Biogeosciences
Discussions

Interactive comment on "Bio-geographic classification of the Caspian Sea" *by* F. Fendereski et al.

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We would like to thank Reviewer 1 for his/her valuable comments, suggestions and remarks that have greatly helped to improve the quality of our manuscript. The manuscript has been revised accordingly.

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Response to reviewer's comments:

RC1: The authors use a hierarchical agglomerative clustering but provide no apriori rationale or discussion of spatial hierarchy.

Response to RC1: One of the advantages of HAC over other methods, such as K-

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means is that it produces not only the clustering, but also the hierarchy of clusters, which can be useful to understand the relative distance and similarity/dissimilarity between clusters. Additionally, HAC is an unsupervised classification method that requires no a priori specification of the number of classes. Finally, HAC provides a deterministic clustering insensitive to initialization. The use of HAC is now explicitly motivated in the revised manuscript and the advantages of the method are listed in the methods section, page 11, lines 5-11). Furthermore, we now highlight and better discuss the hierarchical organization of ecoregions illustrated by figures S4 and 4 (page 14, lines 10-18). Please also see our detailed response to reviewer comment RC6.

RC2: How the in situ data were analyzed, and it is unclear whether data treatment resulted in a robust assessment of group identity. As these data were used as a means of independent verification, it calls into question the ecological relevance of the physical classification.

Response to RC2: After an extensive literature search for species distribution maps for the entire CS, distribution maps for 36 endemic/non-endemic species, from phytoplankton to mammals, compiled by the Caspian Environment Program (CEP) in 2002 were used for biological validation of our bio-geophysical classification. The data set we used in our paper is the only data set currently available for the CS where individual species distributions are mapped/extrapolated for the entire CS. The data are in the form of digitalized maps and the raw sampling data was not provided by the project. We chose 25 of the 36 individual habitat maps for our validation test (the justification for this selection is now given on page 8, lines 6 to 19). The distribution maps (provided in the form of either presence or biomass observations/estimates) were geo-referenced and all the data sets were transformed into presence patterns. Ecoregions map was then overlaid on each species distribution map and species presence in each ecoregion was inspected. The presence information was finally used for biological classification of ecoregions. We now describe in-situ data analysis in more detail in the methods section of the manuscript, page 8, lines 20-31.

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RC3: The authors should try to more explicitly tie the spatial patterns revealed by a classification based on multivariate physical factors alone (and subsequent bottom-up effects) and the top-down effects alluded to by the patterns of M. leidyi. In other words, explicitly discuss how this classification effort has informed the ecosystem based management needs stated in the introduction.

Response to RC3: We agree with the reviewer that top-down and bottom-up factors have not been very well linked in the previous version of the manuscript. We have clarified the manuscript in order to make the link between biophysical classification and its potential for ecosystem management applications more clear: In the introduction, we highlight more clearly, where biophysical classification has been essential for the quantification of biological community composition in other studies (page 4, lines 25-32 and page 5, lines 1-3). We also add another paragraph on the current deficiency of the species distribution data in the CS, both in space and time, in the introduction section, page 5, lines 3-19 and emphasize the importance of ecosystem classification and remote sensing for high-resolution ecosystem monitoring in the area. We further state that while physical bottom-up controls on plankton physiology and species distribution have been shown to have cascading effects also on higher trophic levels through competition between preys and their specific predators (Day and Roff, 2000), but there are other bottom-up and top-down controls on biological patterns in the CS (page 22, lines11-13 and page 24, lines 18-22). One of the most important top-down controls is the occurrence/abundance of the invasive jellyfish M. leidyi, to which anomalous algal blooms in the SCB are attributed. Since our physical classification has captured the area where these anomalous blooms have been frequently reported, and with the prior knowledge that the jellyfish distribution has been suggested to be controlled mainly by environmental forcing factors (especially SST; Shiganova et al., 2004; Kideys et al., 2008), we believe that our biophysical classification has the potential to capture M. leidyi distribution. We now made this clear in the discussion section, page 22, lines13-23. We further suggest a comprehensive species distribution data synthesis/compilation for the entire CS to further study the biological significance of the identified ecoregions,

