

Interactive comment on “The UKC3 regional coupled environmental prediction system” by Huw W. Lewis et al.

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General comments:

In the paper titled “The UKC3 regional coupled environmental prediction system” by Lewis et al, the researchers document the third generation of a regional coupled prediction system across sky, sea and land of the United Kingdom and the north-west European continental shelf. They start out by sketching the context in which their developments take place, both nationally and internationally. The system consists of an atmospheric component (tightly integrated with a land component), an ocean component and a wave component. The most important change from previous generation

C1

UKC2 model is the option to pass the significant wave height, stokes drift velocity, mean wave period, reduction factor for atmospheric stress, and the wave-modified surface drag coefficient from the wave component to the ocean component to include the feedback from the waves on the ocean dynamics. The paper tries to follow a delicate balance between discussing these and other changes in detail and skipping over complexity in physics and implementation challenges. This discussion ends in a general discussion of simulation results; the presented results serve two aims: evaluating the effect of the new wave to ocean feedback (mostly a localized effect), and showing the model (stable) behavior over month-long simulations (compared to 5-day simulation for the UKC2 model in a previous paper). The combination of the discussion of technical details and complex simulations, could easily have been extended to a paper twice its current length. As a result the current paper is very interesting, but due to the high density of information and references at times also challenging to follow.

Specific comments:

1. Abstract, page 1, line 19-20: The major update is indicated to be “explicit representation of wave processes in the ocean and their feedbacks through wave-to-ocean coupling”. This suggests that the wave component is new to the UKC3 system, but later in the paper it’s indicated that the UKC2 model already included the waves component (forced by ocean currents and interacting with the atmosphere) and thus that the wave-to-ocean feedback coupling by including wave forces on the ocean are new.
2. Abstract, p1, l25: extended periods. The meaning of word “extended” only becomes clear on page 2, line 7/8 where it’s indicated that periods are extended compared to the analysis of UKC2.
3. Sec 1.2, p3, l26-28. The term “component model” (or “component model technologies”?) is not defined. I’ve interpreted the text as: Developing increased

C2

understanding and system improvements benefit from the application of a diversity of different simulation components and coupling technologies in a range of environments.

4. Sec 1.3, p6, l2. It's confusing that here UKA3 atmosphere and UKL3 land components are distinguished, whereas a couple of lines further in Sec 2, p6, l13 the combined atmosphere-land component is also indicated by UKA3. Throughout the paper UKA3 is used for both the atmosphere component and the combined atmosphere-land component (latter more frequent). Only Sec 2.2 discusses UKL3 as a separate component.
5. Sec 2.3, p12, l10-12. This is an important remark: the land fluxes don't run off into the ocean. So, the atmosphere-land and ocean models are not as fully coupled as may be suggested. This influences long term model stability.
6. Sec 4.2, p17, l26-28. No serious model drift found even without data assimilation. However, since the UKC3 is a local, nested model both the atmosphere and ocean models are significantly forced on these time scales by their boundary conditions. Furthermore, the land run off – a potential source for drift – is not connected to the ocean influx.
7. Sec 5, p25, l21-23. Data assimilation of observations in one part of the system may help to improve the state of coupled components as well, e.g. wave observations may help to improve atmospheric and oceanic state.
8. Sec 4.6, p23, l25-28. Very short section without any discussion about performance whereas in Sec 3, p13, l26-29 statements are included about the poor performance of coupled systems. The earlier remark demands a least a bit more discussion here. Possible effect of coupling frequency?
9. Table 2, p43.

C3

- The Description column sometimes indicates that forcing comes from (external) files, sometimes just that the model is forced. The inconsistency in wording causes uncertainty about interpretation. Are the following additions correct?
 - UKCao: no wave effects included
 - UKCaw: no ocean currents included
 - UKC3owg: global meteorology forcing from files
 - UKA3g: no wave effects
 - UKO3g/UKO3h: no wave forcing
 - UKW3g/h: ... forcing from files
10. Figure 2-15, p54-67, graphics.
 - Text size is small and hard to read (same for Figs 3-15)

Technical corrections:

1. Abstract, p1, l25-27. Long sentence, consider breaking up or add a comma after "one month in duration" on line 25.
2. Abstract, p1, l28-29. The formulation that the results of the coupled model are "at least comparable skill to the equivalent uncoupled control simulations" suggest that the coupled approach does not show major improvements. Consider rephrasing to something like "The coupled approach shows notable improvements in surface temperature, wave state (in near-coastal regions) and wind speed over the sea, whereas the prediction quality of other quantities shows no significant improvement."
3. Sec 1.1, p2, l25. Reference Simpson (1992) not included unless Simpson (1997) is intended.

