Response to reviewer 2

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1 General response

We thank reviewer two for his detailed and constructive comments on our manuscript. We agree with the reviewer that there are many inherent limitations in this type of modelling, and have reworded our results to reflect this, mainly in the discussions and conclusions sections. We have also reworded our title, taking into account the these considerations as well as the on-line comment left by Dr. Gattuso: ”Potential future fisheries yields in shelf waters: a model study of the effects of climate change and ocean acidification”. A detailed response to specific comments is given below:

- *First, direct impacts of OA on fishes and invertebrate can be various, interactions among these will be numerous, and uncertainties are still high. Authors choice to simulate direct impact of OA with a decreased in growth of detritivores is surely a sound assumption, based on several observations and related meta-analysis, but its just one of many possible assumptions. Reduced growth of calcifying detritivores could be compensated by increased growth of non-calcifying detritivores since the reduction of interspecific competition. Many studies also highlight potential direct impact of OA on pelagic fishes (e.g. otolith development, metabolic cost, reproduction success, behavioural response to cues) but authors do not account for these (and not even discuss those) and this could significantly affect the pelagic predator community, and therefore fishery yields.*

  Additional text has been added to the introduction and conclusion section justifying the selection of the processes emulated in this study and recognising the implications of choosing different acidification affects to emulate in the model, as indicated by the reviewer.

- *Secondly, the work is based on the implementation of 1D models that, by nature, do not include lateral advection. Authors are transparent on this limitation (see beginning of section 6), however they just mention this without discussing what are the consequences. The North Sea is indeed heavily influenced by the oceanic input, particularly regarding to nutrient inputs (e.g. Vermaat et al., Estuarine Coastal and Shelf Science,* *2008). CC projections from recent IPCC scenarios project an increase in stratification in the North Atlantic with consequent decrease surface nutrients (e.g. Steinacher et al, Biogeosciences, 2010, and more generally AR4 and AR5 reports), and this could impact significantly the North Sea, particularly the central and Northern part (ND and OG see Holt et al., Biogeosciences, 2012). Given the monodimensionality of the study, authors do not consider such reduction of nutrient input with the oceanic waters and focus only on the local dynamic. This could potentially lead to an overestimation of the temperature effect that could be significantly changed (e.g. change the sign of CC impact) when nutrient reduction is considered.*

We agree with the reviewer that changes in future nutrient supply could impact heavily on the results discussed here. However, in order to disentangle effects of different pressures with uncertain levels of impact we have elected to restrict the study to two stressors. As we consider three different locations and impacts on both the abiotic and biotic level on three different magnitudes we have performed 33 simulations in total (1 reference run and 10 scenario runs per site). We hope that this work will help interpret future studies which also take into account the effect of nutrients. However, future nutrient scenario's will necessarily be tentative particularly for land-based sources. North Sea circulation patterns cause offshore transport of land-derived nutrients: the peer-reviewed science report by Painting et al. (2013) shows that both the SB site and the OG site are within the influence zone of riverine nutrients (their figure 4). We have therefore added the following line to the discussion: ”The bottom-up stressor of nutrient supply should also be considered in future studies, as changes in nutrient availability can change lower trophic level dynamics considerably. However, for the North Sea future changes in nutrient supply should take into account changes in Atlantic sources (Holt et al, 2012) as well as land-based sources (Painting et al,2013, their figure 4).”.

Concerning the effect of 3D processes we have added the following lines to the discussion: ”Advective processes and oceanic changes have the potential to outweight local response. This applies mainly to ocean acidification impacts, as climatic impacts are mainly a direct response to local meteorology.”.

