

**Author response to RC1: “Evaluating the impact of atmospheric forcing resolution and air-sea coupling on near-coastal regional ocean prediction” by Huw W. Lewis et al.**

We thank RC1 for their constructive comments, which have led to improvements in the revised manuscript. Their contribution has also been acknowledged in the revised manuscript. A detailed response to the ‘Major comments’ and ‘Minor comments’ are provided further below.

RC1 also highlighted a particular concern, which we address directly, that:

*“...I got a slight impression, that the detailed evaluation of the improved system (UKC3) is separated into a second article to increase the number of publications (just using only the time period in summer and additional a slightly smaller area of interest.... There is also an article about the evaluation of the wave coupling (doi: /10.5194/os-2018-148; did not read this one), which is introduced in the GMD discussion paper....”*

The introduction text has been amended to be clearer about the distinction between the UKC3 description paper in GMD and this manuscript, but we also clarify the situation here.

The UKC3 system description paper has now been accepted for publication in GMD, and aimed to provide a high-level overview of system performance across 4 different times of year and evaluating results across atmosphere, ocean and wave components for the whole North West Shelf domain. The focus of that work is on the impact of coupling, and in particular on the effect of introducing new wave feedbacks within the ocean component in UKC3 (relative to UKC2 capability).

In contrast, this paper aims to take a much closer evaluation of the different atmospheric forcing on ocean results only, and to better define the impact of coupling we have conducted a series of new simulations not discussed at all in the GMD paper – referred to as FOR\_HI, pCPL\_WIN and pCPL\_RAD here. The assessment of the FOR\_HI results relative to FOR\_GL is important to identify the impact of changing atmosphere forcing from global-scale to regional-scale, and therefore enables some measure of the additional impact of representing the feedbacks by then comparing coupled results with FOR\_GL.

This paper focuses on only a small region to better highlight the changes in near-coastal regions, where we could make use of radiation measurements over sea at L4. A summer simulation period is selected as representing the period when atmosphere forcing and coupling changes had most impact. This also coincided with a period of good observational data coverage at L4, where we had access to both atmosphere and ocean observations. Of course, a more expansive discussion looking at a number of different times of year and locations within the domain would be desirable, but not feasible while also providing the kind of detailed evaluation advocated by RC1 as *“This evaluation is important and should be publish”*.

In summary, the current manuscript is fundamentally different to the UKC3 system documentation paper, and no material initially intended for that paper has been *“separated”* into this paper as suggested. We therefore appreciate the editor continuing to consider the submitted paper for the CMEMS Special Issue, revised in light of reviewer comments.

The Ocean Science submission on the impact of wave coupling referenced by RC1, is also briefly cited in this paper, but concerns evaluation of wave impacts in the AMM15 system over a 2-year trial period (2017-2018) based on the operational ocean forecast configuration with/without data

assimilation, and using global-scale ECMWF forcing throughout. Beyond the common domain and use of the NEMO wave coupling configuration, there is very little practical overlap between the themes of the ocean-wave coupling paper and this manuscript.

**Author response to RC1 Major comments:**

1. *I suggest to focus only on changes in model resolution and air-sea coupling here and mainly use the experiment CPL\_AO and not CPL\_AOW for comparison with uncoupled simulations (FOR\_GL and FOR\_HI) as CPL\_AOW also includes the wave coupling. Although the CPL\_AOW shows the best results, it would not be clear if differences arise from the air-sea coupling or the wave model coupling, especially as differences of wave model coupling are only shortly described in the section about near-surface wind speed. In addition, there is the other article submitted to Ocean Science about the wave coupling, which probably discuss this topic in detail.*

We made a deliberate choice to show both CPL\_AO and CPL\_AOW results where possible, and consider the use of wave coupling to be important in general. The summary results (e.g. Fig. 2) show the differences between CPL\_AO and CPL\_AOW to be small at this time of year relative to the influence of including air-sea coupling, or the change in atmospheric forcing. Demonstrating this consistency is considered to be a useful result. The impact of wave coupling is discussed in more depth with regard to wind forcing, as the feedback between the wave model and atmosphere through the Charnock parameter has the potential to improve the wind forcing – here we argue that the SST-wind feedbacks are in fact more important, and that the coupling cannot ‘correct’ for the change in wind characteristics in the regional scale system relative to global.

