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Biogeosciences Discussions

Interactive comment on "Putative fishery-induced changes in biomass and population size structures of demersal deep-sea fishes in ICES Sub-area VII, North East Atlantic Ocean" *by* J. A. Godbold et al.

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Response to Referees comments: Godbold et al. "Putative fishery-induced changes in biomass and population size structures of demersal deep-sea fishes in ICES Sub-area VII, North East Atlantic Ocean"

We thank all three referees for their strong support of our paper and the helpful suggestions they have made, particularly in terms of the Discussion, which are "not fundamental" and have further improved the manuscript. We have amended the manuscript

to include the referee's suggestions and respond to individual comments below.

Referee 1:

Methods:

1) Page 10764, lines 22-24: The referee suggested that we *present the results of the length-frequency distributions of all 8 species*, rather than just representative species, as it may help to identify some of the underlying mechanisms of our observed changes in biomass. We can confirm that we have extended the statistical analyses on the length-frequency distributions to all 8 species that were analysed for biomass. We have also amended the discussion where appropriate (see Discussion points below). Inclusion of the length-frequency distributions for all 8 species has not altered our conclusions, but it has improved the manuscript and provided stronger evidence for our arguments.

Results:

2) The referee would like us to *add of a table of official ICES catches* to the manuscript, however, we do not feel that this would be a worthwhile addition to the paper as these data are available elsewhere and they would distract from the main objectives of our paper. However, the official landing information is discussed later and a source is given (ICES WGDEEP assessment).

Discussion:

3) The referee suggests that the *removal of predators and a decrease in intra- and interspecific competition increasing availability of food may be more important for the patterns observed in N. aequalis, H. macrochir, S. kaupii, L. eques and A. rostrata*. We already speculate on the likely role of reductions in competition for the observed changes in biomass for some species (e.g. *C. armatus*). For *H. macrochir* and *N. aequalis* we also suggest that the observed changes in biomass may be due to density dependent growth for which the mechanism may be a reduction in intra-specific

competition increasing food availability as the referee suggests, however further research is needed to truly elucidate the factors controlling the change in biomass for each species. In addition the discussion has been amended to reflect the possibility of such mechanisms for the patterns observed in *S. kaupii*. For *L. eques* we see a significant reduction in the size-frequency distribution in the late period relative to the early period across its depth range, but no reduction in biomass (or abundance Bailey et al. 2009) and further research is required to elucidate the exact mechanisms of these observations.

4) *C. rupestris length-frequency distribution discussion does not match results*: The referee is correct that in the discussion we described the observed patterns of the results the wrong way round and we thank him/her for highlighting this. The manuscript has been amended to reflect that we observed a decline in the number of small individuals and a shift to larger individuals in the late period (Fig. 6d).

5) *Could the observed reduction in biomass at shallower depths in C. guentheri a recruitment rather than trawling effect?* We have now included the figures and analyses on the length-frequency distribution of *C. guentheri* as the reviewer suggested, which strengthens our argument for a fishery rather than recruitment effect at the shallow end of the depth range as the referee suggests. Although the differences observed in the length-frequency distribution between the time periods for each depth interval are not significant, the data does suggest that at the shallow end of the depth range, there are a smaller number of larger individuals (>3.5 cm head length) in the late period, relative to the early period. Thus the observed reduction in biomass at shallower depths is more likely to be the results of reducing numbers of large individuals due to the fishery rather than a recruitment effect.

6) Page 10769, line 29: This omission has now been corrected

7) *The referee suggests that the trends seen for A. rostrata may be movements of the species to shallower waters as a result of the decrease of abundance of possible com-*

peting species or predators. (25 of page 10770. The referee is correct; a shallowing of the depth distribution of this species may be possible. Unfortunately, no trawls were conducted at the deeper end of the depth range of *A. rostrata* during the later period and therefore we are unable to fully test this idea. We have amended the discussion to reflect the same.

Referee 2:

1) Correction to Benn et al. (2011) paper interpretation (page 10760, line 6): This inaccuracy has now been amended to reflect the spatial extent of the various *activities* on the seabed, rather than referring to them as impacts.

2) Use of terms "significant" and "significantly" (pg. 10760, lines 7 &9): These two terms have been removed and sentences modified.

3) Addition of a couple of sentences and references referring to the importance of ICES efforts in detecting decreases CPU in the mid to late 1990s (page 10761, lines 5 – 15). We have amended this section as the referee suggested to make clearer that the earlier long-term data collected by ICES had detected the fish declines before the fisheries-independent studies were published.

