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

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We thank you for your consideration of this paper. These are our responses to the specific issues you raise. We provide a detailed plan to revise the paper in the Authors Response.

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.

The issue raised here is rather unclear. In a forced system the forcing methodology becomes part of the modelling approach, just as for example the surface flux parameterisation is part of the approach. The climate change response of two different models

is a combination of the changes in climate system they are being forced with and the modelling approach. The former is common in all the regions here; the latter differs in many ways between the models (including the forcing methodology). This does mean the quantitative comparison of the climate change response between regions using different models needs to be treated with caution, as it mixes the actually difference in response with (the non-linear) aspects of the differences in modelling approaches (simplistically linear aspects of the response would be expected to 'cancel'). This does not mean that useful qualitative differences between regions cannot be derived from such a comparison, in a process oriented analysis. This does mean that details of the approach need to be clearly stated (which we will address in the revision), and this issue brought up in the discussion section.

Process analysis as presented is very weak and not enough quantitative.

While we acknowledge that this needs to be substantially strengthened, we would point out that, in our opinion, the analysis presented in fig 6 and 7 is novel and informative - it synthesises a complex range of responses into a tractable picture. We have, in these simulations a substantial wealth of information to build on this, and how we proceed is described in our plan for revision.

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.

New understanding of the important scientific issues pertinent to a problem can be developed through the review and synthesis of previous work, and by relating this to a particular conceptual framework, which it might either support or question. As far as we are aware this is first time the various physical processes potentially important to the impacts of climate change in regional seas have been brought together in a common framework (the three paradigms of biophysical interaction we identify), and most importantly, contrasted with the expected impacts in the open ocean. The clear message here is that lessons learned from the CMIP models are not readily transferable to regional seas, and a different analysis is required. However, what is often missing in section 2 is detailed supporting evidence based on the new model simulations. Essentially bringing together sections 2 and 3 is what we aim to specifically address in our plan for revision. The reviewer hints that this section is too long. To address this we will separate out and summarise introductory material, and move the discussion of particular processes that we identify in our model results to sit alongside the discussion of those results. We will remove passages that are more speculative, and we cannot support either with these or previously published simulations, but still identify key gaps in our approach, e.g. relating to model resolution near the coast.

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.

We concur and will add more detailed descriptions of the models and experiment design.

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?

The global model provides inorganic nutrient boundary conditions for the regional models. The netPP is an emergent property of the regional models and not specifically constrained by this condition. So we would not expect them to necessarily match at the boundary, as the ecosystems models differ substantially.

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.

Indeed – we will provide a more full description of the experiments

What is in their opinion the needed degree of harmonization and how good was the harmonization in MEECE.

The need for harmonisation is articulated in the response to the first comment above. Short of running a common model, which is difficult for a multi-institute project, there has to be an element of compromise here. In MEECE the harmonisation was good, particularly considering the starting point, although in hindsight some aspects could have been improved (e.g. a more detailed set of pre-defined model output).

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.

We will provide a more comprehensive description of the models and the experiments

The argument to have all MEECE models in one paper (perhaps) does not hold because the Mediteranean and Biscay Bay models are not included. Authors did not address the question why.

It was never our intension to make this argument. This paper only includes a subset of the MEECE models, simply because the ability existed with these to further explore the details of the biophysical interactions. Other papers, e.g. Chust et al (2014) have explored other aspects of this set of models.

I would suggest that they restrict their paper to POLCOMS-ERSEM model and remove all other incomplete pieces of information about other models. Perhaps they can keep ECOSMO. . .

Multi-model, multi-region analyses of potential climate impacts in regional seas are exceptionally rare. Apart from the analysis in Chust et al (2014) this is the first time this group of regions, which are important in the European context, has been brought together in a common analysis, which can explore and attempt to explain the heterogeneity of impact on the primary production in terms of the driving physical processes. Hence in our view it represents a significant step forward in the state-of-the-art in this field, albeit still lagging the degree of coordination seen in the CMIP process. On this basis we feel it important to retain the five regions and three models in this paper. We agree a weakness of the paper is it is overly focused on the Northwest European shelf. The solution to this is not to focus it entirely on this region. For example, that would neglect an important conclusion from the paper: that two highly stratified deep sea basins (the Baltic and Black Sea), both show a (potential) response to climate change that is completely contrary to what would be expected from a global ocean view. In our view, a more appropriate response to this weakness is to introduce some regionally specific model results for each region to illustrate the processes identified in 2.4 and 2.5, and to strengthen the common analysis. This will allow us to better exploit the fact that we have under consideration five contrasting regions forced by a common global ESM (albeit using three different modelling approaches). To rebalance, this will be done at the expense of some of the original NW European shelf results.

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).

The difference between this paper, as it stands, and Cannaby (2014), is primarily the comparison with the Baltic and the regions exposed to the open ocean. While the mechanisms at work in the Black Sea are largely described in Cannaby (2014), and referenced as such, the similarities with the behaviour in the Baltic are not. We will provide further model diagnostics specific to this region to build on that paper and develop this comparison further in the planned revision.

If it will help, we are happy to make the in-revision version of Cannaby (2014) available to reviewers and editor of this 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.

We agree this section is too general, but to do this important topic real justice goes beyond the scope of this paper.

References

Cannaby, H., et al., In Revision. Climatic controls on biophysical interactions in the Black Sea under present day conditions and a potential future (A1B) climate scenario, Journal of Marine Systems

Chust, G., et al., 2014. Biomass changes and trophic amplification of plankton in a warmer ocean, Global Change Biology, DOI: 10.1111/gcb.12562

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