2 Detailed response

Section 2.3 *coupling between ERSEM-BFM and the HTL model is achieved via the biomass of some of the planktonic biomasses (diatoms, flagellates, picophytoplankton, microzooplankton and heterotrophic nanoflagellates). Authors state that ERSEMBFM has more planktonic groups than these ones (dinoflagellates, phaeocystis, small diatoms and 2 groups of mesozooplankton) but its not clear why these biomassess are not used to couple the HTL model.*

Dinoflagellates and *Phaeocystis* were not included as these functional groups are predominantly inedible to other species. Mesozooplankton groups were not included as these overlap in size with the start of the pelagic predator size spectrum: this issue is currently being addressed in futher development of the coupled model. Small diatoms were not included initially, as they represented a new functional group within ERSEM-BFM with very limited occurrence (early spring and in small numbers). This group has been included since and comparison studies showed no significant difference due to their small contribution. Subsequent model development on ERSEM-BFM has seen this group transformed into resuspended benthic diatoms with a more defined impact on plankton biomass levels. We have added the following to section 2.3:

"Note that dinoflagellates and *Phaeocystis* were not used as food as these are predominantly inedible. Mesozooplankton was not included as food as it overlaps with the start of the pelagic predator size spectrum."

Section 2.6 *authors assume a decreasing growth of 2, 6 and 10%, with those numbers coming from a combination of impact of OA on growth of calcifying organisms and percentage of calcifiers in the detritivores community. Even though it would not make any different from a modelling point of view, it would be helpful to disentangle the impact of OA from the community composition effect, in order to better contextualise the study (e.g. is the impact small/large because the simulated community has less/more calcifiers or because of OA?). Furthermore, do the three thresholds have been chosen by authors on the basis of useful range to test model sensitivity, or on the basis of experimental data? To my knowledge the two papers cited do not offer estimates of the decrease of calcifiers growth.*

Doney et al. (2009) does document studies where decreased calcification has been observed in bivalves (10 to 25 %) so this range could be used to set the sensitivity range to be tested, although somewhat precautionary. A review by **?** stated "under short-term experimentally enhanced CO_2 conditions, many organisms have shown trade-offs in their physiological responses, such as reductions in calcification rate and reproductive output", but some of the detritivore/deposit feeder acidification work is not consistent between or within macrofaunal groups as **?** also discuss. The limits were therefore set in terms of a sensitivity test using the available experimental data, in a conservative fashion and relevant to the detritivores present in the model version. The paper by Andersson and Mackenzie (2011) may be less relevant but has a discussion on infaunal calcification changes and the complexity of this issue, and therefore we included it. We have now added the **?** reference to this section.

Regarding community structure, we agree with the reviewer that this will be important. But without specific representations of calcifiers in the model it is beyond the scope of this study to quantify this effect. In contrast, the lack of community structure information allows us to make predictions for larger areas. Local community information can then be used together with results presented here to indicate possible local response to future pressures. We have added the following line to section 2.6 to highlight this issue: "Note that community structure observations would be necessary to interpret localised effects of reduced detritivore growth rate.".

Section 3.1 *Authors suggest that organisms adapted to high Ammonium/lower nitrate regime induced by OA: how the model can show organism adaptation? Surely it cannot be evolutionary adaptation, as parameters in ERSEM-BFM are, to my knowledge, static. What is the trait/processes that changed (adapted)? And how? (see also comment on 3.3)*

Indeed adaptation is not possible with the ERSEM-BFM model. But functional groups have the option to use ammonium or nitrate to satisfy their internal nitrogen demand, with each functional group displaying different affinity to the different nutrients. With the reduction of pelagic nitrification the supply of pelagic nitrate decreased while that of pelagic ammonium increased. This led organisms to take up more ammonium compared to nitrate than in the unperturbed scenario. In the text we have changed the word ”adapted” to ”reverted” to clarify this.

