Review of “Cell tracking of convective rainfall: sensitivity of climate-change signal tracking algorithm and cell definition (Cell-TAO v1.0)” by E. P. Meredith, U. Ulbrich, and H. W. Rust.

Review by Matthew Igel. Review requested on September 28, 2022. Review completed October 10, 2022.

Summary

A large ensemble of convective-allowing simulations driven by observed meteorology over Germany and a pseudo global warming perturbation are used to test the sensitivity of climate change signals to arbitrary thresholds in Lagrangian cloud-object methods. Choices of arbitrary parametric thresholds are shown to impact the magnitude of the climate change signal while methodological choices are not. Finally, some broad conclusions regarding the climate change signal are drawn.

I think the question of whether arbitrary methodological and parametric choices have an impact of our conclusions regarding climate change is an excellent one. The simulations used are a reasonable test of this, and the analysis framework is clear and logical. I have concerns about the generality of their simulations to represent *magnitudes* of changes due to climate warming rather than *differences* in changes. I also wonder how sensitive the conclusions are to the native time resolution of the input data.

Major Comments

1. The lack of sensitivity to the tracking method of the climate change signal is encouraging, but I think more needs to be done to solidify this conclusion. At 5-minutely data input and 2.8km spacing, the conceptual difference between the overlap and advection methods are very small. It is likely that any horizontally extensive rain object will “overlap” both before and after “advection”. So, in that way, it is unsurprising that these methods yield similar results. To really test this conclusion, I think some sensitivity tests to input data resolution need to be tested. For example, 10-minutely and 15-minutely data inputs could be tested. By 15-minutely data, one might imagine that the redundancy of the methods might be relaxed and the resulting signals might be more different. Additionally, one could imagine coarsening the input data from 2.8km to 5.6km and 11.2km and then re-running the analysis again.
2. Section 5 feels incongruous with Section 4. As stated in the second paragraph, the results feel too anecdotal (and I would add the microphysics scheme as a probable source of uncertainty) to derive much confidence in the magnitude of the climate change signal. Even focusing on the sign of the changes feels too confident. The experimental setup is an excellent one for showing the sensitivity to arbitrary thresholds, but 2-week simulations cannot be considered representative of climatology.

Minor Comments

L14: I don’t follow this sentence.

Introduction: The introduction is clear and appropriately thorough. It is nicely written.

L100: I had never heard of this technique for creating an ensemble. It seems perfectly reasonable, but why was it chosen for this study? Do you track precipitation only over the overlapping domain? If not, why introduce this complication to data interpretation?

Section 4.2: It might be worth mentioning that even the *sign* of the climate change signal sometimes depends on A_{min}.

L214: this is a nice point.

Figure 7: why use different colorbar minima?

Conclusions: some of these will need to be removed (pending the response to Major Concern 2).