Reply to the reviewers of the RCS-UKC4 configuration paper

Reviewer2:

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.

This manuscript presents an updated version of the Regional Coupled Suite – UK Coupled (RCS-UKC4) which includes numerous updates of the different models and parameterizations used with notably new addition of river model and biochemistry model as well as the ability to do ensemble forecast. The papers aims to evaluate the new coupled system compared to its predecessor and/or observations available through validation of different weather scenario, i.e. hindcast, forecast, specific weather events like storm, marine heatwaves and meteotsunamis. The paper is generally well organized and figures supports the results described in the text and I believe it fits very well the scope of GMD.

Pending some revisions I would consider this manuscript for publication. Overall, I think the authors might want to carefully read the paper as some details or descriptions are not always very clear, to eliminate any remaining inconsistency, typos and special care to figures where labels and captions could be much improved. See more specifics comments below.

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

My main comment here is regarding the Figure 6a of the drag coefficient against wind speed. How does the modeled Cd in Fig 6a compares to actual observation/widely used parameterization i.e. COARE3.6, ECMWF or else? Up to 20 m/s the mean values of Cd are relatively well known (in open ocean conditions). Somehow RAL3 and UKC4 on Fig 6a seem to have unrealistically high values of Cd on average reaching 0.004~0.005 while RAL2 seem to have more acceptable range of values. While Cd can have a relatively large scatter due to wave modulation, generally speaking Cd is usually not greater than 0.003 on average below 20 m/s, which I believe would be consistent also with Gentile et al.'s study cited here. So, higher drag will give you weaker winds here which might helps with biases, but I wonder if this bias correction happens for the wrong reasons ? are there any drag/stress observations in this region or any reasoning that would justify the Cd used in UKC4 or RAL3 over RAL2 ? I think this here needs a bit more clarification.

Thank you for pointing out this mistake – our figure was actually showing the drag coefficient as calculated prognostically by the model, and therefore being on the lowest model level, which is at 2.5m, instead of the 10m drag coefficient. We've corrected the figure shown the neutral drag coefficient at 10m, which indeed following COARE4.0 bulk parameterisation and is capped at 0.003 in the RAL3 configuration, as is shown in Gentile et al. (2022).

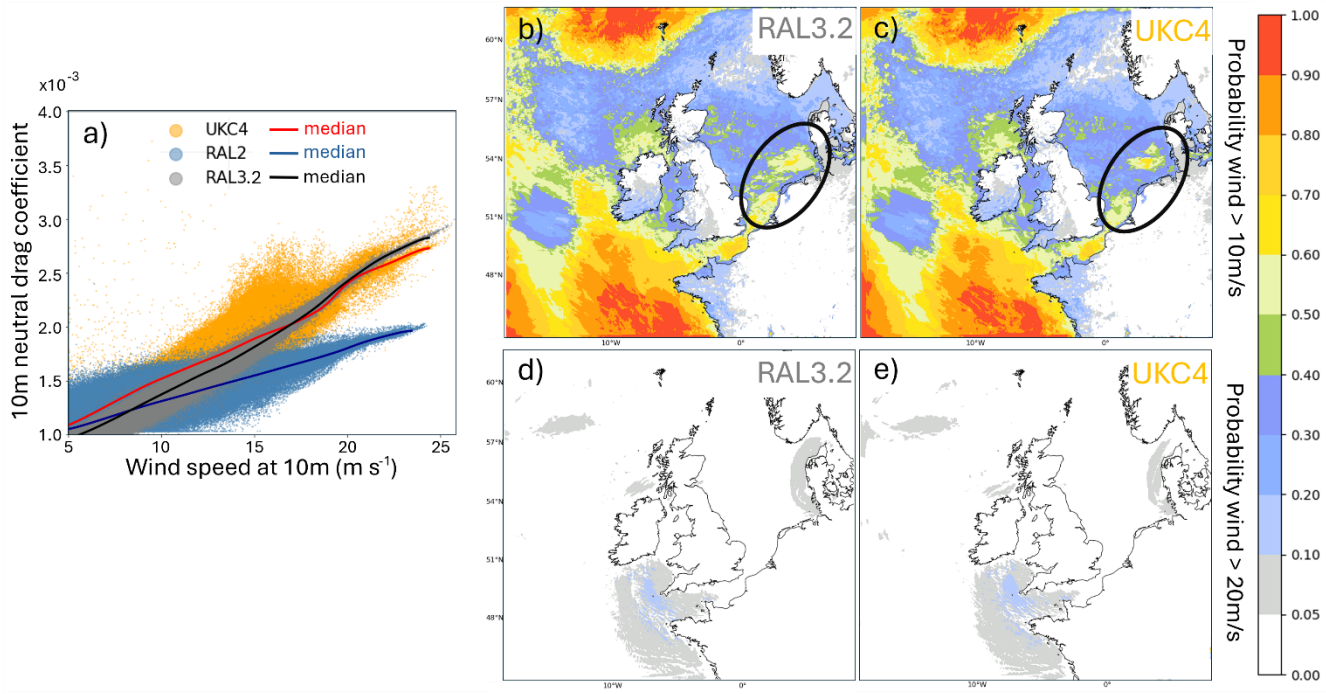

Figure 6: a) shows the 10m neutral drag coefficient as a function of 10m wind speed for RAL3.2 (grey), UKC4 (yellow), and RAL2 (blue) on 10/03/23. Probability of wind speed > 10m/s on 10/03/23 at 01:00UTC during storm Larisa (b, c), >20m/s (d, e) for atmosphere only with RAL3.2 configuration (a, c) and UKC4 (c, e). The black oval indicates the region with the largest differences.

