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Interactive comment on "Heterogeneity of impacts of high CO_2 on the North Western European Shelf" *by* Y. Artioli et al.

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First of all, we want to thank the two anonymous reviewers for the detailed review and the helpful comments that will improve the quality of the paper and in particular its clarity.

Before going in the details of the answers to reviewers, we believe it is helpful to clarify the aim of the paper: we want to study the variability of the impact og high CO2 (i.e. climate change and Ocean Acidification (OA) together) on the carbonate chemistry, primary and secondary production in the North Western European Shelf (NWES) and to test the hypothesis that the potential feedback of OA on netPP can significantly modify the impact of climate change on primary and secondary production. It is not our intention to give a full description of all the impacts of climate change and OA

on the NWES ecosystem, as this will require a more extensive analysis impossible to summarize in a single scientific paper.

Here, we clustered together the more important criticisms/suggestions in three main groups. The first one includes the request for supplementary analysis of the model outputs and additional explanation regarding the model or our interpretation of the outcomes. In the second one we will respond directly to a specific suggestion of the second reviewer to remove one of the scenarios from the paper. The third one will show how we intend to better organize the paper to increase its clarity.

1. Further analysis/information requested:

Both reviewers requested additional plots in order to compare the differences between runs of few other variables: T, mixing, nutrient supply, saturation state and vertical structure of pH.

The first three variables have been already published and analysed with high detail in Holt et al, 2012a, as cited in the paper. We believe that it is not necessary to reproduce the same set of pictures/analyses in this paper, considering that those have already been published and discussed with more details than we could do here. Nevertheless, we will add a paragraph in order to summarize those outputs and make the link between the two papers clearer.

Regarding the carbonate system variables, we will add a summer transect of pH in order to show the vertical gradient mentioned in the text. For the aragonite saturation state, we agree with the first reviewer that showing also a map of the decrease of saturation state could be informative, but we also believe that it is sensible to limit the number of figures in order not to overload the manuscript. In this particular case, we believe that the map with the absolute value in the future scenarios is more informative, because it highlights if, where and when undersaturation could occur, which is arguably more critical than the difference of the saturation state (omega). Calcifying organism, indeed, seems to be more affected by low absolute value of omega than by big drop in this variable (e.g. a drop of 1 unit from 1.5 to 0.5 is potentially much more disrupting than a drop of 3 units from 6 to 3). Moreover, as shown in the figure below, the difference between the pattern of delta-omega and the one of delta-pH are not huge. Therefore, we will add a paragraph in the text summarizing the range of omega in the present day run and the delta-omega, in order to give a term of comparison. We can eventually add this figure as supplementary information, if editor and/or reviewers believe that is a critical missing point.

Both reviewers also asked to give more information about the freshwater balance/riverine input. This information will be added in the final manuscript in the form of a paragraph, summarising the changes in salinity at the boundaries, precipitation and river flows.

The second reviewer requested more detailed information on the parameterisation related to Temperature, mixing and nutrient. These parameterisations are as used in the standard POLCOMS and ERSEM models, and they have been already published extensively in the past (see references in the manuscript). For this reason we decided to explicitly write only the equations that have been changed in this particular implementation (in its structure or in their parameters, see sections 2.1 and 2.2), and to cite the publications where the two models are described in more details. We believe that this strategy is the best one in order to avoid overwhelming the reader with standard heat transfer / nutrient cycling equations that are not the focus of this paper and have been already intensively published.

Regarding the sensitivity analysis of the Cenh factor to the empirically derived constant, we agree with the second reviewer that this would be important information. Due to the lack of quantitative information from the PEECE mesocosm experiment regarding the uncertainty related to that parameterisation and the computational cost of a rigorous sensitivity analysis on a 3D domain, we did not perform any rigorous sensitivity analysis. In order to answer to the reviewer, we propose to run the model on a 1D set up for the A1B,nit scenario and for the A1Bnit,PP scenario with different values of that

constant (ranging from 0.00025 to 0.00075, i.e. $\pm$50%) in order to test the sensitivity. The 1D model will use the same atmospheric forcing and initial conditions as in the 3D run. We will describe in words how the netPP and zooplankton biomass are sensitive to changes in Cenh.

