

Comments on the reviewed manuscript “Modelling the marine ecosystem of IBI European waters for CMEMS operational applications” by E. Gutknecht, G. Reffray, A. Mignot, T. Dabrowski, and M.G. Sotillo.

The reviewed version of the manuscript is significantly improved, in particular for the clarification of the focus, now specifically targeted to the pre-operational assessment of the model system. I encourage the authors to also provide, in a next contribution, a quality assessment of the NRT products, which may have an important impact for CMEMS users.

In any case, I think this work will remain as a reference for biogeochemical modelling in the IBI area.

In the list of minor comments, I suggested some possible changes to the text, in particular concerning the use of the very short sentences, that may reduce the readability. A slightly major modification would be to reduce most of the Conclusions (lines 570 to 602) to 4/5 main results in form of items, to be considered as synthetical “take-home messages” – but I leave this option to authors.

Further, please pay particular attention to the three following comments, the second being potentially more effort-demanding:

1. Table 1 refers to the global synthesis performance rather than to the 12 specific areas: I remain of the idea that expressing the quality indicators (mean and std, mean error, RMSE, %Bias and Correlation) for each of the 12 boxes would be much more interesting – maybe including at least one more indicator (RMSE) in Figs. 4 and 6?
2. P14-L567-569: “This paper represents the first validation of the biogeochemical component of the IBI36 system: **the** objective is to show that PISCES can be used for operational applications, and that there is no contraindication to using the PISCES model at such a resolution.” Further, why do you use “contraindication”? is there any specific reason? You already wrote in Introduction that “While PISCES has so far been used to answer a wide range of scientific questions, it has never before been used at such a resolution.” but I cannot see any point to refer its use as a “contraindication”...please reformulate. Finally, and this may be worth of a separate discussion: you showed the assessment (in terms of consistency) of a 1/36 resolution model, but in the manuscript the advantages and the benefits of using such a high resolution are poorly underlined and discussed: how this version compares with the GLO product in the same area, for example? Would it be possible for the authors to briefly discuss more about the added value of a higher resolution system? I think the reader would be interested to see some consideration about this issue.
3. Captions of Fig. 12 and 15 report “r” as the Pearson correlation coefficient, but I cannot see any reference to r in the plots.

As a marginal note: for a better readability of the response letter, I would have preferred to find a more precise correspondence between the issues raised in the first review and the corrections done in the reviewed version (for example reporting the new version for each point, highlighting the number of pages/lines where modifications have been performed), since it has been a little bit cumbersome to check all the updates. However, the track changes version was helpful enough.

Minor comments:

4. P1-L12: better ", " than ";" ...: “...European waters, and more specifically...”

5. P1-L17: the use of “skill” would be more appropriate to evaluate model performance for operational systems (i.e. forecast skill, see for example Lorente et al., 2016¹), rather than for the pre-operational assessment. I suggest the authors consider to use “consistency and skill”, as already done in other parts of the manuscript.
6. P2-L47: maybe "...in Fig. 1a, and further..." ?
7. P2-L65: maybe one space less: "(the INDES0 project; Gutknecht et al., 2016)."
8. P2-L67: possibly remove "before" or put it at the end of the sentence
9. P2-L73: Also Lazzari et al., 2012 could be referred here: Lazzari, P., Solidoro, C., Ibello, V., Salon, S., Teruzzi, A., Béranger, K., Colella, S., and Crise, A.: Seasonal and interannual variability of plankton chlorophyll and primary production in the Mediterranean Sea: a modelling approach, *Biogeosciences*, 9, 217–233, <https://doi.org/10.5194/bg-9-217-2012>, 2012.
10. P3-L88: “synthetic” ?
11. P3-L97: possibly unify the 2 sentences: "...4.1), and are named..."
12. P3-L100: remove the comma between “and” and “when”: “...starts in spring, when seasonal re-stratification begins and when the Mixed Layer Depth (MLD)..."
13. P3 and P4 - L104, 114, 121, 125, 129, 141, 142: Fig. 1a
14. P4-L128: add year to reference “(UNEP LME report, 2008)”
15. P4-L132: “one biomass peak”, please consider to change to “a biomass peak” or, if pertinent, “a major/main/predominant biomass peak”
16. P5-L168-171: the 3 sentences could be unified for better readability, as an example (but please consider also other possibilities): “There are five nutrients that limit phytoplankton growth (nitrate and ammonium, phosphate, silicate and iron), **and the** model distinguishes two phytoplankton size compartments (nanophytoplankton and diatoms) expressed in carbon, iron, Chl-a and silicon content (the latter only for diatoms) and two zooplankton size classes (microzooplankton and mesozooplankton); **the** bacterial pool is not explicitly modelled.”
17. P5-L204: decide if use “NEWS” or “News”.
18. P6-L227: “...(Gohin et al., 2008).”
19. P6-L241: “validates”
20. P6-L242-252: these sentences appear too short and, to improve readability I suggest to slightly re-write this part

