

Review of the revised manuscript “Multi-model high-resolution analysis of Tropical-Like Cyclone Daniel with WRF and ICON: peculiarities and sensitivity to convection schemes” by Serafini et al.

Recommendation: Minor Revisions

I thank the authors for their detailed responses and for the substantial revision of the manuscript. In my view, the authors have addressed most of the major scientific concerns raised in the first review. In particular, they identified and corrected the processing error in the track-error statistics, added a serial-correlation-aware significance assessment, clarified the limitations imposed by unmatched WRF/ICON physics, revised the “gray-zone” terminology, softened several single-case conclusions, specified the IMERG product used, and added useful supplementary analyses for CPS sensitivity, TASM asymmetry/centre-displacement sensitivity, and precipitation differences.

Overall, the manuscript is now considerably improved and is close to being acceptable. However, I recommend minor revisions before final acceptance. My remaining concerns are mainly about internal consistency, and some strong wording.

1. Make sure the observational cross-validation is visible in the manuscript, not only in the response letter

The authors’ response explains that Hérincs (2023) was cross-validated against ERA5 and CERRA, and that the HRV track was compared with reanalysis tracks using the 15 km buffer. This is useful and partly addresses my concern about the observational reference. However, in the track-changes manuscript, the discussion still appears to rely mainly on Hérincs (2023), UKMet analysis, and the HRV track. If the ERA5/CERRA cross-validation remains only in the response letter, please add a concise summary of it to the manuscript itself, or point clearly to the supplementary material where this comparison is shown.

2. Temper operational recommendations and causal language about convection treatment

The revised manuscript now properly acknowledges that Daniel is a single case and that WRF–ICON differences may reflect the full model-physics package, not only convection treatment. Nevertheless, some wording remains too categorical. In particular, the statement that “for operational purposes, these results suggest that ≈ 2 km convection-permitting simulations should not be run as purely explicit” is too strong for a single-case study, especially given the non-significant track ranking and the metric-dependent performance.

I suggest rephrasing this as a Daniel-specific hypothesis, for example: “For Daniel under the present configurations, retaining a shallow-convection parameterization improved some aspects of the simulation, especially for selected precipitation and structure metrics.”.

In conclusion, I consider the revised manuscript substantially improved. The main scientific issues raised in the first review have been addressed satisfactorily, but the final version still requires minor revision to ensure that the conclusions are fully aligned with the new statistical analysis and that the manuscript is consistent.