4. *Statement on the possibility of fisheries induced recruitment failure unnecessary (Page 10761, line 19 – 20*): We agree with the reviewer and have removed this part of the sentence in the manuscript, as we do not have direct evidence of whether we are observing increased mortality in younger fish or a true recruitment failure as a result of the supply of settling larvae being reduced/stopped.

5) *Discuss the 'major declines' (30%) in biomass in the context of MSY targets where a 30% reduction in an exploited fish population is considered modest and acceptable.*

Whilst we agree that a 30% decline in biomass for a target species is not necessarily seen as a 'major decline' from a fisheries point of view, such declines in non-target species are not acceptable. As the total demersal fish population in the Porcupine

Seabight analysed in the present study is composed mainly of non-target species, we argue that a 30% overall decline in biomass represents a significant change in the overall biomass of the fish population.

6) *Consider natural variability in recruitment on population dynamics as a possibility alongside other possible explanations for the observed patterns*. The referee is correct that natural variability in recruitment may well explain some of the patterns observed in the present study. In the discussion we make this point for the observed patterns in the *C. armatus* population for example and hypothesise the same for *A. rostrata* later on in the discussion.

Referee 3:

1) *The fact that the deep-sea harbours high biodiversity is questionable and should be put into context* (Introduction 10759, L23). We have removed this statement from the start of the manuscript, as it is not directly relevant to our study.

2) *The authors statement that "understanding which species are most at risk remains a challenge" is questionable in times where an ecosystem based approach to management is being argued as urgent* (Page 10760, L26). The referee is correct that management has increasingly been moving towards an "ecosystem based approach". However the ecosystem approach to management does not mean that knowledge of the impacts of human activities on individual species is not called for or unnecessary. In order to effectively adopt an ecosystem approach to management knowledge of the status of individual species, which are part of the ecosystem, is of fundamental importance.

3) *The use of ">50%" to refer to a reduction in abundance is misleading* (10761, L12). We have amended the manuscript accrodingly to avoid confusion to "...reduced the abundance of target and non-target species by >50 %...".

4) What are the two periods (10761, L27)? We have added the dates at the end of

this sentence again to avoid confusion. Although we refer to them earlier on in the paragraph (10761, L23)

5) *Long term changes in biomass may have been produced by a variety of factors including fisheries. Although I believe that fisheries may have been the main driver of biomass changes the authors have no data to test the "effects of deep-water fishery" but rather only to test for changes in biomass. Simple solution would be to just add putative to the sentence as you did to the title* (10761, L28). We thank the referee for this omission and have amended the manuscript as he/she suggests.

6) *Although I understand the main goals of the paper, the authors could have explained them better and more objectively. I'd suggest the authors to state here very briefly how they will address the main objectives of the paper. I.e. by analysing research survey data? Modelling spatial fish abundance? I'd also suggest adding that another objective would be to estimate the total biomass of deep-water species for the ICES sub-area VII* (10761, L21-30).We have amended the text in this paragraph to incorporate the referee's suggestions and have added a sentence to make clear that we are using fishery-independent trawl data for our analyses.

7) *What were the criteria for selecting these 3 species* (10762, L1-3)? Please see reply to Referee 1 above. We have now added the analyses of the length-frequency distributions for all 8 species to the manuscript.

Methods

8) *How do spatial differences in the sampling periods may affect the results*? *From figure 1, sets from period 1 were mainly in the north area while for period 2 were mostly in the south part* (10762, L8). This issue was previously also addressed in Bailey et al 2009. There appears to be no important difference between the communities at the same depth from opposite sides of the Porcupine Seabight. In the present study the data was tested for spatial autocorrelation. In addition, data points for areas that were not covered by both trawl surveys were removed prior to the analyses. For individual

species analyses in which spatial autocorrelation was detected (e.g. *C. rupestris*) this was accounted for by incorporating a spatial autocorrelation structure into the statistical model, as described in the statistical analyses section (10764, L 5-6).

9) *What are the landings data for? From the objectives of the paper there's no need for this information (10762, L22).* See response to point 6 above, the manuscript has been amended to include this within the objectives at the end of the introduction. The fishery landings data were only used to compare to the fishery-independent scientific data.

10) *Did the tow speed change with depth of trawling? If yes, how this will affect the analyses of total biomass with depth?* (10763, L9). We can assure the referee that the tow speed did not change with depth of trawling.

