

Response to reviewer 1

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lead author of manuscript bg-2015-196, on behalf of all co-authors

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1 General response

The authors are grateful for the detailed comments provided by Dr. Fulton, a well-known expert in end-to-end modelling. As a result the manuscript is now more descriptive of the shortcomings of the method, and contains a better explanation of the applied coupling. Detailed responses are provided below. We hope we have responded to the correct sections but could not find a version of the manuscript with continuous line numbers as used by the referee.

2 Detailed response

- 1 *Lines 95-103: It would be good to see some reflection on what the expected downsides of not having 2-way coupling are. How do the authors account for consumptive losses in the plankton due to feeding by the fish fauna? Was the level of consumption of the same order as the assumed mortality?*

Regarding 2-way coupling we have added the following line to section 6: "This would also ensure feedback of other predator changes (e.g. increased feeding rates due to increased sea temperature, more pelagic-oriented feeding) on planktonic biomass, which are not included in one-way coupling. ". We expect these impacts to be minor compared to the mortality/feeding feedback.

Regarding the mortality we have added the following lines to section 2.3: "Fish predation mortality in ERSEM-BFM is simulated as cannibalisms of the largest species, ensuring a dynamic mortality dependent on biomass. Thus both lower trophic level mortality and higher trophic level feeding were driven by lower trophic level biomass values, ensuring a proportionate response."

However, these responses do not correspond to the same functional types, as there is some overlap in species representation between the two models. This is being addressed in current development of the coupled model as two-way coupling is being implemented.

2 *Line 118: The fishing pressure that was included, what level was that pressure? Was it based on a particular year? Why was the fishing pressure assumed to be homogeneous spatially? What are the implications of the simplification?*

The applied fishing pressure was that as described in Blanchard et al. (2009), and as such took a value of 1.0 (fishing pressure switched on, pressure proportional to averaged values based on multispecies virtual population analysis ICES (2005) over 1990-2003). This pressure was assumed to be constant over the sites as lack of local observations on the food web scale prevented calibration. Nevertheless, the LTL detritus supply to the HTL model was calibrated to reflect the only available regional detritivore size spectrum observations, representing a fished environment. As such, the different calibration numbers account to some extent for different fishing pressures between the sites. We have changed the text to

”Griffith et al. (2012) showed the importance of including fishing pressure combined with acidification and temperature pressures in an Australian ecosystem. Here it was included in the model as a nominal pressure (based on ICES (2005)), but was not calibrated to represent site-specific mortalities. ”

3 *Table 4: It is interesting that few changes are >40%, but still there are many declines in catches despite quite small changes in POC, primary production and biomasses.*

This is mainly due to the limited selection of model state variables in the tables: not all mechanisms and carbon pathways are represented by them. We have now included more analysis of system response (as requested by reviewer 2), including for instance information about near-bed diatom levels: these can be crucial for carbon transport to the benthic system, but are not necessarily proportional to water-column averaged diatom levels. We have also added fish and detritivore predation mortality time series, to better indicate response mechanisms in the higher trophic levels. However, the fact remains that the higher trophic level model is considerably less complex (2 state variables) than the lower trophic level model (60+ state variables), and therefore will be more sensitive to changes in the few drivers.

4 *Line 194: The pelagic and benthic biomasses do decline, but the pelagic ones in particular are small, would they actually be detected in reality?*

As reviewer 2 points out, this study is essentially a sensitivity study into the effects of the different pressures: the main interest is in the trend of the long-term projections of stressor impact. Changes were small for lower trophic level dynamics for acidification impacts, but climatic impacts would certainly be detectable with the larger impacts on benthos (-20 %) and net primary production (+11 %).

5 *Section 3.2: While there are many declines, the changes are typically small. Again are the declines of a magnitude that they would be noticeable in reality, they seem to be*

potentially negligible relative to likely noise level?

The OG site is in "transitional waters", meaning it is characterised by large interannual variability in the governing hydrodynamics. As such, it can certainly be expected that the predicted pelagic changes are smaller than interannual variability. Nevertheless, the main objective here is to gauge the future trend due to particular pressures.

- 6 *Line 266-267: Clarify this sentence, particularly the bit in brackets on food supply it is not clear what you mean here and the logic of the steps.*

We apologise for this confusing sentence, and have now reworded the part in brackets to "increased general increase due to planktonic food supply". There is a difference in the planktonic food supply for small pelagic predators, and the food supply for larger predators (i.e. smaller fish and detritivores). We have now added new figures showing predation mortality rates for the higher trophic level model, both in the manuscript (for combined impacts) and in the new supplementary materials (for separate impacts). Together with the existing figures for planktonic food supply (adjusted figure title to reflect origin of food) we hope we have clarified this matter.

- 7 *Line 301 (section 5.2 line 24): Pelagic fish food supply increased slightly due to indirect acidification impacts (figure 4d). This should be figure 4c in which case it dipped and rebounded ...*

We have corrected the erroneous figure reference. Figure 4c indeed shows a dip and recovery, with the 2069-2098 averaged values just marginally higher than those for 1079-2008 for all scenario's. But the main point here is that acidification impacts caused fish food supply to increase compared to climatic impacts. We have reworded the line to clarify this: "Pelagic fish food supply increased slightly due to indirect acidification impacts (figure 4(c), increased values compared to climate change scenario), ...".