The second reviewer is also not persuaded by our explanation of the mechanism why in spring (April to June) in the Western Approaches (i.e. west of Ireland) we observe a decrease in net PP and an increase of total zooplankton biomass (p9398, line 6-9). We agree with the reviewer that the shift in zooplankton community cannot be the cause of an increase in the Total zooplankton biomass, but this is true only if we consider the annual mean value, but: on an annual basis indeed the total zooplankton biomass is decreasing in that area. Nevertheless, changes in community structure resulting from changes in phenology can lead to a temporary increase in zooplankton biomass. The plots attached here (figure C2) support our original explanation: they show difference between climatological variables in the scenario A1Bnit and PDnit in a location in the middle of the area discussed in those lines (53.5N, 14W). The plot on the left shows the difference of net PP (blue line) and total Zooplankton biomass (red line) and support the lag mechanism we used to explain the decoupling among those two variables. Here it is possible to see how the increase in net PP in April is followed by the increase in zooplankton biomass with a lag of one month. The increase in net PP is then compensated by a much larger decrease in net PP in May and June and therefore in fig 3b a net decrease in net PP is highlighted. The right plot shows the difference in zooplankton biomass split in the 3 different groups. Here it is possible to see that mesozooplankton is indeed the group experiencing the major increase in biomass. The big increase in mesozooplankton occurs in June, one month after the big decrease in net PP and the big increase in microzooplankton and HNAN, that are two important sources of carbon (see table 5 in Blackford, 2004).

We will expand our statements in order to better explain these mechanisms, but we believe that for the aim of this paper it is not beneficial to explain this in detail adding the figures attached here: phenology is indeed another ecosystem property that will be significantly impacted by climate change and OA and this will require a long discussion on a whole new set of variables and issues that would divert the focus of this manuscript.

Finally, the second reviewer highlights that given the high spatial variability, the global mean over the whole domain in table 1 are too simplistic. We agree with the reviewer on the importance of the heterogeneity, and our original choice was mostly dictated by the balance between the detailed representation of the heterogeneity and the clarity of the message. Considering that we will remove one scenario (see next comment), we can now go a bit more in the detail regarding the spatial variability of the shift in community composition and therefore we will split table 1 regionally (open ocean, English Channel, Celtic Sea, Irish Sea and North Sea) and change the discussion accordingly. Such a way the table will become too crowded (280 cells) to be communicative, therefore we plan to change to substitute the table with a figure, similar to figure C3.

2. Removal of the A1Bnonit scenario:

Reviewer #2 suggested removing from the paper the analysis related to the scenario with nitrification rate not depending from pH. He states that the paper does not have a strong background on nitrification, nor the parameterisation seems to be particularly sensitive to changes in NH4:DIN ratio. We agree with the reviewer that indeed the parameterisation of the ecosystem model is not sensitive to that ratio, particularly regarding the impact on PP, and this could be indeed a limit of the parameterisation used. We also agree that the paper contains strong messages even without this scenario that was more like an addendum to the main ones. Therefore we agree to remove all the part related to the NH4:DIN variables and the 4th scenario. As a consequence, we also renamed all the scenario because the specification about the nitrification could be now confusing: PDnit will become PD, A1Bnit will become A1B, and finally A1Bnit,PP will be changed in A1BPP

3. Structure of the paper:

A lot of comments, particularly from reviewer 1, suggested how to improve the structure of the paper to increase the clarity and give more details. We agree with most of these, and they will be included in the final version of the paper, here we summarize how we intend to implement these or the reasons why we believe it's better not.

INTRODUCTION:

We agree with all suggestion here, we will shorten the introduction related to OA (keeping references and the comment on the importance of interactions between stressor, being the major focus of the paper). We will also add some more comments related to the importance of climate change in the shelf environment and state more clearly the aim of the paper

MATERIAL and METHODS:

We will add all the extra information requested, although we will not create a separate section regarding the study area but we will incorporate this in the section 2.1.

