Interactive comment on "A CMEMS forecasting system for the marine ecosystem of IBI European waters" by Elodie Gutknecht et al.

Anonymous Referee #2

Received and published: 13 March 2019

Dear Referee,

Please find our comments/responses in blue throughout your text.
We have also attached a new release of the article.
This is a major revision of the initial article: the introduction has been completely revised, the sections are modified, a discussion section has been added. A synthesis of the results has been added by using a table and a Taylor diagram.
All your comments have been taken into account in the new release of the article.
If required we can also provide a version with the word track change, but I don't think that's helpful to you.
Thank you for your useful comments.

Elodie Gutknecht on behalf of co-authors.

The main objective of the article "A CMEMS forecasting system for the marine ecosystem of IBI European waters" is to demonstrate the performances of the CMEMS operational 1/36° coupled NEMO/PISCES of the IBI (extended) area from a 2010-2016, 7 years model simulation. The authors state that the model is in relatively good agreement with observations (for the main biogeochemical variables) and reproduce the large scale main biogeochemical processes, with a focus on seasonal cycles. In addition, an interesting oxygen deficiency indicator is presented. The article includes a wide variety of comparisons (15 figures) computed from an impressive set of observation. The document is well written and a very valuable effort has been done to make it as clear as possible. It results in a complete review of PISCES ability to simulate large scale main biogeochemical features in the IBI domain. In my opinion, despite these undeniable qualities, the article misses some important points and there are several major issues that should be (at least mostly) addressed before publication. The "general recommendations section" describes major points that, in my opinion, need to be improved before publication. The second section only provides some specific comments that could help to address the general recommendation needs. I did not go into very specific details as I think some materials should be added (and modified) before going further.

General recommendations:

A) In the actual form, the article is rather a review (interesting) of results from a 7 years PISCES simulation in the IBI region. It not clear to me whether the objectives are 1) demonstrate the ability of this system for regional forecast applications or 2) to assess PISCES ability to represent the main biogeochemical features of the IBI region => suitable for operational applications. In any case, important modifications need to be made and the introduction should be extended to better mention the objectives and the means used to address these objectives. From the sentence p2, line27, I guess the main objective is more on point 2) but the way it is written gives the impression that the authors want to assess the relevancy of this configuration for operational 7 day forecasts which is clearly not at all demonstrated here (ex 4.1.1. first sentence "Predicted sea surface..." should be rather something like "the model sea surface chlorophyll...").

Author answer:
The IBI36 system has been developed to monitor and forecast the ocean dynamics and marine ecosystems of the European waters. But before considering operational applications, a pre-operational qualification simulation is evaluated to assess PISCES ability to represent the main biogeochemical features of the IBI region. The purpose of this paper is to evaluate this qualification simulation.
The title and the Introduction of the submitted version are confusing. So to avoid any misunderstanding, we propose to modify the title of the paper by: "Modelling the marine ecosystem of IBI European waters for CMEMS operational applications". We also modified the abstract and the Introduction to make clearer the objectives of the paper.

This comment also underlines a lack of focus. The result section is more a presentation of various diagnosis than a scientific analysis of the results which might be induced by too unclear objectives of the paper. I understand that the paper is more an analysis of "consistency" but the reader can sometimes be irritated when a figure is nearly not discussed. If you decide to mainly make this scientific analysis in the discussion part i would suggest to dedicate a full section to this discussion (instead of discussion and conclusion) and to clearly state this point in introduction. Finally, taking into account a focus on objective 2), we sometimes lose the purpose of the paper and i am not sure that the article really answer whether or not this PISCES simulation is well adapted to operational simulation in the IBI area (i am sure it is). In my opinion this it mainly the consequence of an efficient but also too straight way of writing with a lack of methodology: "the role of this figure is to show this point, which demonstrates this information, which is related to my main objectives in this way". Nevertheless, i am sure that this could be corrected quite easily as there is also a very robust amount of information.

Author answer:
The entire text has been revised in order to clarify the objectives of the paper. The objective of each figure, each section and sub-section is now better apprehended. Figures are now more discussed. And a full section is now dedicated to the discussion, and it is clarified in the Introduction.

B) One major issue is the total absence of informations regarding uncertainties (except in figure 4). In my opinion, this point is crucial to assess the performance of any simulation even if the authors decide to only focus on point 2). As this might be difficult (and not necessary) to consider uncertainty for every comparison, I suggest to add one section dedicated to uncertainties. It should both discuss uncertainties of the PISCES simulation and the observation products. One simple question that should be addressed: is the model simulation included within the range of observation uncertainty? For instance, first order uncertainties could be deduced from the statistical level of dispersion (in a specific radius representative of error correlations). This particular suggestion might probably be better adapted for comparison with ocean colour data (or when a large amount of data is available and could be presented with histograms).

