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Interactive comment on "Spring distribution of shelled pteropods across the Mediterranean Sea" *by* Roberta Johnson et al.

Anonymous Referee #3

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The paper by Johnson et al. presents a spring distribution of environmental parameters, along with corresponding pteropod abundance and species distribution across the Mediterranean Basin. Using statistical analyses of PCA, CCA and BLMR (binary logistic model), the authors aim to delineate major environment drivers behind pteropod distribution and abundances, and compare it to foraminifera. They compare biological parameters in the W vs E of the Mediterranean basins, the split that is based on the distinct biogeochemical region. The paper amasses valuable datasets, both chemical and biological, but the current analyses and mostly interpretation raise substantial questions that need more careful addressing.

Major: 1) This study presents the distribution of omega saturation state as if this has not been published before. It is not clear if this is novel result of this study or the pro-

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Discussion paper

files were constructed based on data published before. If this is not clear, it is difficult to judge the suitability of carbonate chemistry data. 2) By splitting the basin into W-E before analyzing the station variability within sub-basin ignores inter- and intra-specific variability of each sub-basin. Based on the abundance data (Fig 5), there intra sub-basin variability could be as large as the W-E comparison. The same pattern is true for species distribution. As such, the authors first need to reconcile the level of variability between the stations before they can attempt to make a W-E division. 3) Based on Fig 2 and Fig 5, W and E part of the basins have comparable abundances and species distribution, the difference is really in the transition zone between the E and W (station 9, 14, 15, 16). This is the real results and not random W-E division. However, this makes a large portion of the discussion invalid and needs to be reconsidered and restructured. 4) In addition to #3, I disagree with the findings that omega is a major driver of pteropod distribution, or if it is, the co-authors need to do more throughout job to prove this. a. Given the correlation of majority of environmental parameters (temp, pH, omega, salinity; Fig 3D), it is really impossible to delineate a single driver. Contrary to Fig 5D, Fig 5C actually does not show the same co-linearity. Is this because of the exclusion of nitrate and fluorescence? The authors should attempt to present the correlation matrix of different parameters to the reader can understand the multi-collinearity. b. In the absence of one driver interpretation, I suggest that the authors stick to a multiple parameter interpretation. They delineated that various different parameters impact pteropod distribution but in the discussion they have abandoned this results and reduce it all to one, omega saturation state level. c. PCA analyses actually shows very complex relationship between environmental parameters and different pteropod species (Fig 3C), some with negative and the other species with no interaction with carbonate chemistry. However, what is baffling is the fact that PCA graphs for each species (Suppl Figure 1) actually shows that the environmental drivers are different depending on the basin. In such way, the distribution in the E part is driver by different set of parameters than the W part. In my opinion, this invalidates current analyses by subdividing the basin into E-W instead of dealing with the data on the

station level. I would strongly suggest the reiteration of the analyses on the station level to (in)validate the current results. d. Authors should analyse and interpret species distribution in greater depth based on the species habitat niches, maybe inter-specific species difference are driven by the differences in the vertical migration pattern (DVM)? How could sampling biased the results of species with deeper than 200m DVM? None of this is currently in the paper. e. Current interpretation does not hold up against the presented results and need significant restructuring. Based on the snap-shot spring distribution, the authors need to scale down the extrapolation in the discussion.

Additional: 1) There should be some background on the abundance and species distribution of pteropods in the sub-basin of the Mediterranean. That has been a well-studied topic, both spatially and temporally but authors do not include any of such data and such, fail to establish the baseline knowledge. In addition, there are no comparison of this study with the previous study on pteropods, which would give it a better comparison and evaluation. 2) The introduction fails to identify what exactly will be investigated in this paper – and that is not the population response to climate change. More structure hypothesis testing needs to be presented in the Intro. 3) How were satellite data obtained and averaged; daily/monthly? How was this done? Do you have fluorescence of chl-a data? Different figures and tables have different parameter enlisted. 4) Usually studies report the abundances in ind/m2, not m-3. Can this be, for comparison reasons, amended with 200 m vertical depth. 5) How were abundances enumerated? 6) How were the samples preserved and what was pH of the solution? 7) How do authors explain the salinity as a dominant driver, is it possible that the current distribution is water-mass related, and thus, the species have affinity to specific water mass (rather than to a environmental parameter). Please, comment on this and explain, why could you exclude the advection as a driver behind observed distribution. 8) Forams do not have a proper introduction, very confusion for a reader. 9) Figure 4 does not show Adriatic and NE part, the latitudes do not align.

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