Minor comments:

-Line 160: It would be nice to more clearly associate cases listed in table 2 to each of these evaluation points, while some are easy to see (heatwaves, meteotsunamis) the ones in ensemble forecast are less straightforward to associate with the different bullet points.

Thank you, we added a new table to relate cases and evaluation part:

Evaluation aim	Simulation experiment(s)	Article section
Characterize the impacts of a new atmosphere configuration (Bush et al. 2025) on the coupled ocean	a) 1-month long marine heatwave MO-hindcasts for June 2023 and May 2024 (Berthou *et al.*, 2024) – selected to evaluate the quality of atmospheric forcing. b) 4-year climate hindcast (2000-2003) – to characterise average model biases and potential model drift.	3.1.1 3.1.2
Evaluate the impact of coupling on 10 m wind speed	Winter ensemble forecasts.	3.1.3
Assess the quality of simulated river discharge	4-year climate hindcast (2000-2003).	3.2
Performance of RCS-UKC4 for ensemble wind and wave	Summer and winter 2023 near-real time ensemble forecasts	4.1.1 4.2

forecasting and ensemble spread of 1.5m temperature)		
Assess the sensitivity of results to use of 10 min coupling frequency relative to 1 hour	a) MO-deterministic sensitivity tests of three summer 2023 forecasts, b) additional 3-day MO-hindcast of a meteotsunami event (1 November 2021) and MO-ensemble forecast of the same case	4.1.2 5

Table 1: Summary of RCS-UKC4 evaluation and supporting simulation experiments, details of each set of experiments is given in Table 2.

Also, I found it hard to understand what were exactly the run start and end periods, because there are multiple runs/periods it is a bit confusing, some clarification would help, i.e. in table 2 the 'date' column refers to the actual event date or a run period as the title states "experiments run".

Thank you, indeed this was confusing. We added the start and end dates of all the runs in the table.

-Line 196: "is a foundation SST product (Good et al., 2020), so it should be compared to the model's minimum diurnal temperature" : So does this means the bias mentioned in the text are done this way ?

Yes indeed, we made this clearer in the text and also showed comparison with in-situ observations to evaluate the full SST diurnal cycle in the figure following this one.

-Figure1: Maybe it would be good to indicate names on Fig1b (Celtic Sea, Channel, Southern North Sea and so on) as they are used throughout the paper but for someone unfamiliar with the region and these specific delimitations it could be hard to follow.

This is a good suggestion, thank you, we've added names on Figure 1 and the NWS definition.

-Figure2: 'averaged over the Northwest European shelf', is this the full domain in Fig 1, delimited by the purple line or just inside the orange dotted line ? I don't think this has been clearly stated. Also y-axis says "wakelin_shelf" but it's not clear what it is. Also, would the results of this figure be very different if taken over the different hydrodynamic regions ? Please indicate the line colors in the caption too (OSTIA (black lines) etc...). Why is there no CLIM in the panel (a) ?

Thank you, we added the Northwest European shelf on Fig. 1b and remove any reference to "Wakelin_shelf", changed the legend and added the CLIM line, thank you.

We didn't show other hydrodynamic regions as the spatial maps of Fig. 3 and 4 already cover them.

-Figure 5: The caption could be improved here, (e) is indeed sea surface temperature but (a-d) are SST biases, which is not very well stated. Also, DJF, MAM, JJA and SON acronyms are not mentioned in the caption.

Changed, thank you.

- On the drag coefficient part: Additionally to my main comment, it is not very clear what are the differences in drag here between the three ? RAL2 has a constant Charnock, RAL3 variable Charnock with the wind and UKC4 has a Charnock from the wave model, is that right?

Yes, this is correct, we apologize for the lack of explanation. We've expanded the section describing Fig. 6 and the drag coefficient: *"The drag increases with wind speed faster in WWIII-ST4 than in RAL2, which uses a constant Charnock value (0.011). This difference resulted in a tendency of a wind decrease in coupled simulations, in particular in cases with strong winds Gentile et al., (2022). Gentile et al., (2021) recommended to upgrade the atmosphere-only drag scheme to COARE4.0 with (Donelan, 2018) cap, which was adopted in RAL3, and is closer to the WWIII-ST4 parameterisation: in RAL3, Charnock shows a linear growth with wind speeds, capped at 22 m s^{-1}, which effectively models an empirical weak dependence of the parameter on the wave age."*

-Figure 6: Please revise caption. (a) is the drag coefficient not (e). What are the black circles in (a,b), and longitude/latitude would be nice on the map. Plotting mean lines on 6a would be useful as well. Titles of subplots say RAL3 and UKC4 but caption says RAL3.2 atm-only or coupled, not very clear..

