit messy.
3. Figures 5, 7, 9, and 10 have undergone more substantial changes to either clarify the interpretation of the figure or provide more quantitative information.
4. More quantitative information and statistical tests have been added for much of the analysis (e.g. Figure 7, 8, 9, 10, 12, 13).
5. Many supplemental figures have been added as we want to keep the main manuscript structured to focus on the most essential points for brevity.

Reviewer 1

Thank you Dr. Solman for your constructive review of this work. In particular, the suggested inclusion of more reanalysis-based reference data is useful context for the reader, and we think the manuscript has benefitted a lot from the other suggestions you've made. Please see line-by-line comments below and thank you for your time!

The manuscript assesses the improvements in the most updated WRF evaluation simulation for the CORDEX-NA domain compared with a previous generation simulation. It also compares the simulated results against a convection-permitting simulation available for North America. The analysis is valuable and needed, as it represents the basis for identifying strengths and limitations of the most updated WRF simulation for the NA-CORDEX domain. The manuscript is very well written and conveys a clear message. However, in some passages the narrative seems to be somewhat disorganized and disconnected. This is particularly evident when analyzing snow features and extreme precipitation. I would recommend separating the case study analysis from the climatological assessment given the different approach followed.

There is a number of general and detailed comments, listed below, that should be considered to improve the overall readability and quality of the manuscript, before publication.

General comments

- The overall objectives of the manuscript are too vague. It is mentioned that the authors pursue assessing improvements from the corresponding previous-generation

simulation. On what? What phenomena or climate feature are being assessed? I recommend clarifying on what features the analysis is focused.

Thank you for this comment, the objectives have been clarified by directly calling out which phenomena we focused on. Please see lines 45-50 in the revised manuscript.

- I recommend restructuring the analysis and differentiate the climatological analysis from the case studies analysis. The content starting in page 8 line 240 should be moved to a new section dedicated to case studies.

We agree that this section needed some better organization. We have created subsections under the Section 4 “Evaluation of extremes”, titled “Extreme precipitation”, “Tropical cyclones”, and “Case study of a mesoscale convective system”. After moving many of the methodological content from these sections into the actual Methods section, we hope that this all flows better.

- The discussion on the representation of snow seems to be written in a very different style as compared with previous sections. Please harmonize the writing style if possible.

This section has been revised, and an additional Figure has been moved from the supplement into the main manuscript to better quantify the snow biases. Please see the new section 3.3 and the new Figure 10 which is now properly referenced in the paper.

- Revise the discussion around Figure 10. There is a Figure 10 in the manuscript but it does not contain what is discussed in the paragraph referring to it. Moreover, one figure seems to be lacking (snow features).

Thank you for calling this out, we had a Figure reference error in the original manuscript. This has been corrected!

- The analysis focused on on case studies (TC and MCS) should include some reference to compare with. ERA5 may be too coarse but at least may provide some overall idea of the features evaluated. Particularly for the MCS, since the discussion is around the synoptic environment features, the comparison against ERA5 should be presented.

Thank you for noticing this, we think including the comparisons with ERA5 is useful for demonstrating the benefits and fidelity of these simulations. We have included ERA5 and ERA-Interim visual comparisons for these events in the supplement (Figures S13 and Figure S15) and the text now includes more quantitative comparisons. Note lines 399-409 and 447-455.

Detailed comments

- Page 2 lines 35-45: Are the physical parameterizations mentioned the actual selection used for the NA-CORDEX WRF simulation being assessed? This is not clear and needs to be clarified. As it is written, it seems that the only improvements in the WRF model are related with the physics schemes mentioned in the paragraph (which is certainly not the case). I recommend to clarify that the schemes highlighted are those used in the new simulation.

This has been clarified.

- Page 2 line 44: Who are you referring to as “our contribution” to NA-CORDEX? Who are “our”? Please clarify.

The author list indicates who is contributing these simulations. We have elected to keep this sentence as is.

- Page 3 line 57: Add CORDEX domain after North America.

Done.

- Page 3 lines 70-73: It would be recommendable to consider that the sensitivity tests for identifying the optimal physics selection may not end up in the same choice if the tests were done with different combinations of physical parameterizations, given the strong interdependencies among physical processes and how these interdependencies are treated by different combinations of parameterizations.

We agree that our methods resulted in some untested combinations of physical parameterizations that could have performed differently or even better than our final configuration. We have inserted a statement to acknowledge that this testing was imperfect, but efficient and reproducible. Please see lines 80-94.

- Page 3 lines 75-79: The simulated period should be clearly indicated in this paragraph.

Added.

- Page 3 lines 75-79: The simulated period should be clearly indicated in this paragraph.

Done.

- Figure 1 is not referenced in the text.

Good catch, thank you! This has been fixed and the figure is now referenced when we describe the domain, the land use categories, and the lake model.