Section 3.2 *authors state that CC will impact more the benthic system (with high increase in benthic detritus and decrease in biomass): why? What are the fluxes simulated by the model that lead to that result? Why growth decreases despite the increase in T?*

The future climatic conditions at the Oyster Grounds location are characterised by a reduction of wind speeds in spring, leading to less suspended particulate matter in the water column. This triggers an earlier start of the spring bloom, but as onset of stratification does not change significantly (a trigger for diatom sinking) there is a strong reduction in diatom levels near the sea bed (Van der Molen et al., 2013). As this is the main food source for filter feeders, and the most important carbon pathway into the benthic system, there is an equivalent reduction in benthic biomass in the lower trophic level model (the more noticeable as benthic biomass is larger than pelagic biomass at this site). With less benthic biomass there is less uptake of benthic POC and thus an increase in benthic POC levels despite a decrease in pelagic POC levels and increased benthic metabolic rates. We have added the following line to this section to clarify this site-specific response:

”Onset of stratification (a trigger for diatom sinking) did not change significantly at this site, leading to a longer period of suspended diatoms and a reduction in near-bed diatom levels during spring. This led to a decline in filter feeder biomass (due to a reduction of the main food supply) and a subsequent reduction in other benthic functional groups (as pelagic-feeding filter feeders form the main carbon pathway into the benthic system in the model), see Van der Molen et al. (2013). As a result, benthic POC levels increased.”

Section 3.3 *authors state that reduction in nitrification rate favour plankton with high ammonium preference (picophytoplankton and dinoflagellates). Why this is not seen in the other two test cases? From the paper, it seems that the set of parameter for ERSEM-BFM does not change across the sites, therefore those groups should have higher affinity for ammonium also in the other test cases but in ND the impact is null, while in OG is somehow similar* *to this case for dinoflagellates (even though authors state that is minor in that case and they do not discuss it section 3.2). So whats the mechanism behind the increased biomass of picophytoplankton and dinoflagellate? Is difference in nitrogen speciation, or a some other bottom-up process (e.g. less diatoms in the spring blooms could leave more nutrient available for following blooms) or top down control (e.g. change in the spring bloom could change zooplankton community and biomass and therefore relieve later bloom from some grazing pressure). Looking at the nutrient uptake/grazing fluxes and/or nutrient availability estimated by the models could help in supporting either of the hypotheses.*

The author is correct in assuming the parameters are predominantly the same for each site. The response is therefore also visible for the other two sites (see renewed figures for phytoplankton percentage change with adjusted y axis): percentage change for picophytoplankton and dinoflagellates are 2 % and 4 % for the SB site, 2 % and 13 % for the OG site and 2 % and 0 % for the ND site (no dinoflagellates present). The governing mechanisms differ for each site:

At the SB site picophytoplankton benefit in summer from higher ammonium levels and decreased predation (increased levels of omnivorous mesozooplankton cause lower levels of microzooplankton). As the system is nitrate-depleted during this period they outcompete the flagellates due to their ammonium preference. Dinoflagellates also benefit from this in autumn, and experience no grazing pressure as they are inedible.

The OG site also experiences higher ammonium levels but as this site stratifies in summer it does not become nitrate deplete. Grazing pressure for picophytoplankton remains similar and increased levels of picophytoplankton mainly occur during the spring bloom, not in summer. Dinoflagellates increase during the autumn bloom due to less competition from *Phaeocystis* colonies.

The ND site shows a high increase in ammonium levels in early summer, higher than the general increase throughout the year. Picophytoplankton increases during the spring bloom and summer and levels are higher than those for the other two sites, resulting in a smaller percentage change. They also experience increased grazing pressure during this period. Dinoflagellates decrease due to acidification impacts at current conditions but are quickly wiped out by climate change effects in all tested scenario's.

So the reviewer is right in stating that other factors might be important at the different sites. We have added text to each section to highlight this difference.

Section 4.2 *as fish were more dependent on the detritivore food source could author provide some comparative estimates of the trophic fluxes across the groups on the different sites? This could help to understand at which level of connectivity across groups this mechanism become important.*

To visualise this dependence we have included figures for each site of predation mortality biomass for pelagic predators and detritivores, showing changes in feeding strategy.

The new figures are also discussed in the text in the relevant sections. We have now also included the relevant figures for the separate pressure scenario's, i.e. ocean acidification impact only on lower trophic levels (reduction of pelagic nitrification rate), and ocean acidification impact on higher trophic levels only (reduction of detritivore growth rate). these new figures have been included in the supplementary materials.