We have corrected the caption.

Figure 9 and further: So far, figures have used (a,b,c,d...) to refer to subplots but from figure 9 and after it got lost. For consistency and better clarity of caption and in-text description I would suggest to keep using (a,b,c,d...) for subplots.

We added these for figures with fewer than four panels.

Specifically for Figure 9, maybe it would be nice to locate the 6 gauges on a map.

Good suggestion, we've added a map.

-Line 357: "five storms": maybe include the names here for clarity.

Added.

-Line 378: "six cases": what are the six cases? The paragraph before mentioned looking at 5 storms, here it only shows 3 storms (which ones ?) and three other cases ? Why ?

We added more information on this and made sure the choice was justified – we chose storm cases, weak westerly cases and a settled case, to represent a variety of wind/wave conditions.

-Figure 13: axis says wind speed but text and caption seems to refer to wave height ? This need to be corrected if needed in both text and figure whether it shows wave height or wind speed...?

The caption is right, we changed the y-label of the figure, thank you.

-Line 455: maybe just need clarification here, it doesn't impact the cost of the coupled system while increasing the model cost ? how are those different ?

We changed the sentence to make it clearer:

"*Switching to 10mn coupling itself had no impact on the cost of the coupled system: exchanging fields more frequently does not increase the cost of the coupled system. However, the ocean timestep had to be reduced from 90s to 60s, because more frequent coupling led to decreased*

ocean model stability, so the overall cost of the coupled system increased because the cost of the ocean component increased."

-Line 456: Just wording here "growth and decay" terms usually apply for Hs I think, not sure what it means for Ws and SST here.

We've corrected this with "*increase and decrease*".

-Figure 15: x-axis only shows '00' ticks, it doesn't make it really easy to read, labels could be better.

Corrected, thank you.

-Figure18: "Wakelin regions (Figure 1b)". Also, why only a subset of the Wakelin regions and not all ?

We chose to focus on the largest regions of the Northwest European shelf for the flux/SST anomalies. We added a justification in the text: "*To understand the drivers of either relatively reduced or increased SST spread due to coupling, we investigate whether the SST anomalies imposed in the atmosphere-only run generate a flux adjustment in the radiative or turbulent heat fluxes, focusing on the largest sub-regions of the Northwest European shelf for visibility purposes (Wakelin_1, Wakelin_2, Wakelin_3, Wakelin_4, Wakelin_10 in Fig. 1b), results are still valid if more regions were included (not shown).*"

-Figure 19: Similarly why not all regions and why not the same as in Figure 18 ?

We chose to focus on the regions of the Northwest European and this time included more mixed hydrodynamical regimes. We added: "*In this figure, we added two additional sub-regions in to investigate the behaviour of regions with different hydrodynamical properties.*"

-Line 558: "simple mixed layer heat budget computed on the averaged mixed layer depth": how do you calculates mixed layer heat budget and mixed layer depth ?

We added this sentence: "*The mixed layer heat budget is calculated using surface flux anomalies integrated over 5 days, divided by the density of sea water, the heat capacity of sea water and the mixed layer depth averaged over 5 days. The mixed layer depth is obtained using (de Boyer Montégut et al., 2004) with a 0.2 °C temperature (density equivalent) gradient and 3 m reference level.*"

-Line 563: "having a systematically steeper slope than the other regions in the SST/flux relationship", is this accurate? it's not particularly obvious to me from the figure.

We corrected the sentence to be clearer and reflect the figure results better: *"This is further supported by Wakelin_9 (Irish Sea) and Wakelin_1 (Southern North Sea) having steeper slopes than the other regions in the SST/flux relationship in summer (Fig. 19a, b). These areas are permanently mixed vertically, with an average 30 m depth, so that even large changes in flux result in small changes in SST in summer. The other areas of the shelf can develop a summer stratification, and therefore show a less steep slope, meaning small changes in fluxes can lead to larger changes in SST."*

-Line 600/601: maybe these locations can be placed on the map Figure 21 as for Portsmouth?

These have been added, thank you.

-Figure 20 : Please indicate the gray line in the caption as well.

Done

-Line 646: “UKC4-1h (dashed lines) clearly misses this pressure drop,” it seems on the plot that the dashed lines are getting the drop in MSLP for every lines no?

We made the sentence clearer to make sure the reader can see that this pressure drop is very short-lived, and not captured by the 1h sampling: **“***The frontal system is seen as a 1-1.5 hPa pressure drop in 30 min at 02:00UTC on the western part of the Channel (blue), arriving at 08:30 just south of Portsmouth (red). This sudden pressure drop only lasts for 1h: 1-1.5hPa decrease for 30 min followed by 1-1.5hPa increase for 30 min. UKC4-1h (dashed lines), although generally capturing the overall pressure tendency, clearly misses this 30 min pressure drop followed by a 30 min pressure increase, as 1h sampling frequency is too low to capture this short-lived pressure signal.”*

-Figure 21/22: longitude/latitude would be nice to have on maps

Added, thank you.