Dear Editor,

We would like to thank you for your comments/suggestions and the reviewers' comments you forwarded on our manuscript "Spatial spin-up of precipitation in limited-area convection-permitting simulations over North America using the CRCM6/GEM5.0 model".

Detailed responses to all comments are given below, with the reviewer's comment in black and our response in ***Bold-Italic***.

The Authors

Answer to Editor's comments

Dear authors,

in my role as Executive editor of GMD, I would like to bring to your attention our Editorial version 1.2:

https://www.geosci-model-dev.net/12/2215/2019/

This highlights some requirements of papers published in GMD, which is also available on the GMD website in the 'Manuscript Types' section:

http://www.geoscientific-model-development.net/submission/manuscript_types.html

In particular, please note that for your paper, the following requirements have not been met in the Discussions paper:

- "The main paper must give the model name and version number (or other unique identifier) in the title."
- "If the model development relates to a single model then the model name and the version number must be included in the title of the paper. If the main intention of an article is to make a general (i.e. model independent) statement about the usefulness of a new development, but the usefulness is shown with the help of one specific model,the model name and version number must be stated in the title. The title could have a form such as, "Title outlining amazing generic advance: a case study with Model XXX (version Y)".''

We have changed the title to "Spatial spin-up of precipitation in limited-area convection-permitting simulations over North America using the CRCM6/GEM5.0 model" to reflect the use of a single model.

- "Code must be published on a persistent public archive with a unique identifier for the exact model version described in the paper or uploaded to the supplement, unless this is impossible for reasons beyond the control of authors. All papers must include a section, at the end of the paper, entitled "Code availability". Here, either instructions for obtaining the code, or the reasons why the code is not available should be clearly stated. It is preferred for the code to be uploaded as a supplement or to be made available at a data repository with an associated DOI (digital object identifier) for the exact model version described in the paper. Alternatively, for established models, there may be an existing means of accessing the code through a particular system. In this case, there must exist a means of permanently accessing the precise model version described in the paper. In some cases, authors may prefer to put models on their own website, or to act as a point of contact for obtaining the code. Given the impermanence of websites and email addresses, this is not encouraged, and authors should consider improving the availability with a more permanent arrangement. Making code available through personal websites or via

email contact to the authors is not sufficient. After the paper is accepted the model archive should be updated to include a link to the GMD paper."

Thank you for your feedback. We have now incorporated the source code of the model used for our experiments into a version control system on Git, with the following DOI: https://doi.org/10.5281/zenodo.10372926. We have included it the in the section "Data and code availability".

As your study is based on simulation of the CRCM6/GEM5 model please add something like "a case study based on CRCM6/GME5 model results" to the title of your article. Additionally, please add the information how to access the (exact version) of the model code in the code and data availability section.

Yours,

Astrid Kerkweg

See responses above.

Answer to reviewer 1

I want to add some suggestions to your work that I think it's generally relevant for CPM modelers in order to have more efficient dynamical downscaling strategies, since the spatial spin-up issue is often handled with generic suggestion and not very in-depth analysis.

We would like to thank the reviewer for taking the time of reading and improving our manuscript.

- The definition of spatial spin-up is often directly managed by individual models, where it is possible to set some parameters for relaxation layer. For this reason I believe that the model description section should be expanded with a particular focus on this settings.

Thanks for pointing this out. We have now described further the formulation we have used in to relax the prescribed driving data towards the limited-area domain. It can now be read in the text: "All our GEM simulations use a Newtonian relaxation scheme of 10 grid points to constrain all GEM-simulated prognostic variables v in the neighborhood of the boundaries toward the externally prescribed field $\bar{v}$ (Davies (1976). Essentially, in the relaxation zone (sometimes denoted as sponge zone), a term of the form K(v- $\bar{v}$) is added to the prognostic equations. In our formulation, the weights K follow a cosine-squared profile which decreases from a value of 1 in the outside to a value of zero in the inside of the relaxation zone. "

- Add additional multimodel CPM initiatives, e.g. CORDEX Flagship pilot studies on convection (Coppola et al. 2019)

We have now referred to CORDEX FPS project in the introduction and cited additional initiatives: "In recent years, several multimodel CPM initiatives have been implemented in the context of the Coordinated Regional Climate Downscaling Experiment (CORDEX) Flagship Pilot Studies (Ban et al., 2021; Coppola et al., 2020; Mooney et al., 2022)."

