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## ***Interactive comment on “Projected climate change impacts on North Sea and Baltic Sea: CMIP3 and CMIP5 model based scenarios” by D. Pushpadas et al.***

**D. Pushpadas et al.**

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We wish to thank the reviewer for the criticism and suggestions and will consider them in a revised manuscript- Please find our specific comments below.

Reviewer’s comment: This manuscript assesses climate change impacts to the North Sea and Baltic Sea using the regional bio-physical model ECOSMO forced by output from three CMIP3/A1B and three CMIP5/RCP4.5 global Earth System Models (ESMs). The main findings of the manuscript are similar to those of previous studies.

Response: Our study contributes to resolve and further evaluate potential contrasting conclusions from earlier studies about projected primary production changes in the

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North Sea. Holt et al. (2014, reference in the manuscript) found the North Sea less sensitive to production changes and projected less consequences of climate change for the North Sea compared to the global ocean, whereby Gröger et al (2013, reference in the manuscript) found the opposite, a more sensitive North Sea production to climate change. The question of regional consequences of climate change to the North Sea is therefore far from settled and our study contributes important results to the scientific debate in this respect. Moreover, it evaluates the robustness of regional projected primary production change by presenting for the first time a consistent multi-member downscaling ensemble for the North Sea and Baltic Sea. Our results thereby allow for the first time to span a range for projected primary production changes. In our opinion this is a very important result and should not only be of interest for a small number of climate scientists working on the regional scale, but also to those who further think through the risk of climate change in these regional systems, such as biologists working on consequences of climate change for different organisms or those who would further evaluate consequences of climate change for pollutants and fisheries. Our study is the first one, that presents regional downscaled projections from CMIP5 global scenarios developed for the latest IPCC assessment, and we address the research question whether the regional projected changes are different from AR4 to AR5 and whether the new AR5 scenarios result in increased convergence in regional projections (e.g. due to improved global models) compared to the earlier AR4, which is indeed the case for the North Sea primary production, but not for the Baltic Sea. More over we present a number of new results on forcing and causing mechanisms in the scenario simulations, these include variations in mixed layer depth, the role of atmospheric vs oceanic drivers and regional variations in trophic amplification.

We realize that the novelty of our study became not as clear as we desired, and we will expand on the discussion in a revised version of the manuscript.

Reviewer's comment: The authors mention that multi-model ensemble estimates of climate change simulations based on CMIP3 and CMIP5 scenarios were carried out

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with the objective to provide more reliable and robust regional information on climate change response to the North Sea and Baltic Sea (p. 12234, top lines). However, they do not elaborate on why these projections are more reliable and robust than previous ones. Since, similar to previous downscaling studies, they use the “delta method” and no new technique or analysis seem to be applied, it is not clear why their projections should be better. In addition, results from previous studies are similar to the one presented here and, apart from using projections from CMIP5 models as well as from CMIP3 models, it is not clear what is new here. The authors should clearly state the main goal of the study and include an explicit description of the novelty and value of this work relative to previous work

Response: The expected improvement lays in the use of the new CMIP5 scenarios rather than the old CMIP3 forcing global climate models, not in the use of a new downscaling technique. Latest CMIP5 models are expected to provide more detail and more certain projections owing to the substantial model development after CMIP3 (Knutti & Sedlacek, 2012) and the regional downscaling of CMIP5 models in the North Sea and Baltic Sea is at the initial stage and our study is the first one of this kind. It is important for the climate community to realize whether these expectations are achieved. In this perspective, a comparison of CMIP5 and CMIP3 models and projections is important and necessary to identify uncertainties associated with regional climate change projections and also to re-evaluate previous research and projections, which we have done in this study. Further, we tried to give a mean value and range of climate response of the ecosystems with different simulations rather than foretelling single projection, which is the usual practice in regional climate change studies. Applying new downscaling techniques was not our focus, from previous studies it is evident that projected changes will be different if e.g. direct forcing is used instead of the ‘Delta Method’. However, the ‘Delta Method’ is also ensuring bias correction and thereby realistic sensitivity to change of the system under investigation, which was the reason for us to choose this method as the most robust. From the reviewer’s comment, we understand that we need to explain the motivation and novelty of our work more clearly in a revised manuscript.