Finally *the introduction is not giving an adequate representation of the literature of OA impacts both on biogeochemistry/low trophic levels as well as invertebrate, fish. Although a comprehensive review of OA impact is clearly not the aim of the paper, nor of the introduction, a quick glance of the variety of way on how OA impact on both part of the marine ecosystem citing a series of papers would help those readers not fully aware of the OA topic to put this study in the context and better understand its findings. Here a non-exhaustive and non-compulsory list of suggestions of impact and papers that could help in giving this context:*

- *impacts of OA on Primary producers: Riebesell and Tortell, chapter 6 of Ocean acidification, Gattuso and Hansson eds.; Tagliabue et al., Global Biogeochemical cycles, 2011; Engel et al., Biogeosciences, 2013; Schulz et al., Biogeosciences, 2013; Artioli et al., Biogeosciences, 2014; Taucher et al., L&O, 2015*
- *impacts of OA on benthic detritivores (or more generally benthic fauna): Andersson et al., and Widdicombe et al., chapter 7 and 9 of Ocean acidification, Gattuso and Hansson eds.; Hale et al., Oikos, 2011; Kroeker et al., Global Change Biology, 2013; Wittman and Porter, Nature Climate Change, 2013*
- *impacts of OA on fishes: Porter et al, chapter 8 of Ocean acidification, Gattuso and Hansson eds.; Kroeker et al., Global Change Biology, 2013; Munday et al., Nature Climate Change, 2014; Simpson et al., Biology letters, 2011 C3929*

We thank the reviewer for pointing out more relevant work, not all of which was mentioned previously. Additional text has been added to the introduction to briefly comment on the range of acidification effects that have been identified by the reviewer and references are provided for some of the main reviews that will allow readers to access the ocean acidification literature. There have been many copious reviews of acidification effects: this paper is deliberately seeking to avoid providing yet another review of them.

3 Minor issues

- section 2.4 *which ERSEM-BFM parameters have been calibrated using fish size spectra data? And what is the final value? Why calibrate ERSEM-BFM with fish data instead of calibrating the size spectra model?*

Within the coupled model the ERSEM-BFM lower trophic level model provides detritus time series for the higher trophic level model. This supply was multiplied by a calibration factor before becoming available as food in the size-spectrum model, as detritus supply to the bed was underestimated (Van der Molen et al., 2013). This factor was used to calibrate the higher trophic level model to observations from Maxwell and Jennings (2006) (detritivore observations over the size spectrum). We have changed the text to reflect this: ”Here, data from Maxwell and Jennings (2006) were used for calibration of the ERSEM-BFM near-bed detritus levels (indicated to be a problem by Van der Molen et al. (2013)) as supplied to size-spectrum model, while the data from Jennings et al. (2002) was used for validation. Calibration factors were 25, 2.5 and 5 for the ND, OG and SB sites respectively”. No parameters within ERSEM-BFM were calibrated for this study.

section 2.4 *authors rightly state that correlation between simulation of fish biomasses and observed data is high, but they dont discuss the high difference in variability (standard deviation): detritivores in all sites have a variability about 60% to 70% higher than the data, while predators about 40% lower. Is that due to higher/lower seasonal cycle? Being so consistent across sites, does this suggest a limit of the model?*

We agree with the reviewer that the difference in variability between predators and detritivores can indicate a model limitation. However, it can also relate to the limitations of the observational data used, which was obtained from only 1 study (Maxwell and Jennings, 2006) which did not cover all seasons or all represented detritivore groups. Therefore we think we cannot meaningfully comment on model limitations based on such limited data, but we have highlighted the issue in this section by including the following sentence:

”Note that the large difference in variability between predators and detritivores can indicate both a limitation of the model system (lacking stabilising processes for detritivores or benthic POC supply) or a limitation of the observational data applied (covering only one spring and one autumn cruise and considering only infaunal predators).”.

section 2.5 *I suggest to move the description of the sites earlier in the text, so the readers will know the characteristics of the sites before reading details on validation in section 2.4*