and to better study the link between the physical and biological patterns in the CS (page 24, lines 23-32 and page 25, lines 1-9). As stated in the discussion section, page 23, lines 12-16, our classification map provides a description of "large-scale patterns of the CS ecosystem" which will be useful for many administrative purposes, such as longer-term planning and policy development, management reporting and socio-economic statistics. Researchers, policy makers and ecosystem managers can use our classification results in different applications such as ecosystem conservation and MPAs definition, sampling area selection and/or ecosystem modeling (Conclusion section, page 25, lines 16 to 20). For shorter-term and local applications, however, seasonal or even real-time classification methods may result in a more detailed characterization. Regarding the flexibility of our classification method in terms of input variables, spatial and temporal resolution, extent of the study area and observational data records, the method can be employed using updated datasets to improve the ecological classification of the CS for finer spatio-temporal dynamics (such as inter-annual, seasonal or diel dynamics), and even for a real-time monitoring of the CS. We now state this more clearly in the discussion section (Sect. 4.3) and conclusion section, page 25, lines 20 to 25.

RC4: Introduction: There is much focus on the role of the invasive jellyfish in affecting lower trophic level distributions. Specific predictions of spatial patterns may be merited in the introduction given the top-down control and spatial distribution of Mnemopsis that is alluded to throughout the paper. Inclusion of a distribution map of the species may also be warranted.

Response to RC4: We agree with the reviewer that our manuscript focused too much on the role of M. leidyi, even though we are unable to directly quantify top-down effects on the marine ecosystem structure and composition in this paper. This jellyfish is distributed throughout the CS, while its distribution is limited by temperature (Shiganova et al., 2004; Kideys et al., 2008). The species is only observed in the NCB during summer, and its population is reduced during the cold season, when it is mostly confined

to the SCB. This is now better stated on page 21, lines 29-32 and page 22, line 1. We decided not to include a distribution map of this jellyfish in the main text, since the focus of this paper is on the bio-geophysical classification of the CS rather than on M. leidyi, but now refer to Shiganova et al (2004) in the revised version of the manuscript where such a map of the distribution of this species is available for reference.

RC5: Methods: A few more details of the SOM are warranted, e.g. size of the Gaussian neighborhood function and what the final node approximates (e.g. the mean, the maximum likelihood?).

Response to RC5: We now add a paragraph with further details of the SOM on page 10, lines 1 to 9. "The SOM is an unsupervised learning technique that allows easy visualization of large and high dimensional data sets, thereby helping in uncovering their underlying structures. To this end, SOM implements a topology-preserving mapping from the higher dimensional observation space into a lower (here two) dimensional lattice of prototype vectors called neurons. Each neuron (prototype) represents a local summary of similarity observations. The topology preservation implies that neighboring neurons on the map represent similar observations in the input space. This is achieved thanks to neighborhood relations that connect adjacent neurons. At the end of the training, the SOM approximates the probability density function of the input data (Kohonen, 2000)". Details on the size of the neighborhood function are now added to the page 10, lines 14-17. "The learning rate decreases linearly with time and the radius of the neighborhood decreases from an initial value of n/4 (where n is the size of the map, i.e. number of neurons in each direction) to 1 at the end of the training."

RC6: The discussion of and subsequent choice of neural map size was appropriate however the choice of subsequent clustering algorithm reflects an underlying assumption about the way the system is organized. Is the Caspian Sea organized in a hierarchical fashion? Or was this chosen with consideration of the scale and flow of management decisions? Perhaps circulation in the different regions coupled with bathymetry dictate a hierarchical framework. If optimizing differences between neurons is the goal,

then why not a K-means? If hierarchical organization is presumed, then I would expect a subsequent match up with the larger scale classifications (e.g. North Central and South). In any case, the justification needs to be made clear.

