Reply to the reviewers of the RCS-UKC4 configuration paper

We thank the reviewer for their thorough review of our work and their useful comments, which will improve the manuscript. We give a point-by-point reply to their comments below in blue.

This manuscript presents an updated version of the Regional Coupled Suite for the northwest European shelf (RCS-UKC4), including developments in coupled weather and climate capability, ensemble forecasting, river routing, and high-frequency coupling for meteotsunami representation. The topic is relevant, and the manuscript reflects a substantial amount of technical work.

However, the current version still has important issues regarding language, internal consistency, attribution of results, and figure–text correspondence that should be addressed before publication.

Thank you for raising this point. A further full review of the manuscript has been undertaken to address aspects of consistency and clearer language throughout.

I therefore recommend some moderate revision before the manuscript can be considered for publication.

Major issues

1. The manuscript attributes the lower wave RMSE in UKC4 to "the combination of increased resolution and the wind and wave modulation by tidal currents." While this explanation is physically plausible, the comparison is not sufficiently controlled to support such attribution unambiguously, because the two forecasting systems differ in coupling, resolution and scientific configuration. The manuscript itself later notes that the wind-speed improvement may partly reflect the higher resolution and different configuration of MOGREPS-UK relative to MOGREPS-G. I therefore suggest that the wording be made more cautious, with a clearer distinction between results that are directly demonstrated by the experiments and interpretations that remain plausible but not fully isolated.

Thank you, we replaced it by the following statement and made sure the manuscript didn't include any over statements: "*The improved RMSE in the coupled system is likely to come from a combination of coupling, resolution and scientific configuration differences between the two systems. Nevertheless, this improvement possibly partly comes from the wind and wave modulation by tidal currents, which are particularly strong in the English Channel, Southern North Sea and Irish Sea, where most of the buoys are located.*"

2. The meteotsunami section is interesting, but some mechanism-based conclusions should be softened. The manuscript argues that the event was driven by Proudman resonance because the atmospheric disturbance speed is close to the estimated oceanic long-wave phase speed, and because repeated application of the pressure signal can approximately account for the modeled sea-level amplitude. This is a reasonable interpretation, but the evidence shown remains partly diagnostic and inferential. The wording should distinguish between a strongly supported interpretation and a fully demonstrated mechanism.

Thank you, we softened the wording to: *The alignment of these speeds suggests that the event was driven by Proudman resonance, although idealised experiments would be required to demonstrate this*.

3. Table 1 states that the ocean time step is 90 s or 60 s (with 10 min coupling). However, Section 4.1.2 (Line 454) states that the time step had to be reduced from 100 s to 60 s when switching to 10-minute coupling.

Thank you, the statement in section 4.1.2 was wrong and we have changed it – the ocean timestep was indeed 90s for 1h coupling.

4. The manuscript states that NSE is "the ratio of RMSE to observation variance", which is not correct. It is defined as one minus the ratio of the error variance of the modeled time-series divided by the variance of the observed time-series. You may derive NSE based on RMSE, but the definition should remain unchanged.

Thank you, we double-checked and did use the right NSE definition, but indeed the description in the text is wrong, thank you for pointing it out, we changed it.

Minor issues:

1. Language, typographical and grammatical issues, for example:

Two "for" in the title sounds awkward;

We changed the Km-scale regional coupled system **in** the Northwest European shelf for weather and climate applications: RCS-UKC4

"ensemble forecasts" in the abstract

Done

"biogeochemisty" Line 98;

Done

"This suggest that UKC4…" Line 442;

Done

"10mn" Line 76, "min" is more standard;

Done

"which current operational systems can't currently forecast" sounds awkward.

Changed.

Overall, this reviewer would suggest the authors to carefully read and edit the language to avoid such problems. The manuscript would benefit from a final consistency check in terminology and style.

Thank you for pointing out all these issues, we did a thorough review of the manuscript to improve it.

2. Figure1: the right panel has no labelled longitudes. The tick mark label font is too small (also for other figures).

Thank you, we have changed this and increased font.

3. The main text states that the buoy locations are shown in Figure 10, which is correct. However, the caption of Figures 11,12 refers buoys to Fig. 9.

Changed, thank you.

4. Figure 5, MAM, DJF, JJA and SON, acronyms without explanation.

We defined this in the caption, thank you.

5. Figure 6 panel references in the text do not match the caption ordering. This panel mapping should be corrected to avoid confusion. Meanwhile, please add longitude-latitude ranges for the map figures.

We've made these changes, thank you. This figure was also revised as asked by Reviewer 2.

6. The manuscript notes a strong concentration of HS and SST observations near coasts and North Sea oil rigs, and wind-speed observations are also spatially uneven. This sampling limitation should be emphasized more consistently when drawing broader conclusions about domain-wide model performance.

We added the following statement in Section 4.1.2: *"Fine-tuning the wind/wave coupling in this paper was done on a few case studies and is limited by the sparse and irregular observation coverage. Future work will evaluate more wave properties against both satellite and in-situ wave buoy networks to provide a full assessment of wind and waves over long periods of time."*

7. The statement that SST is "a good proxy" for the heat budget during marine heatwave conditions is plausible but should be framed more carefully.

We changed it to: *Overall, the evaluation of RCS-UKC4 in marine heatwave conditions indicates that use of RAL3.3 for the atmosphere component gives a good total radiative budget when the ocean surface mixed layer is shallow – although no direct measurement of the heat budget was made over the sea, the SST is highly dependent on the quality of surface fluxes in these conditions (Berthou et al., 2024).*

8. Figure 22b, please explain color and dash pattern, either in the figure caption or using legend.

This has been added, thank you.

9. Code availability: "Model code was provided to editor and reviewers at review time". Please clarify what exactly was made available to the editor and reviewers, and whether the material provided is sufficient for independent assessment of the configuration used in this study.

Yes, this was available as a tarball file provided to GMD, we added the following statement: *Model code (UM, JULES, WAVEWATCH III®, NEMO and UKC4 configuration workflow), including code branches were made available to reviewers of this paper.*

10. Line 225 "Thanks to these results, the coupled system contributed, for the first time, in the decision for one more iteration in the development cycle next standard regional atmosphere and land configuration, from RAL3.2 to RAL3.3"

This reviewer does not quite understand the point of this sentence, which seems to be related to some in-house management workflow. Please clarify.

We removed this sentence from the Results section. The sentence in the conclusion is clearer and we kept this one: *The Met Office has now integrated coupled experiments early in the development cycle of its Regional Atmosphere and Land configurations, which adds a constraint of good quality of RAL at the ocean surface (Bush et al., 2024) and good quality of the land for river flow.*