- I appreciate the section "Implications of the spatial spin-up for computing resources" because it's fundamental in the Convection permitting experiment and in particular the information on the data size. The more demanding storage requirements is related to the production of 3D boundary data.

The reviewer is right about 3D data dominating the storage demand. We have added a sentence reflecting this "Furthermore, the third column in Table 1 indicates the disk space required for storing the driving fields, which could also pose a constraint and escalates significantly with the number of 3D prognostic variables."

- It’s relevant to integrate this evaluation on other variables that’s important especially in the configuration of the climate experiment. So, this method is fundamental to assess a good quality of analysis but it needs a more robust integration and consideration in the definition of nesting strategies.

We agree with the reviewer. In the Conclusion section, we have now outlined and restructured the main limitations of our study, indicating directions for future research.

Answer to reviewer 2

- General comments

Thank you for providing an interesting and thought-provoking article on the spatial spin-up of precipitation in convection permitting model domains. Overall, I think that the article is of a high-quality and the experimental design is efficiently described with results of relevance to the wider CPM community discussed. I find the discussion of relative computational costs of different CPM configurations to be a useful addition to the article and thank the authors for including it. I have a few comments and suggestions for possible extensions below – some of these may be appropriate for future work considerations rather than in the current article.

We would like to thank the reviewer for taking the time of reading and improving our manuscript.

- Specific comments

Have the authors considered how the spatial spin up of precipitation will vary according to the region a CPM is located e.g. tropical vs mid-latitude. As the authors state, the SSUD will be dependent on inflow so possibly larger in mid-latitude CPM domains but then perhaps the change from predominantly frontal rainfall in the mid-latitudes to deep convection in the tropics is also important? Perhaps it takes a larger number of grid points to spin up realistic deep convective structures in the tropics (particularly when nesting a CPM inside a coarse resolution GPM that parameterises convection). Maybe the authors could add a comment or sentence in the conclusions on whether they think their results are applicable across all CPM domains?

We thank the reviewer for pointing this out. Results from this and previous studies (Leduc and Laprise, 2009; Matte et al., 2017) suggest that the spatial spinup will be greater in situations where there is a strong inflow at the boundaries and, in mid-latitude domains, these seem to be less prominent during summer when convective processes play a greater role. Having said that, a comprehensive response would entail the application of this methodology to simulations performed over tropical domains. We have added a sentence reflecting this need in the Conclusion section: "Finally, the estimation of the spinup distance should be made using other CPMs and also over different domains (tropical Vs. mid-latitude) to establish the dependence of our results on the choice of model/domain."

In the model description section I wonder if the authors would consider adding a table outlining the similarities and differences between the GEM12 and GEM2.5 models e.g. detailing differences in horizontal and vertical resolution, the convection parameterisation schemes, the cloud microphyics schemes. I think the inclusion of such a table would allow the authors to reduce the amount of text between lines 120 and 151.

We agree with the reviewer that the inclusion of a table outlining the main similarities/differences between models would help the reader and we have now included such a table.

I wonder if in your conclusions section you would want to make a stronger statement about the minimum number of grid points that should be removed prior to analysis for single nested experiments investigating precipitation e.g. "given the results shown here for mid-latitude RCM experiments using a single-nested technique a minimum of 50 grid points should be removed at the inflow boundary(ies) prior to analysis of the precipitation field". I understand if you don't want to make this point too strongly, but it may be useful to other researchers if a minimum value was outlined that could be adopted if using a single nested approach.

Thanks for this suggestion. We have now added the following sentence in the conclusions: "Given these results, it seems that a minimum of 50 grid points should be removed at the inflow boundaries prior to analysis of the precipitation field for mid-latitude CPM experiments that use a single-nesting approach."