Author answer:

The issue of uncertainty is a truly complex one. The model uncertainties can be supported by the use of ensemble simulations. Here, the IBI36 system is a deterministic model. Only one trajectory is described. The uncertainties of the data are also complex to access, and are not always accessible.

In order to complete the skill assessment and better apprehend the uncertainty of the model, the bias and RMSE with to the ESA OC-CCI is added for Chl-a evaluation. Uncertainties (in terms of bias and RMSE) are the greatest in the coastal areas, which also correspond to the areas where the uncertainty of satellite measurements is highest (100% uncertainty compared to 30% for the open ocean; Moore et al., 2009). For NPP, the standard deviation between the three NPP products (VGPM, Eppley-VGPM and CbPM) is compared to the bias between the model and the mean of the three NPP products. This comparison shows that the modelled NPP is included within the range of observation uncertainty (as standard deviation between the three NPP products is considerable). Uncertainty is discussed in the discussion section (Sect. 5).

The suggested analysis deduced from error correlations is a work in progress in the framework of the development of an assimilation system for ocean colour data and in-situ data. This analysis represents a significant work that will be described in detail when setting up the future system with assimilation of biogeochemical tracers.

C) Particularly using a 1/36° model resolution, it is quite a shame not to show simple weekly or monthly mean maps (at least for chlorophyll) highlighting this PISCES simulation ability to catch the variability existing in ocean colour data. This would be relevant to insist on the benefits of using a 1/36° resolution. Are there such 1/36 PISCES simulation elsewhere? What are the benefits compare to lower resolution models? Why do you need this resolution here? I really think that it would be of great importance to compare your results with other existing configurations or models. This would also be a great help to demonstrate that your configuration is well adapted to simulate the IBI biogeochemical features (to solve some of the general recommendation A)). If it is really not possible to perform comparisons this have to be clearly stated in introduction. More generally, the introduction completely misses the state of the art part. This definitely has to be corrected.

Author answer:

The PISCES model is intended to be used for both regional and global configurations at high or low spatial resolutions as well as for short-term (seasonal, interannual) and long-term (climate change, paleoceanography) analyses (Aumont et al., 2015). But to our knowledge and that of the PISCES developers, the PISCES model has never been used at such a resolution before.

Within the framework of CMEMS, three other MFC share a part of their domain with IBI:
- GLO-MFC which covers the world's oceans at 1/4° resolution and is also using the PISCES biogeochemical model,
- MED-MFC which covers the Mediterranean Sea at 1/24° with the Biogeochemical Flux Model (BFM; Vichi et al., 2007a,b),
- NWS-MFC which covers the North-West European Shelf at 1/15° latitudinal resolution and 1/9° longitudinal resolution (~ 7km) with the ERSEM ecosystem model (Baretta et al., 1995).

For the physical component, two intercomparison papers have been submitted in ocean Science - Special issue "The Copernicus Marine Environment Monitoring Service (CMEMS): scientific advances". They are Lorente et al. (2019) and Mason et al. (2019).

For the biogeochemical component, the comparison of the different model applications is a work in progress and will be the subject of a separate paper, including the contribution of the regional in relation to the global, as well as the comparison of three distinct biogeochemical models.

The Introduction has been revised and now includes a state of the art part.

References:

Baretta, J. W., W. Ebenhoh, et al. (1995). "The European regional seas ecosystem model, a complex marine ecosystem model." Netherlands Journal of Sea Research 33(3-4): 233-246.
Lorente, P., García-Sotillo, M., Amo-Baladrón, A., Aznar, R., Levier, B., Sánchez-Garrido, J. C., Sammartino, S., De Pascual, Á., Reffray, G., Toledano, C., and Álvarez-Fanjul, E.: Skill assessment of global, regional and coastal circulation forecast models: evaluating the benefits of dynamical downscaling in IBI surface waters, Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-168, in review, 2019.
Mason, E., Ruiz, S., Bourdalle-Badie, R., Reffray, G., Garcia-Sotillo, M., and Pascual, A.: Copernicus (CMEMS) operational model intercomparison in the western Mediterranean Sea: Insights from an eddy tracker, Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-169, in review, 2019.
Vichi, M., Pinardi, N., and Masina, S.: A generalized model of pelagic biogeochemistry for the global ocean ecosystem. Part I: theory, J. Marine Syst., 64, 89-109, https://doi.org/10.1016/j.jmarsys.2006.03.006, 2007a.
Vichi, M., Masina, S., and Navarra, A.: A generalized model of pelagic biogeochemistry for the global ocean ecosystem. Part II: numerical simulations, J. Marine Syst., 64, 110-134, https://doi.org/10.1016/j.jmarsys.2006.03.014, 2007b.