As noted above, the ‘other article submitted to Ocean Science about wave coupling’ discusses a completely different experimental design over a two-year trial period with a focus on ocean results in the context of the operational CMEMS NWS system. Critically, there is no use of a regional scale atmosphere, or any ocean-atmosphere or wave-atmosphere feedbacks represented in the ‘other article’.

2. *The current article is structured in a way that it describes the different physical properties separately including all model experiments, which makes it difficult to get the connection of all changed processes. Therefore, I would like to suggest to change the structure of the results section of the article as follows and extend the physical analysis.*
  - a. *The evaluation of the newly coupled system and the improvement compared to observations should be located in the GMD paper (is there also the Performance improvement between the UKC3 and UKC2 system shown?). Therefore, only a short overview should be given here (may also include the CPL\_AOW simulation).*
  - b. *Then show how the uncoupled system changes with increasing resolution (FOR\_GL vs. FOR\_HI). Mention how the differences in the general physics influence the results (especially the atmospheric convection). Please describe here possible changes in the physical processes due to the increased resolution and how they influence the others (e.g. how C2 changes in radiation influence, SST, ocean currents, etc). What causes the changes? ...*

- c. *Then compare the high resolution FOR\_HI with the coupled system CPL\_AO. How is the feedback of ocean to atmosphere changing wind speed and direction, and radiative fluxes. How are clouds influenced by changes in winds and how do they change the radiative fluxes. How is the ocean state changed e.g. how are ocean currents changed? Explain the origin of the larger scale patterns occurring in the differences in SST, wind and SW. When looking at these differences by eye, there might be similarities. If yes, what are their origins? Mechanism that shifts clouds leading to changes in radiation in the study area, explaining the differences in the mean fields and e.g. the biases in the SST? ...*

The current manuscript Results section is structured to

- i) Sect. 3.1: provide a brief overview of the SST results, setting the context of the experiments including FOR\_HI and OSTIA results, which are not discussed at all in the GMD paper referenced.
- ii) Sect 3.2: discuss the evaluation of SST across the Celtic Sea sub-region and relative to the L4 observation point in particular, referenced later in the paper.
- iii) Sect 3.3: consider the different heat budget in all experiments.
- iv) Sect 3.4: consider the wind forcing in all experiments.  
Sect 3.5: explore partially coupled sensitivity experiments

We considered the suggestion of RC1 to effectively re-order the discussions of Sect 3.3 and 3.4 in particular to focus on both heat budget and wind forcing changes for a) FOR\_HI vs FOR\_GL, then b) CPL\_AOW vs FOR\_HI. On reflection we have kept the same overall structure however, noting it is helpful to compare all experiments relative to a particular observation or diagnostic (e.g. Fig. 7, 8, 10) within the same broad discussion. To first order, we also find that the heat budget and wind forcing of all regional scale simulations can be distinguished as a group from the FOR\_GL forcing (e.g. Fig. 7, Fig. 10.). We have though revised these sections in light of the comment above, separating out more clearly the discussion of FOR\_HI vs FOR\_GL from CPL\_AOW vs FOR\_HI. In particular, note the revision of new Fig. 5 and new Fig. 6 to make this separation more explicit.

The discussion encouraged by RC1 in bullet c. above on the relationship between SST, wind and SW is better represented in the revised manuscript, including the encouragement of RC2 to consider the change in SST comparing OSTIA (used as a fixed daily boundary condition in FOR\_GL and FOR\_HI atmosphere model simulations) with CPL\_AOW (e.g. Fig. 3f)) and its links to the changes in heat budget (e.g. Fig. 6d)) and wind (Fig. 9f) results.

3. *Please also investigate the changes in physical processes in detail during the other seasons. Are there differences to the summer season?*

As discussed above, we consider the extension of the current paper to results during other seasons to be out of scope, given the emphasis on a period when atmospheric forcing and coupling was considered to have largest effect. We better highlight this choice in the Introduction and Conclusion of the revised manuscript. Were we to add the suggested detailed analysis during other seasons, the manuscript would risk becoming overly long, and lose some of the detail requested by RC1 and RC2 in reviewing the current work.