C4

4. Sec 1.2, p3, l22. Reference Donelan et al. (2018) should read Donelan (2018)
5. Sec 1.2, p4, l7. Reference Skamarock et al. (2008) should read Skamarock and Klemp (2008).
6. Sec 1.2, p4, l26-27. For all the models and coupling techniques mentioned thus far references have been included, but not for NOGAPS atmospheric and NCOM ocean models mentioned here. This is inconsistent.
7. Sec 1.2, p4, l28 30. Reference Seo et al. (2017) should read Seo (2017); the reference Seo et al. (2007) is correct.
8. Sec 1.2, p5, l6-13. There are many other papers about the benefits of coupled wave-ocean models in coastal regions. This includes for instance:
 - Mulligan, R.P., Hay, A.R., Bowen, A.J., 2008. Wave-driven circulation in a coastal bay during the landfall of a hurricane, *Journal of a Geophysical Research: Oceans*, 113:C5, doi:10.1029/2007JC004500.
 - Uchiyama, Y., McWilliams, J.C., Shchepetkin, A.F., 2010. Wave-current interaction in an oceanic circulation model with a vortex-force formalism, *Ocean Modelling*, 34:1-2, 16-35, doi:10.1016/j.ocemod.2010.04.002.
 - Elias, E.P.L., Gelfenbaum, G., and Van der Westhuysen, A.J., 2012. Validation of a coupled wave-flow model in a high-energy setting: The mouth of the Columbia River, *Journal of Geophysical Research: Oceans*, 117:C9, doi:10.1029/2012JC008105.
9. Sec 1.3, p5, l24. Citing Martinez et al. (2018) as Martinez-de la Torre et al. (2018) is more consistent with other references such as “Luiz do Vale Silva et al. (2018)”. Same in Sec 2.2, p10, l26 and Sec 2.2.1, p11, l7 and Sec 5, p26, l6 and Table 6.

C5

10. Sec 2, p6, l28. Suggest to include the fully coupled configuration identification/RUNID: UKC3aow.
11. Sec 2, p7, l26. Reference Castillo et al. (2017) should read Castillo and Lewis (2017).
12. Sec 2.1, p8, l2, Remove duplicate period at end of line (after development..)
13. Sec 2.1, p8, l4. Suggest to include (PS37) after UKA2 to be consistent with UKA3 (RA1/RA1-M).
14. Sec 2.1, p8, l11-13. The phrase “in the context of the UK regional coupled prediction system” is irrelevant in the context of this paper; remove it for simplicity and clarity. Add “on this parameter” at the end of this sentence to put the “strong sensitivity” in context.
15. Sec 2.1, p8, l23. “A number of incremental updates have been introduced in the RA1-M science configuration”. Only the pinned status of RA1-M is relevant not the way in which it was obtained . . . especially if not elaborated on further.
16. Sec 2.1, p8, l27 refers to a GA7 ticket. The introduction of Sec 2.1 indicates where the RA1 tickets can be found, but doesn’t indicate what GA7 ticket numbers refer to.
17. Sec 2.1, p9, l3 and p10,l14 and Sec 2.2, p10, l14 refer to GA tickets. Are those GA7 tickets, or should the first ticket also refer to just GA?
18. Sec 2.1, p9, l7. The term PBL hasn’t been defined. It probably refers to the planetary boundary layer, but this may not be clear for non-global non-atmospheric researchers.

C6

19. Sec 2.1, p9, l11-14. Reference is made to specific values (1.0 K and 1500 m) while other sections state changes without reference to numbers. This seems inconsistent.
20. Sec 2.2, p10, l12. The reference CCL (2018) is missing.
21. Sec 2.4, p12, l29. The exact path is NOT indicated in Table 9.
22. Sec 3, p13, l19. No references included for POLCOMS and WAM; seems to be inconsistent.
23. Sec 3.1, p14, l31. Figure 2 should be replaced by Figures 2(b) and 2(e).
24. Sec 3.2, p15, l13. The use of τ_{ocn} suggests a correlation between all stresses with the local atmospheric stress. The $\tau_{wav:ocn}$ is unlikely to show such correlation at local scales (as resolution increases to resolving surf zones and estuaries).
25. Sec 3.3, p16, l8. Figure 2, plots (c), (f) and (i).
26. Sec 3.4, p16, l20-24. It would help readers if $\frac{665}{0.85} \left(\frac{c_p}{u_*} \right)^{3/2} \frac{u_*}{g}$ were introduced more clearly as an estimate for H_s .
27. Sec 4.2, p17, l21. Rather than “complements” consider “extends”: This approach extends the analysis of Lewis et al. (2018) who considered only a number of relatively short 5-day case study simulations across a range of conditions to evaluate UKC2 performance.
28. Sec 4.2, p17, l24. That Lewis et al. (2018) didn't do long simulations doesn't imply that such simulations were never done, so remove “therefore” in the sentence “These experiments therefore represent the first time . . .”
29. Sec 4.2, p18, l10. “using more” instead of “usin more”.

