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Interactive comment on "Evaluating the impact of atmospheric forcing resolution and air-sea coupling on near-coastal regional ocean prediction" *by* Huw W. Lewis et al.

Anonymous Referee #2

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General comments:

The paper by Lewis et al. presents an evaluation of the impact of atmospheric forcing resolution (17km vs 1.5km) on a regional ocean forecast system (AMM15). The impact of high-resolution ocean-atmosphere(-waves) coupling is also investigated. The evaluation concerns the sea surface temperature with an important work of comparison to in-situ observations, then considering the heat budget and the wind modifications. The justification for only considering one month in summer is however missing.

Such kind of evaluation is quite often done in the regional climate modelling community

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(e.g. Béranger et al., 2010; Akhtar et al. 2018a,b) and it could be relevant here to put forward the novelty of considering ocean forecasts with a very fine coupled system and the inherent difficulties. The added value of the wave coupling is also not so well highlighted.

My main remarks I briefly list here and detail more below concern:

- the key point in the ocean flux forcing which is the SST inconsistency between the one simulated in the ocean model and the one used at the surface boundary of the atmospheric model. It obviously controls the differences in several heat budget terms and very likely the differences in wind between CPL_xx and FOR(_HI) but it is never mentioned here.
- the robustness of the SST improvement (with the higher resolution forcing and coupling) that appears a little altered by the fact that it seems to be more a spin-up effect, with a reduction of the initial bias during the first days of simulation. In addition, the use of partially coupled sensitivity experiments seems very promising, but their results are too briefly discussed.

I'm finally interrogative about the large impact of the higher resolution which is always highlighted by the authors instead of the impact of the physics (qualified as a smaller impact). But for me this is connected, especially over sea, far offshore. I suggest to clarify or discuss more this point.

Consequently, I suggest some major revisions to improve the paper before accepting its publication.

Specific comments:

** Page 6, lines 2-3: "*This indicates improved SST prediction can be achieved for the*

NWS when applying the high resolution."

In my opinion, this conclusion is too rapidly set. Figure 2 shows mostly the stronger cooling during the first days of simulation (till 5 July) in FOR_HI, CPL_AO and CPL_AOW, correcting more efficiently the initial warm bias. Considering the overestimation of the wind in the higher resolution forcing (coupling), this is a possible ocean response to the initial shock with a larger effect of the vertical mixing. How do the mixed layer's depth and thermal content evolve? If possible I suggest to test new initial conditions, more realistic, such as ocean analysis that are available in the CMEMS catalogue or at least a larger discussion about the relative importance of the forcing compared to the model initialisation.

Between 18 and 24 July, it seems there is a warming in FOR_GL whereas SST is stable in FOR_HI and CPL_xx. How is it explained?

** Page 8, lines 8 to 15: "*(...) The impact of coupling on (...) Q_{LW} is dominated by random changes in the spatial distribution of convection. (...) There is also some evidence that the latent heat flux is increased in those near-coastal regions identified as being cooler in CPL_AOW than FOR_HI, where the coupled simulation SST was in closer agreement with observations in Fig. 3(c).*"

To well consider the differences in the heat budget terms between the coupled runs and FOR_HI, the comparison of the CPL_xx and OSTIA SST field(s) must be shown. I think it explains at first order most of the differences found in the long wave upward radiation, latent and sensible heat fluxes. The differences in the convective cloud location play also, but at a second order.

The last sentence is particularly confusing for me as it mixes information about LH, differences in SST simulated by CPL and FOR_HI and the validity of the CPL SST against observations. But what about the comparison between OSTIA and the in-situ observation in this region? Please revise.

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Figure 6 (i-l): Please, adjust the scales to better show the differences. To be fair, it might be shown as relative differences (in %) instead.

Very likely, the differences in the wind field are also controlled by the differences in SST. See Chelton and Xie (2010) or for example Lebeaupin Brossier et al. 2015 (Fig. 8a), Meroni et al. 2018 (Fig. 6).

** Page 9, lines 20-24: "*This provides some evidence that the differences between the representation of the surface energy budget in the global and regional-scale atmosphere simulations is driven mostly by the change in grid resolution and the change from parameterised to explicitly represented convection, rather than from differences between the underpinning MetUM radiation and cloud parameterisation choices, which might be expected to principally drive differences in the mean conditions rather than the spatial variability*"

Page 10, lines 14-16: "*The contrast between the spatial variability of wind speed between FOR_GL and FOR_HI further supports the assessment in Sect. 3.2 that the change in surface energy budget characteristics between the different sources of forcing were driven more by the change in atmosphere grid resolution than by changes to the underpinning model physics.*"

I can not really capture where the contribution of the high-resolution can be separated from the physical behaviour/parametrisations of MetUM between the FOR_GL and FOR_HI forcings. I mean, far from the coasts, there is no reason for these differences apart the MetUM physics?