D) In order to make the document easier to read (in particular considering the probable adding figures (from my previous comments), i also suggest to add an annex section. Indeed some figures are only quickly and partially discussed (for instance : first paragraph of section 4,2,1 only 8 lines to described 3 figures ; in section 4,1,1, 4 figures are covered ; fig 7,8,9 f) are not discussed...). This would also allow to better focus the article on its objectives. (This comment is connected with the general recommendation 1)).

Author answer:
The objectives are now clearly presented in the Introduction, and figures are better discussed. We are aware that it is a substantial paper, with many figures. But it's necessary and voluntary. This paper represents the first validation of the biogeochemical component of the IBI36 system; it is intended to be complete and detailed, as it will serve as a basis for further studies.

Main Specific comments:

Considering the major recommendations, i here only specify, by section, the main specific comments as there will probably have a second review process. Generally, i do think that some accuracy is needed.

1) Introduction - See section major recommendations.
It has to be extended in order to clearly explain the objectives of the article and present a clear state of the art (in terms of existing forecast studies with PISCES and on models on the IBI region) in the area.

Author answer:
Introduction has been fully revised to clearly explain the objectives of the article. A state of the art in terms of existing forecast studies with PISCES and on models on the IBI region is now available.

2) IBI European waters
Interesting and quite complete part. It is a nevertheless a shame that the link between this part and the result section are nearly non-existent.

Author answer:
IBI European waters Section has been improved. It now introduce the boxes used for the evaluation of Chl-a and NPP. The link between this part and the result section in now improved.

3) The IBI36 configuration
- What is the influence of a 1/4° (bio) and 1/12° (physics) initialisation in the 1/36° simulation ? Do you have an idea how long this information is kept in the system, about the differences ?

Don’t you think it can strongly impact the first timing and intensity of the blooms (especially with an initialization in January ? Why don’t you make a few years spin-off ? - I would be very interested to have some informations on differences between a 1/4° and a 1/36° simulations. It would also help to justify the use of your configuration.

Author answer:

Initialization and open boundary conditions of the IBI36 system answer the CMEMS requirements. The physics comes from the CMEMS global physical component and the biogeochemical conditions come the CMEMS global biogeochemical system, this latter being also forced by the CMEMS global physical component.

The influence of a 1/4° (bio) and 1/12° (physics) initialisation and forcing in the 1/36° simulation is an interesting question. Impact analyses have been performed in the framework of the AMICO project, but only for short term simulations (1-2 years), and so the impacts were limited to the borders of the domain, but longer simulations would be necessary.

The date of the initialisation has been tested. The initialisation in January benefits from a low productivity. The seasonal dynamics triggered by seasonal warming and stratification has not yet begun. And so, the system is slowly being set up.

A few years spin-off are not performed here. The IBI36 system starts with the outputs of the global physical and biogeochemical systems, based on the same NEMO-PISCES model, and which has already turned 3 years. The main strengths and weaknesses of the model are found in both global and regional systems, such as the timing of the North Atlantic spring bloom. Systematic comparison between the global and regional systems is being performed, but we think that showing such metrics does not enter in the topic of this paper.

Some additional information about the nutrient forcing files would be welcome as i suppose that it could mainly explain most of the coastal deviations with observations. Do you have an idea of probable impacts of using 1/2_ data to 1/36_ grid, how do you deal with this ?

Author answer:

Nutrient inputs come from the annual climatology at ½° spatial resolution Global News 2. They are reported to the 1/36° grid at the river plumes considered in the IBI36 system (Rhone River and the German Bight) and along the coastline for all other inputs. Thus, we have endeavoured to conserve the nutrient flows between the original Global News 2 grid and the IBI36 grid. Mayorga et al. (2010) report that Global NEWS 2 underestimates nutrient runoffs in the Western Europe. Indeed, the only contribution of Global NEWS 2 is not sufficient to support the high coastal biological production of the IBI European waters. In order to reproduce the maximum Chl-a observed along the European coasts, additional inputs of nitrates and phosphates are provided into the IBI36 system at source points of the 33 main rivers and are linked to the physical flow. Location of the rivers can be found in Maraldi et al. (2013). They come from rivers monitored and listed by the European Environment Agency (www.eea.europa.eu) on the basis of annual averages.

Nutrient forcing description was not clear and maybe confusing. The description is now improved in Section 3.2. Nutrient inputs from rivers are of primary importance for coastal ecosystems. Unfortunately, they are often introduced in a too simplistic way in regional models due to a crucial lack of available measurements. This is a weakness that really needs to be considered and improved, but that is not intrinsic to the IBI36 system.