RESULTS:

We will surely extend the explanation of the pattern shown by model results, because clearly our desire of synthesis did not help the clarity of the paper. In particular in section 3.2, where the role of riverine discharge was not clearly explained (rivers are not the primary cause of difference in net PP, due to the enhanced photosynthetic rate, but they allow the phytoplankton to fully express the enhanced rate thanks to the larger amount of nutrients they discharge in coastal areas).

DISCUSSION

We will make this section clearer, adding references to the plots and tables where needed in order to highlight the supporting information for our statements, and more organized, splitting in 2 subsections: impacts on carbonate system, impact on low trophic levels. Such a way we will be able to discuss more clearly the variability of the combined impact of climate change and OA, and to highlight how the potential impact of OA on netPP can alter significantly the projected impact of climate change on the ecosystem. We believe that the suggestion given by the first reviewer for the organization of this section is not the best one for this paper. In order to fully disentangle the OA impact from the climate change scenario we should run a future climate simulation keeping atmospheric CO2 constant to present day value, in order to check the impact of the solubility pump alone, and one changing only the atmospheric CO2, in order to assess the impact of the increased gradient. This does not entirely fall in the scope of this paper, as defined here in the introduction (and in the new version of the manuscript). Here, we aim to show how in the North Western European Shelf (and potentially in Shelf Seas in general) the variability of the impact of high CO2 (i.e. climate change and OA together, as they will occur at the same time) is high and therefore this impact cannot be summarized by single, simplistic numbers (e.g. pH will decrease of 0.3 units – see last paragraph of the conclusions). At the same time, to test the hypothesis that the OA feedbacks on PP could significantly alter the projected changes in production we compared the two scenarios where PP is affected only by climate change and the one where PP is affected by climate change and OA: here this approach has been used because the impact of OA on PP is still highly uncertain, therefore it is more reasonable to separate the more certain impact (climate change) from the less certain (OA). Such a way we also highlight the strong need for more precise parameterisation given that OA could impact PP as much as climate change.

CONCLUSION

We agree with the first reviewer that the last 5 paragraph of the Discussion contains some of the key messages of the paper (together with some more in-depth discussion about them), and those are indeed briefly summarized in the conclusions. Moreover, we believe that implications, e.g. the need for multi-driver/ecosystem approach or the need for including variability in future experiments, are important mes-

sages/conclusions arising from this study as well and therefore we will keep these comments in this section. In order to increase clarity, we will reorganize the section in a series of bullet points or small paragraphs.

4. Minor technical points: We thank the reviewers for the detailed set of comments: we will implement those in the revised version of the manuscript.

CAPTIONS for figure attached to this comment:

Fig. C1. Impacts of climate change and OA in the carbonate system as projected by the A1Bnit scenario: on the left (A) absolute difference in surface pH compared to the present day scenario, on the centre (B) future surface saturation state of aragonite and on the right (C) the absolute difference in surface saturation state of aragonite

Figure C2: left: simulated seasonal cycle of netPP and Zooplankton biomass at 53.5N, 14W: the inertia of zooplankton to respond to increase in net PP leads to the contrasting situation of having a global summer (AMJ) decrease in netPP and an increase in zooplankton biomass. Furthermore the big negative peak in zooplankton biomass is not present due to compensating effect among the different zooplankton types (on the right)

FigC3. Seasonal mean in 5 different areas of the difference between the A1Bnit scenario and the PD (A1Bnit-PDnit) and the enhanced production run and the A1Bnit scenarios (A1Bnit,PP-A1Bnit) for primary production split into PFT and for the zooplankton biomass. Both data are depth integrated.