4. *Please make sure that in the conclusion and discussion, it is clear what the new finding of this study is compared to earlier studies and the GMD discussion paper.*

The contrast to the GMD paper is explained more clearly in the revised Introduction. The first 3 paragraphs of the Conclusion refer to a summary of new findings of this study, and a characterisation of the sensitivity of ocean SST simulation over the NWS to choice of atmospheric forcing. The comparison to the GMD discussion paper is provided in the 4<sup>th</sup> paragraph.

5. *There are some results “not shown”, which should be. For example: - Page 8, Line 10: please show the examination of the cloud field - Page 9, line 16: please show the std - Page 9, line 25: please show the time-series of Qns*

The cloud fields are not shown in interest of brevity, and as the fields are not readily available within the archived global-scale ocean model forcing fields (FOR\_GL).

Other requested plots have now been added to the revised manuscript, within the updated Fig. 8 (std in d)-f)) and updated Fig. 7 (time series of Qns).

6. *I would like to suggest to leave out section 3.4 (Partially coupled sensitivity experiments) as coupling only wind or radiation would lead to inconsistencies in the fluxes between atmosphere and ocean, which in my point of view would make interpretation difficult if not impossible.*

RC1 is correct to highlight the inconsistencies in fluxes between atmosphere and ocean in the partially coupled sensitivity experiments. We highlight this point more clearly in the revised manuscript in light of this comment. However, given the encouragement of RC2 to expand rather than remove this section, we consider there still to be value in the results – indeed they help to illustrate the relative impact of the heat budget and wind forcing in isolation within the system, and enable us to conclude that coupling both wind and radiation leads to improved results although the evaluation of the regional-scale wind field is worse than the global scale atmosphere forcing.

7. *During comparisons sometimes only snapshots of one particular time point is shown. Are these snapshots representative for the 10 day period?*

Revised spatial plots in Fig. 3 (SST differences relative to OSTIA), Fig. 5 (heat budget terms and differences between FOR\_HI and FOR\_GL), Fig. 6 (differences in heat budget between CPL\_AOW and FOR\_HI) and Fig. 9d)-f) are all now consistently presented as 10-day means. This does mask some of the variability in fields such as the heat budget terms, but does provide a more representative illustration of results – indeed demonstrate that the snapshots presented in the original manuscript were generally representative.

**Author response to RC1 Minor comments:**

- *There are references used that are still in preparation, submitted or in review (Tonani, et al. 2018, Bush et al., 2018, Lewis et al., 2018c). I'm not sure if this is allowed in Ocean Science. If they are published as discussion papers it might be possible, but what about the ones in preparation?*

The references list has been amended with relevant doi to reflect the updated status of papers currently in review in Ocean Sciences. The only reference still listed as in preparation is Bush et al., (2018), which we anticipate to be submitted and have a citable doi very shortly and in time to be referenced were the current manuscript accepted for publication.

- *Page 2, line 17-20: Please make two sentences out of it as it is hard to read.*

This sentence has been shortened in the revised manuscript.

- *Page 2, line 21-23: how are the mesoscale ocean processes changed?*

An additional sentence has been added to more fully explain the results of Lebaupin Brossier et al. (2011).

- *Page 2, line 28-29: Put here also other references (e.g. the examples used in the following)*

In accordance with a related comment from RC2, the example citation has been updated and highlighted as offering a review of regional coupled studies in the revised manuscript.

- *Page 3, line 30: In this study: do you mean in the experiments FOR\_GL and FOR\_HI? Please clarify. Do atmosphere and ocean have the same model domain?*

Yes, and this is now clarified in the revised manuscript. The FOR\_HI atmosphere and ocean model have the same model domain (as applied in coupled mode in CPL\_AO and CPL\_AOW simulations). This is also explicitly clarified in Section 2.1.

- *Page 4, line 19: please include a line break after "provided by Bush et al. (2018)."*

This is done in the revised manuscript.

- *Page 5, line 8-9: please provide a short description or a reference to the "double penalty" effects*

This has been added and a reference provided in the revised manuscript.

- *Page 6, line 28-29, "potentially link" Please investigate if it is linked or not?*

RC2 highlighted the difficulty in attributing the scale of SST variability to either the diurnal heating cycle or to tidal variations. In practice, the SST variability at any location will be some combination of these factors. We argue that the SST variability at L4 is mostly driven by the tides, but of course there is some influence of diurnal heating. It is not really possible to be any more definitive than 'potentially linked' in a paper of this scope, nor considered so important for the main conclusions drawn.

