Reply on RC1
Emanuela Fanelli et al.

Author comment on "Mesoscale variations in the assemblage structure and
trophodynamics of mesozooplankton communities of the Adriatic basin (Mediterranean
Sea)" by Emanuela Fanelli et al., Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2021-240-AC2, 2021

Dear Anonymous Reviewer 1, first of all thank you for your thorough revision, which will
allow us to provide a more accurate description of the assemblage and trophic structure of
the mesozooplanktonic communities of the Adriatic basin, please find below a point-by-point
replies to your concerns, while we’re preparing an edited version of the manuscript, once
received a positive reply from the editor.

Best regards Emnauela Fanelli

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This manuscript provides a description of the differences in taxonomic composition and
several trophic indicators of mesozooplankton between several subregions. The authors
claim that this is the first study covering both shallow and deep waters across the whole
Adriatic basin, a productive ecosystem within the otherwise oligotrophic Mediterranean
sea. From plankton data collected in a single cruise, several communities were
characterized from abundance and biomass composition assemblages as well as by their
values of natural abundance of stable carbon and nitrogen isotopes. While the description
of the characteristics of these communities and the new data constitute a valuable
contribution for the understanding of the functioning of the Adriatic ecosystem, the
manuscript requires a careful revision addressing several important issues:

- The word “mesoscale” in the title and in the objective is misleading, as no evidence is
  presented on hydrographic features or dynamics (i.e. currents, fronts, eddies,...)
  characterising the different subareas or were directly related to the described plankton
  assemblages. As the study conclusions are limited to large subregions, mostly related
  to the fishery management, the title and the objective should be more precise in this
  regard (e.g. Differences in taxonomic and trophic mesozooplankton assemblages across
  the Adriatic basin...).

Authors: Thanks for the observation, we agree and change the title accordingly
"Differences in community composition and food web structure of mesozooplankton
communities across the Adriatic basin (Mediterranean Sea)"
The claim that the study is relevant for the whole basin is not entirely supported by the data. The samples were collected on mostly the southern coast, with most of the stations on shallow depths (< 50 m) and none of the stations were located in the deepest waters (>200 m). The study characterized differences between inshore and offshore plankton and also between some subregions. The latter, initially defined from fishery survey areas, were subsequently grouped in the different analysis. However, because the limitations in the sampling, the different zones finally considered for the analysis need to be clarified. In the current version of the manuscript, the initial expectation of having 4 geographic subareas and two major depth categories (Fig. 1) is dismissed in the subsequent analyses. It would be appropriate to define the final analysed regions from the beginning by providing clearly the reasons for grouping the “fishery” areas and use an specific, more intuitive naming (e.g. North, Central, South) instead of the non-intuitive original code names (GSA17N, GSA18,…). Also, the variables used for supporting the groupings must be detailed (e.g. L 129-131), as well as the criteria for the inshore vs. offshore classification (L 176).

Authors: we agree with the reviewer concerning the lack of specifications in the abstract about the surficial “nature” of our samples. The MEDIAS survey effort, the samples came from, is concentrated to the epipelagic zone and thus no samples below 200 m are usually collected (we added this clarification in the abstract and throughout the text). Concerning the spatial effort, this included, samples collected across the entire basin, on the western side, as indicated by haul points in figure 1. Regarding the sub-areas we agree with AR1 that the used of fishery subdivision could be misleading and thus we provided a clear rationale for the use of the three areas which are the object of the following analyses from the beginning, as suggested by AR1. In the revised version we’ll also detail the variables used for supporting the groupings, as well as the criteria for the inshore vs. offshore classification.

The organization of the manuscript must be improved. There is methodological information among the results (e.g. co linearity tests L 298-300; acidification test L 307-310), excess of auxiliary tables that could be safely moved to supplementary information (e.g. Tables 1 to 3, table 6), results among the discussion (e.g. L 421-424; 427-429), and analysis not well justified (e.g. PERMANOVA on untransformed data in L 174-177 and later on transformed data in L 182, a priori classification in trophic groups in L 209-213 and cluster in Fig. 4). In addition there are several inaccuracies and inconsistencies in the naming of variables and data through the text and tables (e.g. taxa with unknown trophic group = “Unk” or “?”).

Authors: we wish to thank AR1 for his/her indications about ms organization improvement. We’ll carefully revise this aspect in preparing the revised version. Auxiliary tables (1-3 and 6) will be moved to Supplementary materials, as requested. inaccuracies and inconsistencies will be carefully checked and corrected.

The abundance and biomass comparisons are made on data averaged in the water column (i.e. N or B / m^3). Given the difference in the water column depths sampled in the different stations this approach tend to reduce the importance of offshore stations as the numbers are “diluted” in a large volume of water. In this case a normalization per unit surface (m^2) may be appropriate (or at least explored). Also, the use of a logarithmic
scale in the relevant plots (e.g. Fig. 2a) will facilitate the comparison of values.

Authors: we really thank the reviewer for this observation and we’re using the normalization to surface unit as suggested. We’re also changing the scale (to logarithmic) in the related plots.

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- The isotopic niche comparisons are flawed because of the lack of samples of species of low trophic position (TP<3) in one of the areas (Table 5). It would be more appropriate to limit the comparison of ellipses to all three areas but only for species with TP>=3 and the comparison of species to the ones having samples in all areas (e.g. C. helgolandicus, Chaetognatha, and Decapoda-zoea). In addition, the trophic position estimations are based on only one d15N measurement of G. tenuispinus from the central area that is applied to all areas. The authors must clearly justify the use of this baseline taking into account that the feeding of this species is ranked as “Unknown”. The use of a single baseline implies that the average source of N is the same across the basin which does not seem the case because of the inputs of the Po river (e.g. L 34-36, 125-126). Have the authors explored the possibility of using other values reported for zooplankton in the region?

Authors: We’re restructuring this part according to reviewer suggestions, i.e. considering only species in common to the three areas and also exploring the possibility to use other values reported for zooplankton in the region. However this last point could be difficult due to the scarcity of data in this sense and also to the possible bias generated by using different IRMS and labs as highlighted in Mill et al., 2007 (Mill AC, Sweeting CJ, Barnes C, Al-Habsi SH, MacNeil MA (2008) Mass-spectrometer bias in stable isotope ecology. Limnol Oceanogr Methods 6:34–39).

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In addition there are several minor issues that need the attention of the authors, as exemplified below:

Authors: All minor issues have been addressed.