Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-162-RC1, 2019

Ocean Science
Discussions

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 #1

Received and published: 4 February 2019

The study evaluates the impact of air-sea coupling in a North-West Europe shelf region and compares it with uncoupled simulations using high and low-resolution atmospheric forcing. It provides further evaluation of the coupled model system described in the GMD Discuss. paper by Lewis et al. with doi: 10.5194/gmd-2018-245. 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. This impression might be wrong, but it is also supported by the fact that the article reviewed here,

mainly uses the coupled experiment that also includes the wave model for evaluation of air-sea coupling. This evaluation is important and should be publish. However, I leave the decision whether to merge this article with the one submitted to GMD to the editor(s). In case this publication shall be a stand alone one, I suggest major revisions and would like to suggest a more detailed discussion of the changes in the physical processes (some are already there). Detailed comments are shown in the following.

Major comments:

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.

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 extent the physical analysis.

- 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). - 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

changes in radiation influence, SST, ocean currents, etc). What causes the changes? ... - 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? ...

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

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.

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

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.

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

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?

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

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

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

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?

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

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

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

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 neighborhood grid cells for averaging.

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

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

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

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

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

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

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

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

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.

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.

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

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

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

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

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)

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

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-162, 2019.