Done. The section has been placed after model description but before model validation.

section 2.6 *authors refer to future conditions to the period 1958-2089. Clearly this run does not represent only future condition, but it is a transient run forced by climatic forcing (HADRM3) instead that by reanlaysis forcing (ECMWF). Therefore, I would suggest as more appropriate names reference (or reanalsyis or hindcast) for the ECMWF forced run, and climate or transient for the HADRM3 forced one.*

The reviewer is right and we have changed the names of the different simulations, with ”reference simulation” now referring to the ECMWF-forced run and ”climate change simulation” referring to the HADRM3-forced run in section 2.6. To reflect this we have changed the header in tables 4-6 to have ”actual” values rather than ”reference” values as these correspond to the climate change simulation and not the ECMWF-forced simulation.

section 6 *authors state that 3D models, contrarily to 1D models, lacks of specific local parameters (e.g. bed composition or sediments properties). Although I generally agree with the authors that medium-coarse resolution models can neglect local specificity, and that high resolution 3D models are costly, its not clear which specific local parameters in this 1D implementation that couldnt be included in a 3D model and that improved the results.*

There are no parameters or processes included in the 1DV model that are not included in the 3D version of the same code, and we apologize for suggesting this in the text. What we meant was that local parameter calibration is possible in a 1DV setting whereas the same parameter can have a spatially constant value in a 3D model. We have changed the text here to better reflect our meaning:

”The use of 3D models adds advective processes and far-field influences, but generally lacks specific local parameter settings (here bed porosity and increased vertical resolution). As such, the two approaches are complementary. The less computationally expensive water column model also allows for many scenario simulations to be performed within a reasonable time frame, and is therefore very suitable for scenario studies including different pressures.”

table 3 *I assume that the repetition of the first row is a mistake*

Indeed, and this has now been corrected.

tables 4-6 *it would be interesting to highlight which changes are statistically significant (any simple significance test would do, t-test or Kurskal-Wallis). Furthermore, I would suggest authors to write in the caption that changes shown here are 2069-2098 vs 1979-2009*

We have added the requested time indication to the captions. We have not added any statistical tests to show significance as we only present a subset of variables here from a very complex model system. We also feel it would distract from the overall goal of presenting long-term change as any statistical analysis would have to be performed on the original time series (or at the least annual time series), including seasonal, interannual and decadal effects.

figure 2 *there is no legend for the white areas in the domain*

We apologise for this omission and have added the following line to the caption:

”The white areas represent transitional waters, which experience large variability in duration of mixed and stratified conditions, defying classification.”

figure 3,4,5 *similarly to table 4,5,6, I would specify in the caption the two time horizons used to calculate the data shown in the bar plot. More importantly, why outputs from ERSEM-BFM* *are shown with bar plot while outputs from the size spectra models are shown by time series? My understanding is that both models have been run for the same period 1958-2089, so the results could be shown in the same way to better understand the dynamics. Furthermore, since authors have run the models for the full period, why showing the outcome averaged by 30 years? In my opinion, such a way authors reduce significantly the power of their work, flattening all variability, masking non-linearities and limiting the ability to highlight and understand interacting mechanisms. If authors decide to keep the 4 timeslices approach to show their results, I would suggest to remove the lines among the dots (or maybe choose bar plots) to avoid suggesting (unlikely) linear trends across 30 years average.*

We agree with the reviewer that the 30-year averages reduce variability, but this was precisely the aim: with such complex models we wanted to focus on the long-term changes which can be obscured by interannual and decadal variability within the system. The full results were used in the analysis of the work when necessary, but with the large number of state variables and the many different scenario's we have opted to focus on the long-term changes only.

Concerning the type of plots used we have elected to present the majority of the work in line plots, as they contain more information. Bar plots showing percentage change were only applied to functional groups as actual values can vary widely between groups and thus obscure change if presented in one graph. Thus we have decided to stick with the current graph styles: the new figures added which relate to the higher trophic level model are therefore also line plots. We have adjusted the scale on some of the bar plots to better show change.

References

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