Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-161-RC2, 2019

Ocean Science
Discussions

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

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 biogeachemical features in the IBI domain. In my opinion, despite these

undeniable qualities, the article miss some important points and there are several major issues that should be (at least mostly) adressed 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 recommandation needs. I did not go into very specific details as i think some materials should be added (and modified) before going further.

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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 adress 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..."). 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 writting 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.

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).

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) highligting this PISCES simulation ability to catch the variability existing in ocean colour data. This would be relevant to insit 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 comparions this have to be clearly stated in introduction. More generally, the introduction completely miss the state of the art part. This definitely

has to be corrected.

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 annexe 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 recommandation 1))

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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.

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.

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. Some additional information about the nutrient

forcing files would be welcome as i suppose that it could mainly explain most of the 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 ?

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 ?

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.

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 hight, 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)

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.

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