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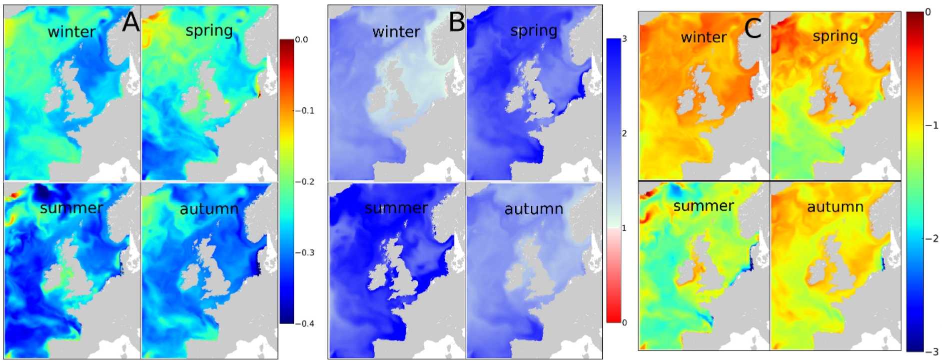

Fig. 1. As fig1 but with added delta saturation state(C): patterns in C are similar to pattern in A - see captions in the comments for details

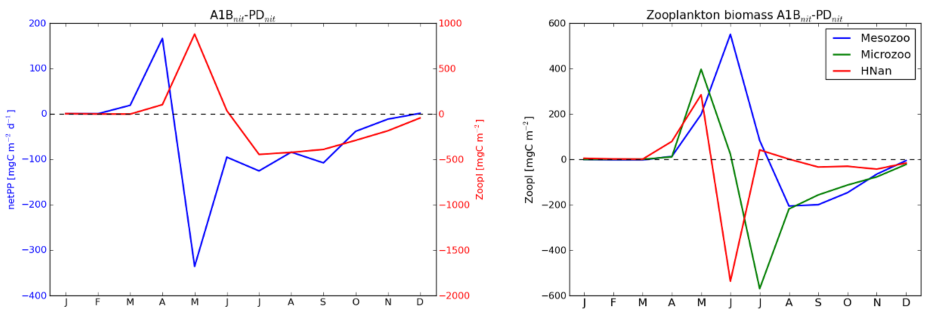

Fig. 2. average seasonal cycle of netPP and zooplankton biomass (total and split by groups) in western approach - see captions in the comments for details

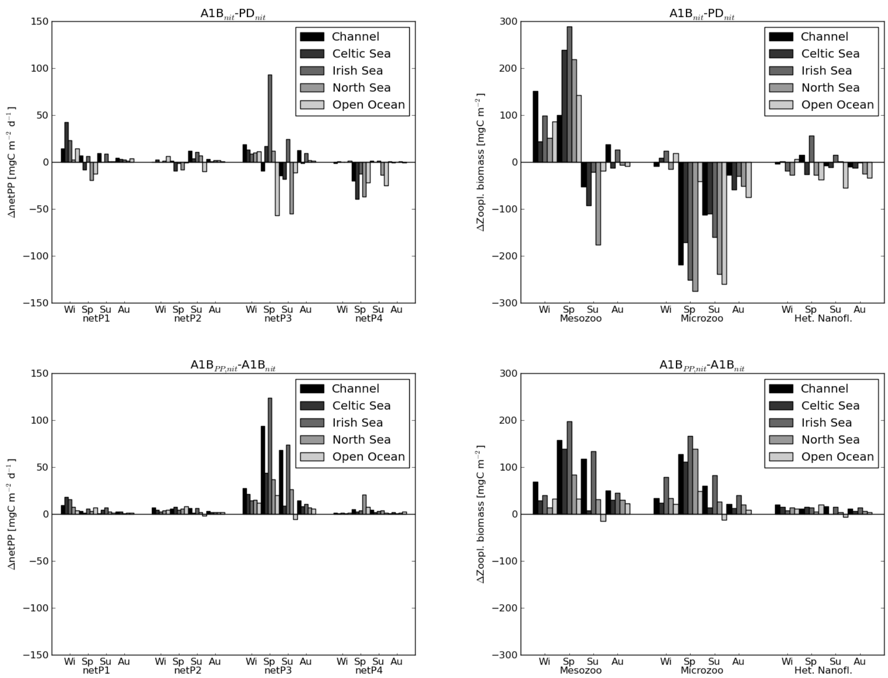

Fig. 3. to substitute table 1 - see captions in the comments for details