In addition, connections between resolution and physics exist. Some physical parametrizations may depend on the grid resolution (and time step).

Please, clarify how you distinguish the relative importance of physics compared to the benefit of a finer grid mesh.

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Other comments:

- p1, lines 14-15: "*Observations... data*". Please, revise the sentence as you do not only consider L4 observations...
- p1, lines 21-22: "*...by global-scale NWP (0.7 K in the model domain) is shown...*"
- p1, line 23: "*...reduced* (*by 0.2K*)."
- p2, lines 28-29: "*A number of studies... (Lewis et al., 2018a)*": revise citation.
- p3, line 7: "*...for one of those periods in July 2014.*" The motivation(s) to dedicate this study to this reduced period must be given here.
- p3, line 30: "*...describing the surface* heat and water *budget...*"
- p3, line 31 "*...NEMO using the 'flux formulation'...*": Where (and how) is computed the wind stress?
- p4, line 8 (and lines 17-18): "*the wave-dependent roughness Charnock parameter of 0.085 is used.*": Could you precise if it is α or z_0? If it is a constant, it is not wave dependent...?
- p4, line 17: "*...assumed zero and a constant value...*"
- p4, line 31: Valcke et al. (2015) is missing in the references list. Moreover, I think the citation for OASIS3-MCT is Craig et al., 2017.
- p4, line 31: "*...all information exchanged...*". A brief list of the exchanged fields could be useful.

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- p5, lines 7-9: I am happy to see here this comment concerning the 'double penalty' effect that is indeed of primary importance when comparing high-resolution modelling results with observations.

- p5, end of section 2.2: I am a little surprise there are only GTS data considered. Some other kinds of data could be available on the CMEMS website, in particular profilers to examine the vertical stratification or satellite data that allows a 2D coverage at the surface. Was it a choice to exclude them? And if yes, why?

- p5, lines 26-27: "*Figure 2 demonstrates that all ocean simulations had the same initial conditions...*": This is not something that must be demonstrated. That must be said before in section 2.

- p6, line27-28: "*On several days (e.g. 20, 21, 23, 26 and 29 July) a tidally-forced heating signal of about 1 K is apparent.*" Well, it is not so apparent it is a tidally-induced heating or if it is a diurnal cycle.

- p6, lines 30-31: "*The SST variability of FOR_GL is in general stronger than observed...*" Where is it shown?

- p7, line 19: "*Surface* heat *budget*..." Please change also everywhere after: 'Energy' can be potential or kinetic. . . 'Heat' is more precise.

- p7, lines 29-30: "*...and from CPL_AO and CPL_AOW coupled systems...*": The flux fields for CPL_AO and CPL_AOW are not shown in Figure 6.

- p8, line 20: ". . . *increased cloud* cover *on 24 July...*"

- p8, lines 26-27: "*The warm surface temperature bias of FOR_GL at L4 appears to result despite rather than because of this difference however.*" Maybe mixing (*i.e.* cooling by entrainment) is also lower in FOR_GL?

- p9, line 8: "*...consistently higher during night time...*". Could you explain more this result?
- p9, lines 18-19: "*...both day and night...*" ?? "*...but typically of order 20-50% lower...*" Where the '50%' comes from?
- p9, line 27: Delete "*the accumulated* ".
- p10, line 17: "*The* atmospheric *forcing*..."
- p10, line 25: "... *than FIX_HI...*" FOR_HI?
- p11, lines 13-15: "*Some evidence of the link between SST and near-surface atmosphere conditions within the coupled system was discussed by Lewis et al. (2018b) in considering the relationship between near-surface stability and wind speed over the ocean.*" How this relates to the sentence before? More details or a summary of Lewis et al. (2018a)'s conclusions about the SST/stability/wind relationship would be useful.

- Tables 2/3: Replace FIX_xx by FOR_xx
- Please revise Figure 1: The colour scale for bathymetry in a is blank. What is the 'UKV' atmosphere grid? What are the small black and red points in b?
- Figure 2: If possible add the OSTIA SST error time-series.
- Figure 6: Please, adjust the colour bars in i, j, k, l.
- Figure 7: Add the colour legend for b (which simulation is the blue line?). Larger plots can also help to distinguish more the time-series in c.
- Figure 12: "*...between 20 and* 30 *July 2014...*"

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