

Review of gmd-2022-202

In this paper the authors test the sensitivity of climate change signals found in simulations of present and future convective rainfall to Lagrangian analysis method and convective cell definition. They test two commonly-used object tracking methods and find that the choice of method makes no significant difference to the resulting climate change signal. The definition of the convective cell tracked by the object-based method, however, is shown to have a significant effect on the changes shown by the analysis. The authors thoroughly analyse these differences in climate change signal and propose that future studies use spectrum-based analyses and consider cell definition carefully.

The paper is admirably clear and well written; the study is thorough and the results highly relevant. The results show the importance of good experimental design and consideration of the limitations of any given analysis technique. The paper is highly polished and scientifically rigorous. I have only a few minor comments that the authors should take into account in a revised manuscript.

Comments

1. Figure 1: Some possibly-spurious black dots appear on the edges of sub-plots a) and b). Units are required for the gdp scale and sea-level pressure.
2. Section 3.1: What spin-up time was used for the simulations?
3. Line 106: The authors should briefly explain their choice of using RCP8.5 as the future pathway in this study.
4. Line 111: Figure 1c includes only specific humidity and temperature, yet the climate change signal mentioned here includes wind and pressure also. The authors should clarify which variables are affected.
5. Figure 2: 'green' in the caption should be 'yellow'.
6. Line 153: While these variables are mentioned in Section 3.2, the fact the authors analyse those variables is only mentioned here. I suggest rephrasing this line.
7. Line 154: The median is calculated yet the change in mean is considered as the climate signal – is this correct, or is the change in median also used as the climate change signal? The authors should clarify.

8. Lines 158-166: When the different thresholds are introduced it would be useful for the authors to include a short description of how they were chosen; for example were they chosen to fit within observed ranges, or around average conditions for the region?
9. Line 166: Absolutely values of which variables or properties?
10. Figures 4 and 5: It seems statistical significance of the signals are shown by using solid or dashed symbols – this should be noted in the Figure captions. This comment goes for all similar plots in the manuscript and supplementary material.
11. Line 226: Should P_{min} here be P_{avg} , since P_{min} was held constant when A_{min} was varied?
12. Lines 240-245: It would be useful to include some analysis of the physical meanings of these results – is the reason that the climate change signal is more affected by T_{min} when A_{min} is small that when larger objects tend to have longer lifespans and therefore increasing A_{min} removes objects that would be excluded by increasing T_{min} ?
13. Figure S6 and discussion: The authors have included a useful analysis of a longer timeseries to see whether their conclusion that the tracking method has no significant effect on climate change signal holds. They comment that the one parameter in which differences are observed is the storm lifespan, the only discretised parameter, and that the discretisation may cause changes to appear large. The authors should note here whether they tested this hypothesis for explaining this difference, by for example looking at the distributions of lifespan values produced by each method instead of only comparing medians.
14. Lines 346-352: The suggestion to use spectrum-based analysis is a good one that follows nicely from this study. However, I presume that the results of spectrum-based analyses also depend somewhat on the definition of the object; for example if the minimum storm size is large then smaller storms will not be included even in the spectrum-based analysis. The authors should comment on this point.
15. Line 368: Related to the previous point; I see the authors' recommendation is to choose thresholds based on the study region and aims of the study. I would also suggest that the thresholds may depend on the model and convective scheme used, and should be chosen to best represent objects in the present climate. One way to do so would be, for example, to compare to radar observations as per Caine et al. (2013) or Raupach et al. (2021).
16. Conclusions: The authors tested tracking algorithms based on advection and object overlap. Do the authors have any comment or hypothesis on the effect of using a pattern-matching algorithm?

References

Caine, S., T. P. Lane, P. T. May, C. Jakob, S. T. Siems, M. J. Manton, and J. Pinto, 2013: Statistical assessment of tropical convection-permitting model simulations using a cell-tracking algorithm. *Monthly Weather Review*, **141** (2), 557 – 581, doi:10.1175/MWR-D-11-00274.1.

Raupach, T. H., A. Martynov, L. Nisi, A. Hering, Y. Barton, and O. Martius, 2021: Object-based analysis of simulated thunderstorms in switzerland: application and validation of automated thunderstorm tracking with simulation data. *Geosci Model Dev*, **14 (10)**, 6495–6514, doi:10.5194/gmd-14-6495-2021.