C7

30. Sec 4.3, p18, l24-25. “In April, July and October runs . . . typically 0.2 K, but by up to 1 K during July 2014.” July should probably not be included in the first list of months; should this be February?
31. Sec 4.3, p19, l18 refers incorrectly to Figure 4. This should be Figure 3.
32. Sec 4.3, p19, l9 l12 l13 l19 refer incorrectly to Figure 5. This should be Figure 4.
33. Sec 4.3, p19, l9. The list of subplots should probably include subplot 4(h).
34. Sec 4.4, p19, l32 refers incorrectly to Figure 6. This should be Figure 5.
35. Sec 4.4, p20, l15. Figure 5(c) should probably refer to Figure 5(b).
36. Sec 4.4, p21, l5. Reference Lewis et al. (2018a) doesn't exist.
37. Sec 4.4, p21, l7. “due to” instead of “du to”
38. Sec 4.5, p21, l16. Reference Donlon et al. (2008) doesn't exist.
39. Sec 4.5, p21, l27. “notable” instead of “nonable”
40. Sec 4.5, p21, l29. “coastline” instead of “coastaline”
41. Sec 4.5, p22, l20. “experiments” instead of “experimnets”
42. Sec 4.5, p22, l28. “relatively increased sea surface (and air) temperatures” instead of “relatively enhanced . . .”
43. Sec 4.5, p23, l1. “increased” instead of “increases”
44. Sec 5, p24, l2. “UKC3aow provides a truly coupled system” . . . still without water flowing from the land to the ocean (Sec 2.3, p12, l10-12), so not so truly coupled.

C8

45. Sec 5, p24, l16. Most likely “. . . through further publications” instead of singular.
46. App B, p30, l5. “This capability is provided in NEMO from vn 4.0, . . .” instead of “This capability is provided at from NEMO vn 4.0, . . .”
47. App B, p30, l7. Which “Appendix III”?
48. App B.3, p32. This list of quantities that can be used during a wave to NEMO coupling includes three quantities “Normalized wave to ocean energy”, “mean wave number” and “peak frequency” that are not actually used in UKC3. This seems to be inconsistent with the title and introduction of App B that indicate that these NEMO wave forcing changes were implemented for UKC3.
49. References, p34, l26. DOI seems to be completely incorrect, should read 10.1029/98JC02622.
50. References, p34, l29-30. Duplicate reference entry . . . also p35, l1-3.
51. References, p35, l9. Bush et al (2018). Check names and submission status.
52. References, p37, l6. “Bakhoday Paskyabi” with space.
53. References, p37, l26. Kinter et al. (2012) not referred to.
54. References, p38, l3-4. Formatting deviates from rest of document.
55. References, p41, l18-23. Walters et al. (2017) in review. Check status.
56. Table 1, p42.
 - Most of the version information is also included in later tables. Consider restructuring to reduce duplication.

C9

- The table lists the atmosphere/land configuration of UKC2 as OS37. Based on the rest of the paper and Lewis et al. (2018) this should probably be PS37.
 - The table lists the atmosphere/land configuration of UKC3 as RA1-M. Throughout the paper seemingly random the terms RA1 and RA1-M are used. Consistency is suggested or clarify the difference.
 - OASIS3-MCT coupling libraries are consistent across versions and should therefore be in a cell merged across columns.
 - The “model domain” does not actually specify the model domain, but merely the model coordinates.
57. Table 4, p45.
 - Why does this table not include the atmosphere/land configuration science configuration ids PS37 and RA1-M? Isn't this a key difference for the atmosphere component? The ids are included in Table 5 also about the UKA2/3 components.
 - Wood et al. (2014) is not included in the references.
 - Arakawa and Lamb (1977) is not included in the references. Also in Table 8.
 - Charney and Phillips (1953) is not included in the references.
 - Brown et al. (2008) is not included in the references.
 - UKA3h simulation obtains SST from UKO3 simulation. Which simulation: UKO3g?
 58. Table 6, p47.
 - CEH (2007) is not included in the references.
 - Best (2005) is not included in the references.

C10

- PDM, RFM and UKV are not defined.

59. Table 8, p49-50.

- Umlauf and Burchard (2003) is not included in the references.
- Craig and Banner (1994) is not included in the references.
- Dee et al. (2011) is not included in the references.
- MacLachlan et al. (2015) is not included in the references.
- Horizontal boundary conditions section refers to “simulations based on 2015 dates in Sect. 5”. Reference doesn’t seem to be correct. No relevant information found in Sec 5.
- Siddorn et al. (2016) is not included in the references.

60. Tabl 9, p51.

- WAVEWATCH III model code base are different (see Table 1).
- Missing repository links as promised in Sec 2.4, p12, l29
- Bidlot et al. (2012) is not included in the references.
- Li (2008) is not included in the references.

61. Figure 2, p54, caption.

- “(a, d, g) normalized stress fraction τ_{oc} ” – this list of subplots shouldn’t include (g) which plots Charnock parameter.
- “In (h) . . . SB-75 blue dashed line.” There are multiple dashed lines. Remove unused and thus unnecessary lines.

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