- 8 *Line 303: Fishing yield decreases according. I am not sure I would agree based on Figures 4e and 4f. Please be clear about whether you are talking about transients or snapshot end points.*

We have changed the text to better reflect our meaning: i.e. that the decline is for the future conditions under OA and CC pressure relative to the current state (CC simulation 1979-2008 result). The decline therefore does not refer to the temporal development of any one scenario, but to the change between the scenario's. New text included:

"Fisheries yield decreases accordingly (figure 4(e), acidification scenario's vs Temp scenario), with the main decline in the winter period (figure 4(f))."

- 9 *Line 308 (section 5.3 line 7): Detritivore growth rates remain more or less equal... I disagree, please make sure the results match the plot, which indicates an increase.*

We apologise for the confusion. Indeed the corresponding figure shows an increase in detritivore growth rates for all scenario's as a function of time: what we meant was that there is virtually no change in the growth rate between current conditions (climate change run, 1979-2008) and future conditions with both stressors (High scenario run, 2069-2098). We have changed the text to reflect this better: "Detritivore growth rates remained more or less equal when combined effects are applied (figure 5(d), High scenario 2069-2098 result compared to Temp scenario 1979-2008), ..."

- 10 *Lines 310-311: There is a decline in winter, but there is a big increase in summer, that should not be ignored.*

We have changed the text to reflect this: "Changes to fisheries yield depended strongly on the strength of acidification impacts (figure 5(e)), affecting all seasons (figure 5(f)) and showing a strong decline in winter and strong increase in summer."

- 11 *Line 331-332: Interaction effects could be explored using the Hedges d method in Griffith et al 2012. Conservation Biology 26: 1145-1152*

We agree with the reviewer that the Hedge's d method is very useful for exploring interaction effects: it was applied in Griffith et al. (2012) to disentangle effects of fishing pressure, climate change and ocean acidification in Australian marine waters. Unfortunately we do not have a reference simulation without any pressures, so cannot apply the same method here without additional effort (we only consider two pressures, with the climate change simulation acting as the benchmark to measure acidification impacts against). With 3 different sites, 2 different pressures, 2 different trophic impact levels and 3 different reduction rates we do not want to complicate the analysis further by adding an additional reference plane. In this manuscript we want to focus on the underlying processes that effect future fisheries yield in three distinct areas. However, the Hedge's d method would be very useful in future work to include additional pressures like fishing or nutrient stress. Griffith et al. (2012) also notes that variation in interaction response must be taken into account (i.e. combinations of different impact levels to gauge interaction response): this is exactly what this manuscript has tried to do with respect to ocean acidification.

- 12 *Line 364: What time scale is being considered here? When could monitoring schemes start to check for it?*

As sea surface temperatures are already increasing in the North Sea (Kühn et al., 2010; Tinker et al., 2015), with large scale regional differences (Eastern North Sea vs Western North Sea), the effect is already visible in observations. However, to discern the separate effects of acidification and changing meteorological conditions would require considerable effort, as understanding of species response to OA effects remains limited.

- 13 *Line 368: Griffith et al 2012 is an example of a paper that has already started to explore this topic.*

We sincerely apologise for not including this highly relevant paper before. We have now included references to this paper where appropriate, and have added the following paragraph to the Discussion:

”When considering multiple stressors like acidification, climate change, nutrient supply and fishing pressure the interactions of different pressures should be studied using statistical techniques such as the Hedges-d method: this was applied in Griffith et al. (2012) with respect to ocean warming, ocean acidification and fishing pressure in Australian marine waters. They showed that interactions between pressures could lead to less than or more than the additive response of the system: for instance, fishing pressure counteracted negative effects from acidification on benthic invertebrates by relieving predation pressure. Similar results were found in this study, as climate-change induced increases in biomass were counteracted by acidification impacts. Together with different impact level studies like the one presented here these methods have the potential to provide a good indication of future marine response to known pressures.”

3 Technical Comments

- 14 *Line 73: Missing a fullstop after light climate*

Corrected.

- 15 *Figure 2.1: Define ROFI. Explain what the white space in the marine sections of the plot means. Does it mean there is no clear pattern for classification (i.e. transition zone)?*

We have now included a description of the white areas and the ROFI in the caption:

”ROFI stands for Regions of Fresh Water Influence. The white areas represent transitional waters, which experience large variability in duration of mixed and stratified conditions, defying classification.”

- 16 *Table 3: It appears that the second row is a duplicate*

Yes, and this has now been removed.

- 17 *Section 3.2 and 3.3. For both of these sections (on Oyster Grounds and Southern Bight) please make it clearer when referring to acidification vs temperature effects, or combined effects.*

We have tried to make this more clear in the relevant sections. Please note that the original text was already structured to this effect: first paragraph on climate impact, second paragraph on acidification impact, third paragraph on combined impact.

18 *Line 283: Should be ... enhanced by the direct acidification...*

Corrected.

19 *Figure 3 (and Figures 4 and 5): The panels are small and hard to read. The colouring for the bar plots are not friendly for colour blind people. Could a different scale be used (or could the bar plots be broken into multiple panels) so that the temperature results do not obscure what is happening in the other cases.*

We apologise for the use of colours unsuitable for colour blind people, and have now changed the colour scheme to one more suitable. Additionally, hatchings have been applied to the bar plots for easy identification. Temperature effects have been excluded in separate plots where necessary, mainly in the new supplementary materials.

20 *Line 345: This would read more easily as ... aggravate or relieve the impacts of future pressures.*

The text has been changed accordingly.

References

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