4) IBI36 evaluation

4.1 Satellite estimations

- How is the temporal correlation figure 2d calculated ? From monthly averages ? - On which grid are the differences calculated (model or verification) ? Are there impacts resulting from this grid changing ? In particularly for Net primary production comparison (1/6 degree compared to 1/36 degree). Please clarify this point as non linearities can have significant effects. - It is difficult to see something in figure 3. The discussion on the bloom timing could also be done using different boxes of figure 4. This would permit to remove one figure (or in annex). - p6. lines 18-21 you say that Net primary production estimates are model products ? If it is true why do you include these data sets in a section 4,1 called satellite estimations ?

Author answer:

Temporal correlation is calculated from monthly averages between 2010 and 2016.). It is now clearly stated at the beginning of Section 4.1.

We have chosen to interpolate the model on the data grid, i. e. 1 km for the comparison to ESA OC-CCI ocean colour product and 1/6° for NPP.

Yes, the discussion on the bloom timing could be done only using different boxes of Figure 4, and we could remove Figure 3. But Figures 3 and 4 are complementary. Figure 3 clearly shows the seasonal phase shift and the high interannual variability in the data while the model is more seasonal. This is more evidenced than in Figure 4.

Net primary production estimates are model products because an algorithm (or model) is used to determine NPP from ocean colour data. But it is considered primarily as a product derived from satellite estimates. To avoid any misunderstanding, the term "NPP product" instead of NPP model" is used in the revised version.

4,2 In situ historical data

- In fig 8, for oxygen, it would be clearer if you could modify the colorbar. At a first view we think that the data and the model are very different while the bias is only 4% - It is a shame you do not discuss at all some possible reasons why the model does not catch the low oxygen period in 2014-2015. Especially when you thereafter discuss about oxygen deficiencies: : : -Don 't you think that one of the main limitation comes from the nutrient forcing files ? Could you specify a little bit more (than it is in 3.2) as it seems to be quite important ? Where are the anthropic inputs located in the model ? What is the impact of these additional anthropic inputs ? It could be relevant to go a little bit deeper into this point.

Author answer:
The colorbar of Figure 9 has been modified.

The model does not catch the lower oxygen concentrations observed in ICES in 2014-2015. It can come from the Baltic Sea or from river inflows. But I didn't find any reference to this event in the literature.

The oxygen-deficient areas are quite well estimated by the model in terms of spatial distribution and extension. They are located mainly along the coastline and are certainly impacted by river inflows but also sedimentary processes. More realistic external forcing files would obviously improve the estimation of vulnerable areas. But as explained above, they are introduced in a too simplistic way due to a crucial lack of available measurements.

Nutrient forcing description was not clear and maybe confusing. The terms "natural inputs" and "anthropogenic inputs" are now removed because they are not really accurate and could be confusing. The description is now improved in Section 3.2.

This adaptation was necessary to simulate higher coastal Chl-a and reproduce the maximum Chl-a observed along the European coasts. But it is not totally satisfactory because a strong seasonal signal seems to emerge from ICES comparisons. The future system will improve this external forcing.

4,3 Argo data

How are the Argo data co-located with the model data ? Do you grid Argo data on the model grid ? Could you re-precise dates ? Although correlations are still high, results are much less good than previously. Do you have an idea why ? Small scale features ? Have you compared the Argo data with some of the previous observations ? (it also could help in defining uncertainty levels).

Author answer:
As for the satellite comparison, we have chosen to interpolate the model on the BGC-Argo data grid. We used daily averaged model outputs at the nearest model grid point, and a linear interpolation for the vertical.

BGC-Argo float comparisons open new perspectives. Bu the comparison should be considered with some cautions because the product quality procedures are on-going work. They are not fully established or homogenized for all floats. Temporal drifts, constant or even non-constant vertical bias, and negative concentrations (see the nitrate in Fig. 15d) are still observed in the BGC-Argo data.

4,4 Discussion and conclusion

- I would suggest to separate the discussion and the conclusion since the authors have clearly decided not to deeply analyze the results in the result section. The discussion proposed here is interesting but should be extended and also make a clear link better with the objectives that should be first clarified in introduction.

Author answer:

Discussion and Conclusions are now separated, and the link with the objectives of the paper as clarified in introduction is now improved.

We decided to only describe the results in Section 4, and then deeply analyse the results in the Discussion section. This structure seems simpler because some aspects come up several times in the evaluation (smoothed vertical profiles, rivers, sediments…). So we discuss them once and for all in the dedicated section.