- *Page 7, line 8-11: Please also compare with model results in the morning to clarify if it is an artefact. Maybe also check it by using less neighbourhood grid cells for averaging.*

The vertical profile results presented in this study were available as daily mean diagnostics, so it is not easily practical to look at sub-daily patterns. The main aspect of interest from Fig. 4(b) is in contrasting the daily mean profiles from the four model experiments, using the CTD observations from L4 as a reference.

- *Page 7, line 12: Can you give a physical explanation for the improvement?*

We attribute the improvement to representing air-sea interactions within the coupled system, and the impact not only being apparent at the surface. Additional text has been added in the revised manuscript.

- *Page 8, line 20: cloud on 24 July → cloud cover on 24 July*

This has been amended, also in line with the related comment of RC2.

- *Page 8, line 24: This could be related → Is this a hypothesis or is it related to*

This is indeed a hypothesis. The text has been amended to be clearer.

- *Page 9, line 14: with “patches” you mean the small scale spatial variations in the fields, right? If yes, do they origin from physical processes, (e.g. from sea surface roughness) or is it just the increased noise that occur in higher resolution models*

Yes, we intended to say small scale spatial variations in the fields, although the ‘patches’ here refer to the variations where there is relatively reduced radiation. The text has been amended in the revised manuscript to clarify. We do not characterise source of the variations as “just the increased noise” but reflecting some combination of the explicit rather than parameterised convection, scale-dependent physics and different grid sizes in the higher resolution models. As discussed in response to RC2, attributing changes to resolution vs physics is a challenge.

- *Page 9, line 20-24: Please reduce the length of this sentence by separating it into at least two and eliminate “might be expected”*

The original sentence has been removed in light of the response to the comment above, and in line with the comments of RC2 in this regard.

- *Page 9, line 32-33: For clarification, please reformulate the sentence*

The sentence has been reviewed and reformulated as suggested.

- *Page 12, line 4: further still by including → further by including*

The sentence has been updated.

- *Please note if the forecasting system includes data assimilation. In case it does, mention the data that is assimilated in the method section.*

All regional-scale ocean and atmosphere simulations discussed in this paper are free-running without any data assimilation. This has been explicitly mentioned in the updated Section 2.

- *Please mention in the method section, which parameters are used as atmospheric forcing for the uncoupled simulations and which parameters are exchanged between atmosphere and ocean and where the fluxes are computed.*

This has been added in the updated Section 2, in line also with the comment of RC2.

- *In table 2 and table 3 and page 10, line 25: FIX\_GL und FIX\_HI written instead of FOR\_GL and FOR\_HI, please correct.*

Corrected.

- *Please carefully check the reference list. For example, Valcke et al. (2015) is mentioned in the text but is missing in the reference list.*

The reference list has been amended in the updated manuscript, and the missing Valcke et al. reference changed for a more recent reference to OASIS in light of RC2 comment.

- *Figure 1a) please increase the lines in the colorbar to be able to identify the different colors*

Corrected.

- *Figure 1b) what are the black and yellow dots? The Celtic Sea study area is partly located outside the UKV atmospheric grid. Does it impact the model results?*

The dots are indicative of the volume of data from each location during the period of interest, as now clarified in the updated figure caption. The yellow dot indicates the location of the L4 buoy. The caption has also been updated to clarify the 'outside the UKV atmospheric grid' – to highlight only that the inner region of the variable grid atmosphere domain has a regular spaced grid resolution, with stretching outside. We do not consider that this impacts the model results.

- *Figure 3) please use the same blue-red colorbar in a) and b) as used in c)*

In the original manuscript, Fig. 3c) presented model differences while a) and b) were model fields, so it was consistent to have different colorbars. In any case, Fig. 3 has been updated in the revised manuscript and its presentation and consistency of colorbars improved.

- *Figure 4a) and 4b) please increase the limits of the y-axis to include the max. difference in figure a). Also label the x-axis with the days (at the moment always 00.00)*

It was decided to merge some of the content between the original Fig. 3 and Fig. 4 in the revised manuscript given the suggested change in focus to consider spatial model fields relative to OSTIA also.

- *In Labels of Figures 3, and 5-11 need to be larger to be readable in a printed copy*

All Figures in the revised manuscript have been updated in light of this comment and in response to other reviewer comments.