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Interactive comment on “Cold-water coral growth under extreme environmental conditions, the Cape Lookout area, NW Atlantic” by F. Mienis et al.

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Comments reviewer 2

We wish to thank the reviewer for the efforts and input provided. We carefully went through all the comments and suggestions. We have adjusted the manuscript according to the comments made. Below we provide a description of the adjustments made, addressing the reviewers remarks.

General comments:

the integration with the community ecology and habitat modelling components could be improved. We have extended the discussion and have tried to integrate the community ecology and modelling components. It should be mentioned that a detailed analyses

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of the diversity and abundance of invertebrates and fish species recorded on the video will be presented in another manuscript.

In particular, the oceanographic data presented in this study do not appear to be used in the niche modeling analysis to help improve the predictability of coral distribution. In addition, the models are presented at a spatial scale that is far greater than the data presented in the manuscript, and because of the difference in scale. The model we refer to in the paper was made *a priori* on the basis of available large-scale oceanographic dataset. The extreme temperatures records we found will become part of this growing dataset but our single point observation on extreme temperatures is a drop in the ocean of observations around cold-water corals (CWC) and therefore will not yet impact large scale models. More in-situ observations or experimental data will be needed to show that the temperature envelope in the models must be radically adjusted. Recently data on CWC distribution in the warm Mediterranean are becoming available and this will help to make the model improvement. Furthermore, local measured oceanographic data is not useful in the modelling analysis, we do not have reliable shelf scale data on currents, and the single point data cannot be used as a predictor variable.

I don't find the models very informative. They simply confirm that there should be deep water corals in a very large region, where they are already known to occur. If these models would use more of the new data presented here and used to refine our understanding of the controls on coral distribution, they would be much more informative and would be a better fit in the manuscript. See also our comments above. The model was made and used to confine the area of our explorations (e.g. multibeam). This showed that CWC occurred only in part of the area which clearly exposes the shortcomings in current general habitat suitability models for CWC. Although our measurements would perhaps allow for an improved local model – i.e. only applicable to Cap Lookout - the principal goal is to make models that have much wider (global) validity. However, there is still a way to go before the key variables for CWC growth have been clearly defined. This model was used to reflect the general lack of suitability in the region, indicating that

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this site is at the limit of the tolerance of Lophelia, which is confirmed by the near-bed environmental conditions that were measured.

The community ecology section of the manuscript generally feels like an add-on that is not well integrated with the rest of the study. The data are presented as differences between substrate types, but this is not tested with any rigorous statistics. It is also not correlated to the oceanographic data in any way, and since that is the real driver of the paper, it made me wonder why these data were included. There was a chance here to integrate the oceanography data, especially the acoustic backscatter, with an analysis of the community structure of pelagic species that may be interacting with the coral habitat, but I never saw this happen. The composition of the larger i.e. long-lived benthic megafauna forms an integration of the variable conditions (quantity and quality food, oceanography etc.) near the seafloor. The fauna data are in our view crucial because they show that there are spatial differences between reefs which are not visible in for instance the large-scale temperature jumps we measured over the wider area by two landers and a mooring. This points to other factors – we have not identified which are important for the reefs and their inhabitants. Using acoustic backscatter from adcp's to identify the composition of the pelagic community, is riddled with errors and requires more dedicated surveys and techniques. What we show is evidence for migration of deep-scattering layers (zooplankton, fish?) which might transfer food to the reefs in the form of biomass or perhaps faeces.

Specific comments:

P5 L1-2: There are many more studies of the : : "living fauna and ecosystem functioning of smaller coral habitats: : :" that the authors neglect, including any mention of the Gulf of Mexico work of some of the co-authors. This seemed strange to me. We have added references.

P8 Section 2.2: The methods of the predictive modelling need clarification. There is no mention of the sources of the data used in this study. Were some of these

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collected as part of this study? If not, where did they come from? Are they actual data, or the results of modelling studies? This is especially significant for the aragonite saturation state data. We have provided this data as a supplementary material file that has a full description of data sources, handling and model training gain statistics. We chose to omit this from the paper and put them in the electronic supplementary materials as the main focus of the study is on the geophysical and faunal data and its inclusion would heavily complicate the manuscript. To answer your questions fully, the model data were not collected as part of this study but are fully referenced in the electronic supplementary materials. Many of the variables are actual data, i.e. substrate, temperature and so on. The aragonite data were obtained from models by Orr et al. (2005) and is fully explained in the electronic supplementary materials.

P18 L6: This is one of the most glaring examples of a lack of statistical rigor. Statements like “In general, faunal abundance increased whenever coral framework was present: : :” is not backed up by any sort of test. Please avoid making conclusions without statistical tests. We have added statistical evidence for our statements to the paper.

Fig 6: These pictures don't seem to support the habitat classification. Were the proportions of substrate types simply determined by approximation, or were the photos digitized and proportions calculated using software somehow? There seems to be a lot more living coral in 3 than in 4. Also, it is unclear what the word “slope” is referring to here. The continental slope? The depth range doesn't seem to be sufficient for that to be true – there are no true “lower slope” surveys. The habitat types were operationally defined a priori i.e after analysis of all video clips. By taking a small number of contrasting habitat types, we have reduced the error in assigning images to habitat type. One-minute video intervals were assigned to the predominant habitat type on the basis of estimation of important features by two observers. The photos shown in the paper are usable high-quality frame grabs for the purpose of illustrating important habitat features. Because of limited video quality due to strong currents and high towing speed

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not all recording were suitable to extract stills. However, the extensive video recordings we analysed were key for the habitat identification.

Fig. 7: Was it really not possible to ID the fish beyond bony fish vs. cartilaginous fish? I'm not sure whether these very large groupings are relevant in any community function analysis. It was possible to ID the fish beyond bony vs cartilagenous fish. However, this paper gives an overview and first impression of the megafauna and fish fauna. At present data are analysed in more detail. These data will be published in a separate manuscript.

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