11) *Did the authors try the model with other variables (such as year, month, duration of the trawl, area of trawling, latitude or longitude, among many others) that were afterwards rejected? If yes, let the reader know. If not, explain why you choose to model using only 2 variables (depth and period) (10764, L15).* As described in the statistical analysis section of the manuscript (10764, L2-4) latitude and longitude were incorporated into the analysis in order to check for spatial autocorrelation. In addition, we also used checked whether other explanatory variables including year, month or Cruise number were of importance in explaining any of the observed patterns. As described in the statistical analysis we did find that for the analysis of *S. kaupii* biomass that differences in biomass between sampling cruises resulted in heterogeneity of variances, which were dealt with by using a random effects model (see description 10764, L14).

12) *Why did you choose to model using period has the main variable and not year 10764, L15? I believe the effect of pre- and post- fisheries development could also be detected using year.* We used time period, rather than individual years in our analyses, as the aim of the paper is to compare pre-and post fishery changes, rather than changes on an annual basis. Due to the gap in available data in the 1990s using

"period" as a nominal variable with two levels is more appropriate for the aims of our paper.

13) *2.4 GIS methods: How this section relates to the objectives of the paper? No such detail is needed when describing what you've done (10765).* The importance of the GIS analyses have now been incorporated within the objectives of the paper (see response to points 6 and 9). Although the referee finds that the GIS methods are too detailed, we find that incorporation of such detail is necessary to ensure that the use of our methods are comprehensively reflected and allow the reader to follow our process.

14) *The assumption that spatial fish distribution and abundance can be modelled using depth as the unique explanatory variable needs to be better supported or will look like a gross simplification (10765, L12).* We do not agree with the reviewer that the use of depth as an explanatory variable for the present analysis will be seen as a gross simplification. Not least our own previous publications (Collins et al. 2005, Bailey et al. 2009) and those of other authors (e.g. Rex et al. (2006) Mar. Ecol. Progr. Ser. 317: 1-8, McClain (2004) Global Ecol. Biogeogr. 13: 327-334) have shown that depth is of vital importance to the distribution and abundance of deepwater animals. Using depth as an explanatory variable, does no way assume anything about the relative importance of other potential (but unmeasured) factors.

15) *3.2: The results of the model show inconsistent results among results. How to explain that 3 out 8 species declined in biomass, 3 had no changes and 2 may have increased their biomass? It may looks like having detected the biomass changes by chance (3, 3, 2). How the modelling approaches may have influenced the outcome of the models?* (10766). We are not sure we understand the referee's comment and why our results are inconsistent within results. The total biomass analysis was based on the complete set of demersal fish species (n = 93) caught in the PSB within our surveys to give an indication of how the demersal fish community as a whole may have changed between the two periods. The second part of the analysis only concerns the 8 most dominant species in terms of biomass within the area for which there were enough

data points available to conduct a robust statistical analysis. Thus any of the biomass changes that we detected for the 8 species were not detected by chance.

16) *The authors could provide information on average landings per year for both periods* (10768, L12-15). Please see response to Reviewer 1 point 2.

17) *The authors could provide information on average landings per year for both periods* (10768, L12-15). The reviewer raises an interesting point, however, there is no single agreed way in which errors can be associated with the predicted values of GLS. The disadvantage the reviewer alludes to, is that the reader is left without any feeling of the variability of the data. Rather than use standard measures of variability (R^2 is not appropriate for GLS, because there is no requirement for R to be bounded by 0 and 1 and does not represent the percentage total variation accounted for by the model; bootstrapping is seen by some as appropriate, but others argue the opposite), we plotted the raw data along side the model prediction lines to give the reader an indication of variability in the data. This is in line with standard methodology (e.g. Langenheder et al. (2012) BMC Ecology 12: 14)

187) *How this conclusion contradicts Bailey et al., 2009 general conclusions?* (10769, L21-22). This comment from the referee is not clear. We do not say that the present results contradict results from Bailey et al. for *C. guentheri*. In fact both the abundance and the biomass results are showing the same patterns.

198) *The discussion of the communalities and differences among species is difficult to follow. A general discussion on factors affecting the differences observed between species could be provided (10769 and 10770).* We agree with the referee that our results are very complex and species specific, however a general discussion on the factors affecting differences between species would at this stage may not be useful as, there are too many unknowns on the mechanisms that are underlying some of the observed patterns.

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