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Reviewer's comment: Not enough information is given on the coupled bio-physical model ECOSMO and the selected ESM models. This information is necessary to ensure reproduction of results by other scientists and to compare these results with those from other studies. For example, information is not provided on the resolution (horizontal and vertical) of the global and regional models, biogeochemical components (biological variables and parameter values), and initial and boundary conditions. In particular, given the importance of changes in oceanic nutrient input to the projected changes in North Sea primary production, the authors should describe how the boundary conditions for the future scenario simulations were downscaled from the coarse resolution Global Earth System Models (ESMs).

Response: We agree that a more elaborate presentation of methods is desirable and we will provide this information in the revised manuscript.

Reviewer's comment: In general, the results are poorly presented and some important results are not shown. For example: – It is not clear if changes in North Sea primary production are due to decrease in nutrient concentration in the surface layer or deep layer (results not shown). Based on nitrate changes in the surface layer (Fig. 3), one would expect a larger reduction of primary production in IPSL-CM5.

Response: We agree, we only presented surface changes. Our selection of the results presented was motivated by scientific reasons. Primary production in the North Sea is controlled by winter nutrient inflow from the North Atlantic. The total nutrient reduction is more for ECHAM model (specifically in winter) for both surface layer and deep layer that explain the larger reduction in NPP. The surface (upper 50m) and below changes at the northern North Sea boundary are very similar, since the water column here is well mixed in winter. The water column is of course stratified during summer and change in surface and lower layers could be different. However, the latter are less important for primary production, since these nutrients are not supplied to the euphotic zone. We will add respective explanations to the manuscript and can provide supplementary information about below surface layer changes and N/P ratio to the reader in the revised

manuscript.

Reviewer's comment: Why was January chosen to show projected nitrate changes in winter (Fig. 10)? Changes in open ocean boundary are quite small in January compared to February (Fig. 3).

Response: Since nutrient changes in February can be affected by an early spring production under changing climate, we have picked January to represent winter nutrient changes. We will explain this in the revised manuscript.

Reviewer's comment: – The color scale of Fig. 7 is not contrasting enough to show changes in the variable.

Response: We will modify the color scale in the revised manuscript

Reviewer's comment: – Since Fig. 12 shows the ensemble mean of projected changes in primary production but not the ensemble spread, it is not clear if there is a consistent response among the models or if the reduced change in future primary production is the results of contrasting responses among them.

Response: The changes presented are the result of a sensitivity experiment considering only atmospheric drivers and neglecting the impact of oceanic changes. The changes are overall small in regional scenarios compared to the combined impacts, we therefore selected only the mean and did not present the spread of this sensitivity ensemble. However, the reviewer is right and the question he raised is a valid one, which we will address in the revised version of the manuscript.

Reviewer's comment: – There is not enough information given in the text or caption of Fig. 13 for the readers to know what is being presented and discussed in this section. What is trophic amplification? How were bottom-up versus top-down controlled responses evaluated?

Response: The concept is indeed too briefly introduced, we just referred to the original papers introducing the concept of trophic amplification. We will explain the concept in

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more detail in the revision.

Reviewer's comment: – The bottom half of the summary and conclusions section is misplaced since it is mostly a discussion of the technique used in the downscaling.

Response: The lower part of the summary and conclusion section is an important general discussion of downscaling methods, rather than a specific discussion of a certain technique. It highlights general aspects, limitations and future developments, which should be discussed to frame the results of the study. We agree it appears misplaced as part of a summary and conclusion section and we will revise the structure accordingly.

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Interactive comment on Biogeosciences Discuss., 12, 12229, 2015.

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