In the final sentence of your conclusion you state that "the determination of SSUD values is based on seasonal mean variable and thus we should expect SSUD values to be larger in some specific situations." – could you provide some example meteorological situations where you'd expect the SSUD to be larger than the seasonal mean?

We have modified the sentence to provide more context to this statement: "…the determination of SSUD values was based on seasonal mean variables and it would be valuable to develop methodologies to assess SSUD values in specific situations (using for example the Big Brother framework). It is probable that, at certain times during a season, meteorological conditions may lead to a pronounced inflow at the boundaries, causing the SSUD to be significantly larger than the value estimated using seasonal mean values."

In Figure 3 – consider the ordering of your panels, I think it might be more logical to the reader if ERA-5 precip is in the top left panel.

The figure has been corrected.

In Figure 4 – would it be more useful to show these panels for the full domain i.e. x axis just becomes e.g. number of grid points from southern border – then the reader can see the relative magnitude of spin up at both northern (western) and southern (eastern) boundaries on a single panel?

Thanks for this suggestion. We tried to implement the use of a single panel but given the large number of grid points (1330x 1060), it is harder to appreciate the spin up distance in both figures 3 and 4 so we have decided to leave the presentation using two panels.

Figures 4 and 5 – consider whether you can differentiate more clearly between the two GEM2.5 configurations shown. Particularly in Fig. 5 as for the solid horizontal lines showing the standard deviation of the relative difference are hard to see which line refers to which model.

Thanks for this suggestion. We have modified the layout of the data by using a shaded colored area around the relative difference RD (see left in the attached document). We believe it is now easier to differentiate among the two simulations.

Could your naming convention of the different simulations be made clearer and more concise for the benefit of the reader? E.g. removal of the GEM12 section of the naming GEM2.5 (GEM12_SU) could become GEM2.5 SU? (as long as it remains clear that the change is to the parent model and not the GEM2.5 simulations)

We agree that the naming convention is somewhat lengthy, and we have now removed "GEM12" from the naming of the driving data. We have updated figure 2 and references to simulations everywhere.

Line 120 – "Two versions of the CRCM6/GEM5 model are used in this study 120 and differ mainly in their horizontal resolution." – I would consider revising this sentence, I understand what you are trying to say but I think the differences between GEM12 and GEM2.5 are larger than this – particularly thinking about the parameterisation of convection. I think the idea of having a table outlining the differences between GEM12 and GEM2.5 (see above) would be helpful.

We agree with the reviewer, and we have updated the sentence to: "Two versions of the CRCM6/GEM5.0 model are used in this study and differ in their horizontal resolution and the choice of some parameterizations (see Table 1)."

- Technical corrections

Line 59 – "The Big Brother and(?) idealized CPM simulations…"

Corrected.

Section 2.3 – could the section header title be more informative – e.g. "Experimental design of simulations"

Corrected.

Line 191 – Consider using “All four GEM2.4 (GEM12_P3xx)” rather than “All 4”

Corrected.

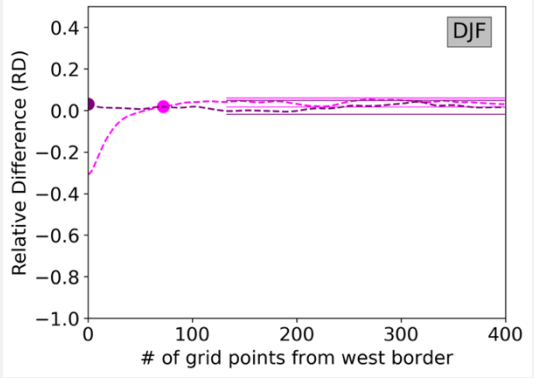
Line 218 – “While GEM2.5 precipitation fields (left panels)” – shouldn’t this read ““While GEM2.5 precipitation fields (top row)”?

In accordance with the previous comment on figure 3, this has been corrected to “bottom row”.

Line 308 – Delete “egligible”

Corrected.

Old figure



New figure