- Page 5 – lines 142-143: Should “from July 1 through August 31” be “from June 1 through August 31”, to be consistent with Fig. 5 legend?

Correct, this has been fixed.

- Page 6 – line 157: NAM region has not been defined. Please clarify.

We have added the lat/lon bounds for the NAM region and the northern and southern sub-domains used in this study to the text. They are also present in the figure caption.

- Page 6 – line 163: Are you referring to Figure 5d for IMERGE? If this is the case, replace Fig. 5d-g by Fig. 5d and refer to Fig. 5e-g when referring to the WRF simulations.

We have adjusted the reference for this statement on line 270.

- Page 6 – lines 163-164: I don’t see IMERGE rainfall rate being systematically smaller than WRF in Figure 5d. Can you identify where this feature is apparent?

Thank you for noticing this inconsistency. We had an incorrect reference to Figure 5d that has been corrected. Note the smaller magnitudes in the diurnal precipitation magnitude that are most visible over the Southeast United States and the Northern Sierra Madre mountain range in Mexico, and the provided reference (Hosseini-Moghari and Tang, 2022) systematically evaluates IMERG and comes to this conclusion.

- Page 7 - lines 173-178: Revise. It seems that some parentheses are lacking.

Fixed.

- Figure 7: Revise figure legend and panel titles.

Done.

- Page 8 - lines 201-205: This paragraph seems to be disconnected from the narrative. I recommend inverting the order of this and the following paragraph to ease readiness.

We have reorganized this section to provide a more direct description of the results with some discussion included in the paragraph of this subsection. Please see the new section 3.3.2.

- Page 8 – lines 209-210: The information on how Figure 9 is organized is already in the figure legend. It is not necessary to repeat it in the text. Please remove this line.

Done.

- Page 8 - lines 214-215: Why is the seasonality of the snow cycle not possible to be evaluated against the University of Arizona SWE data? Please clarify.

Seasonal results were provided per basin as a supplemental figure. We recognize that the corresponding supplemental figure was incorrectly referenced so that it was not clear, and have since moved this figure into the main manuscript for better viewing. Please see Figure 10 which includes seasonality of SWE compared to SNOTEL sites for a more robust, station-based comparison.

- Page 8: there is no analysis based on Figure 10, but what is the expected result. Figure 10 only displays the results from the simulations with no observation. Please provide an analysis of that figure, otherwise, it should be removed.

Addressed below.

- Page 8 – line 216: Please revise the reference to the Figure. Figure ??a is indicated.

Addressed below.

- Page 8 –lines 216-223: Revise this paragraph. No figure is displayed related with what is discussed here. There is a reference to Figure 10b/10c-e but Figure 10 only shows the histogram of monthly precipitation. Check carefully the consistency between the text and the figures before submitting the manuscript, otherwise it is impossible to provide a comment on the analysis.

I apologize for this figure reference problem. The supplemental figure looking at the seasonality of SWE compared to the SNOTEL station data is not properly referenced in the manuscript.

- Page 8: last paragraph: Figure 10 is referenced but referring to a different content. Please revise the list of figures included in the manuscript.

Same as above.

- Page 8 – Page 9 - lines 225-239: The analysis of extreme precipitation lacks a more in-depth discussion. It is written in a very disorganized way. Please re write.

We have changed this figure in response to another reviewer comment and have also rewritten the section. Please see the new Figure 11 and the associated text. This now includes a more nuanced view at extreme precipitation rates at 1-hourly, 6-hourly, and daily time scales. We have also provided percentile estimates of precipitation to better quantify the difference between the WRF simulations (Figure 11d-f)

- Page 9 – lines 236-237: Where is the observed histogram displayed to state that the C404 dataset closely matches station-based observational estimates? The observational-based results should be included in this figure. Otherwise, make it clear that you are just referring to referenced literature.

Please see Figure 8 in Rasmussen et al. (2023) for the comparison of C404 precipitation extremes with station based data. We have rewritten this sentence to be clearer, and have also added a recent reference from Schumacher et al. (2026) which further evaluates extreme precipitation in C404.

“Rasmussen et al. (2023) showed that the C404 dataset simulates extreme precipitation rates that closely match station-based observational estimates, rendering this simulation a quality benchmark for our precipitation comparisons (Schumacher et al. 2026).

- Page 9 – lines 247-255: To what extent reanalysis data (ERA5) can be used to compare against simulations? The discussion focus on highlighting that the C404 results are comparable with NAC6 results, but are these realistic? Consider including ERA5 reanalysis results just to have a reference to compare with.

We have included the ERA5 and ERA-Interim representation of the TC and the MCS case study. Please see the supplemental Figures S13 and S15 as well as the associated text and quantitative comparisons of the reanalysis-based case studies and their WRF simulation counterparts (Lines 399-409 and 447-455 are good examples).

- Figure S4 is not referenced in the text.

Fixed!