

Interactive comment on “Physical processes mediating climate change impacts on regional sea ecosystems” by J. Holt et al.

Anonymous Referee #1

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This paper addresses the overall climate change effect on net primary production, which is a very important issue. However the presentation is largely unbalanced and not well focused. In the present form the manuscript does not meet the standards of Biogeosciences and I don't recommend publication in this form.

1. General comments

Because forcing methodology differs between models it seems misleading to interpret (and compare!) the differences between results in individual basins as due to climate change.

Authors promise that they “explore these physical processes and their biophysical interactions”. Process analysis as presented is very weak and not enough quantitative

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(with some exceptions in northwest European shelf).

Section 2 is almost 20 p review of others people's work. What is here new? How much the specific results in section 3 are related to section 2? Perhaps remove this section and substitute it with a new one which well explains what has been done in this paper, and not so much in earlier publications.

In order to ensure reproduction of results by other scientists clear information is needed about characteristics of individual models (including parameters etc.). Table 1 is not enough.

2. Specific comments

The abbreviation netpp appears first in section 3. However it is used in the caption of Fig. 1 much earlier.

Can authors comment on the consistency between individual plots in Fig. 1 and the global pattern. It seems that the values at the boundaries of regional models are different from the ones of global model. Or this is due to using different color bars?

There are statements like "The model simulations are drawn from the MEECE project, and so have a degree of harmonisation between them." This needs better explanation. Can the authors make this statement more specific. What is in their opinion the needed degree of harmonization and how good was the harmonization in MEECE.

I am not sure whether it is in the policy of the journal to cite www non-refereed reports (MEECE). Please, try to avoid such references. Perhaps the problem is that no all models are equally well documented in the literature. If this is the case the problem is serious.

The authors say: "For expediency, we focus on the POLCOMS-ERSEM model of the northwest European shelf (NWS) for much of the further analysis." I would suggest that they restruct their paper to POLCOMS-ERSEM model and remove all other incomplete pieces of information about other models. Perhaps they can keep ECOSMO,

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but increase the presentation of its results. If you do this, please address carefully the differences and similarities between POLCOMS-ERSEM and ECOSMO. The argument to have all MEECE models in one paper (perhaps) does not hold because the Mediterranean and Biscay Bay models are not included. Authors did not address the question why. Actually what I see in MEECE website is a different composition of regions (<http://www.meece.eu/regions.html>). This is another argument not to refer to www sources.

Authors have to be careful in balancing between what they and other researchers have said in past publications and what is described here as a new research. Based on the example of Cannaby et al. (2014), I would ask what the present manuscript adds to the fundamental issues in the Black Sea in comparison to Cannaby et al. (2014). In cases like this authors have to present manuscripts under revision.

The discussion in the second part of section 3.1 is interesting, but not all causal effects can be derived from the illustrations.

Please avoid statements like “most likely arises from” (p 1939, line 20), but give quantitative evidence.

I do not comment section 3.2 because this is truly POLCOMS-ERSEM section. If authors decide to write the paper as POLCOMS-ERSEM paper this part has to be reviewed new depending on the context of the paper.

I would recommend that authors relate section 4 more closely to the results of the present study. As it is written, it is too general. The new results (after line 20, p. 1944) need more weight in comparison with the long discussion in the beginning of this section.

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