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Interactive comment on "Future fisheries yield in shelf waters: a model study into effects of a warmer and more acidic marine environment" *by* S. M. van Leeuwen et al.

Anonymous Referee #2

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The paper presents an interesting implementation of a coupled modelling system in order to assess impact of climate change (CC) and ocean acidification (OA) on North Sea ecosystem, on the low trophic level (LTL) looking at planktonic primary production and high trophic level (HTL) looking at changes in biomass of pelagic fishes and detritivores. Authors also highlight potential impact on fishery.

The subject is highly important, and surely relevant to Biogeosciences. Authors draw strong conclusions on the impact of CC and OA on fishery harvests that are crucial for understanding the evolution of marine ecosystem and the sustainable exploitation of these. The work is generally sound, however in my opinion the limitations inherent to the methodology would suggest to be more cautious in the conclusions and

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to present the results more as a sensitivity study than “an indication of future trends in fisheries harvests” (p9711 l12). Authors partly acknowledge this in the discussion (p9710 l20-25), but conclusions are phrased in a way that in my opinion would require a level of certainty higher than the one underpinning this study (e.g. “fisheries yield expected to increase”, “Fish yield is predicted to decline, particularly in winter months”, “Changes in fish yield are equally distributed over the seasons when impacts are of similar strength”). For this reason I would also suggest authors to change the title to highlight that the work shows sensitivity of fisheries yield to CC and OA more than future predictions.

On top of the limitations authors discussed in section 6, I would suggest authors to discuss these other two.

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 it’s “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.

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

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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.

All these caveats do not undermine the scientific value of the work presented here: it is a very useful work that allow to disentangle the influence of the external environment from the local processes focussing on the latter ones, and to better understand the local interaction of CC and OA. While the interacting mechanisms authors highlight are likely to happen in the future, the final estimates of the trends could be significantly biased by this simplification.

The models used in this paper are robust and already been used for similar scope either separately or jointly as here. However, authors do not fully explain or justify all the assumptions they take. In particular:

- 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 ERSEM – BFM has more planktonic groups than these ones (dinoflagellates, phaeocystis, small diatoms and 2 groups of mesozooplankton) but it's not clear why these biomassess are not used to couple the HTL model.

- 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 detritivore's 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.

Interpretation of the model results are sometimes quite speculative, authors could better exploit model capabilities in order to support their interpretation. For instance:

- 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 EREM-BFM are, to my knowledge, static. What is the trait/processes that changed (adapted)? And how? (see also comment on 3.3)

- 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?

- 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 what's 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

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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.

- 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.

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

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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?

- again on section 2.4: authors rightly state that correlation between simulation of fish biomasses and observed data is high, but they don't 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?

- 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

- 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.

- 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, it's not clear which specific local parameters in this 1D implementation that couldn't be included in a 3D model and that improved the results.

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

- tables 4, 5 and 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

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

- 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.

Interactive comment on Biogeosciences Discuss., 12, 9695, 2015.