¹ <https://www.tandfonline.com/doi/full/10.1080/1755876X.2016.1215224>

21. P7-L264: “Finally, corrections are applied on each variable to correct from calibration biases and sensor drifts.” Repetition: possible suggestion: “Finally, corrections are applied on each variable to **remove/reduce** calibration biases and sensor drifts.”
22. P7-L265-268: these sentences appear too short and, to improve readability I suggest to slightly re-write this part
23. P7-L272: “**daily**”
24. P7-L286: Fig. 1a
25. P8-L301-306: I would mention also the biases around Faroe Islands: do you have any explanation for that?
26. P8-L321-324: suggested changes: “Coastal ecosystems of the Bay of Biscay (box 7) **show** a peak biomass during spring bloom, **while** the upwelling off Portugal and Morocco (boxes 8 and 10) presents a maximum in spring with more interannual variability off Morocco. **In the** Gulf of Cadiz (box 9) and the Western Mediterranean (boxes 11 and 12), **IBI36** succeeds in reproducing the seasonal cycle of Chl-a (Fig. 4), with a high correlation coefficient ($r > 0.71$) **with** the satellite product.”
27. P8-325-326: “In the open North Sea (**box 4**), the first peak is usually reproduced, but the data present a strong interannual variability. In the southern North Sea (**box 5**)...”
28. P10-L377-378: “In addition, the model does not capture the lower sea surface oxygen concentrations ~~than that~~ measured during 2014-2015 period (Fig. 10).”
29. P10-L399-400: “The statistics are low (Fig. 7) while the density plot, surface distribution and time series (Fig. 8 to 10) are positive.” ...not clear: can you formulate better?
30. P10-L406-407: suggestion... “Oxygen content is a key element in biogeochemical cycles and can be an indicator of the health of marine ecosystems: **for this reason we analyse** the minimum oxygen concentrations.”
31. P11-L428: “illustrates”
32. P11-L428: I would say “**Model** oxygen and nutrients show...”
33. P11-L429: “0.95, **see** Fig. 7.” Or simply “0.95 (Fig. 7).”
34. P12-L458: I would avoid adverbs as “now”, “comparison **with**...”
35. P12-L482: “...develops **in** summer in the model simulation and is not present in the observations”
36. P13-L492-494: suggestion “...providing the trails for improvement to be explored, **which are here** discussed.”
37. P13-L517: should it be “**even** out of phase” ?
38. P13-L520: “performs”

39. P13-L522-526: again, to improve readability I suggest: “This behaviour is also observed in the global model at $\frac{1}{4}^\circ$ (Perruche et al., 2016) used **for/to set-up** the initial and open boundary conditions, **and** can originate from the physical or biogeochemical models. Different approaches are currently under study: **in particular, vertical** diffusion could explain the loss of peaks and minima in vertical profiles, but biogeochemical processes (**e.g.**, parameterization of remineralisation processes, rate of sinking of particulate detritus, vertical migration of zooplankton which export organic matter at depth) are* also investigated.” *using “are” it means you are showing this somewhere: if this is not the case, I would use “will be” or something similar.
40. P14-L538: “This assumption may be too restrictive: **as** an alternative ...”
41. P14-L566: I think “description” or similar may be better than “vision”...
42. P14-L570-571: again, some suggested improvement for readability “Chl-a and NPP are compared to satellite estimates, **describing here their** mean spatial distribution and seasonal cycle.”
43. P14-L574-576: “Some of these areas are outside of the IBI Service Domain (that is the geographical domain covered by the CMEMS IBI-MFC products), **but** in order to take advantage of their *in situ* observational coverage, **we also evaluated** the IBI Extended Domain.”
44. P15-L579: “MOC”? maybe you refer to Deep Oxygen Maximum (DOM)?
45. P15-L583-586: “~~Simulated NPP lies between the three NPP products.~~ The **model** averaged spatial distribution is close to CbPM product, but spatial distribution, cross-shore gradients and the seasonal variations are better correlated to the VGPM-based products. The modelled NPP is thus within the range of variability of the satellite derived estimates.” There is a repetition between first and last sentences: I would remove the first sentence and add “model” in the following.
46. P15-L588: highlights
47. Caption Fig.1, L905: “(boxes 11 and 12).”; L908: why "font" ?... maybe "blue colour shading" or something similar?