Response to RC6: Like other ecological systems, hierarchy is an important property of the Caspian Sea. We now revised the methods, section 2.4.2 (page 11, line 1-2) and added an introductory paragraph on the use of HAC in our study: "Multiple scales are in interaction within and between ecosystems. With the prior knowledge of hierarchy being an important feature of ecosystems (Vichi et al., 2011), we used the Hierarchical Agglomerative Clustering (HAC) method". We agree with the reviewer that the justification of our use of HAC techniques needs to be made more clear. Thus, we have modified the methods section to better highlight the advantages of HAC over K-means and other clustering algorithms (page 11, lines 5-11). It can be seen in Fig. 4 to which degree the Caspian Sea is organized in a hierarchical fashion. This figure shows that, indeed the Northern Caspian is significantly different from the Middle and the Southern Caspian Sea (Fig. 4, upper panels). For the Southern and Middle Caspian Sea, coastal ecoregions differ from open ocean ecoregions (Fig. 4, middle panels). In the revised version of the manuscript, we now highlight the hierarchical nature of CS ecoregions more in our extended discussion of figure 4 on page 14, lines 10-18. Matching up with the larger scale classification of the Caspian Sea has been provided throughout the results section. Ecoregions have been named based on in which of the three Northern, Middle or Southern basins they are located (Results, section 3.1). Also, further physical and biological comparisons of ecoregions have been done separately for these large-scale basins (Results, sections 3.3 and 3.4, respectively). Please also refer to our earlier response to RC1 above.

RC7: It is unclear in the methods how the input data were standardized prior to classification and whether non-normally distributed data were transformed prior to standardization prior to classification. This is important to understand how total multivariate information was partitioned into the initial neural map.

Response to RC7: All the variables were normalized to unit variance in order to avoid the dominance of those variables with significantly higher variance in the data clustering (Kohonen, 2000). In addition, for bathymetry, we used log-transformed data. The normalization and data transformation strategy is described in the methods section on Page 9, Lines 20 to 22. We further add information on the transformation of the bathymetry variable to Page 6, lines 28 to 30.

RC8: Agglomerative strategies can be divided into two groups with different objectives: 1) those that optimize some property of a group of entities and 2) those that optimize the route by which the groups are attained. You seem to use both strategies: the SOM and subsequent HAC on the input variables were of the former but the choice of physical variables appeared to be of the latter. However the choice of linkage method and distance metric is critical to how well a particular HAC meets an objective. Please be explicit and state both the linkage method and the distance method for every clustering that is conducted.

Response to RC8: Because the data was normalized and because of the low level of collinearity among the selected variables, we used the Euclidean distance as the distance measure for the correlation results, SOM and HAC. We used average linkage for clustering correlation test results between input variables and the Ward's method as a linkage criterion for the HAC clustering. This information was now added to the main text, on methods section, Page 11, lines 11-12 and lines 26-27 and also to supplementary materials, section S1, page 1, lines 14-15.

RC9: Section 2.1.1. Initial resolution of the input variables. When resampled at higher resolution, please acknowledge and or account for the pseudoreplication (spatial autocorrelation) that occurs, thus magnifying any differences between ecoregions.

Response to RC9: We agree with the reviewer that resampling at higher resolution may lead to pseudoreplication effects. Yet, in our analysis, resampling at a higher resolution was only performed on the wind speed data. Due to its correlation with other variables

(Fig. 2), wind speed data was not used in the final set of clustering variables. Thus, pseudoreplication was not an issue on our ecoregion classification.

RC10: Did the different climatological extents affect the patterns? For example, the surface isohaline maps (from pre 1995), the ice coverage (2004-2012) and the ocean color and surface scattering may reflect different mean states if includes anomalous years. State the span and spatial scale over which the climatology was calculated for each variable and then discuss implications if different.

Response to RC10: Decadal scale inter-annual variability of physical parameters in the Caspian Sea is, in general, much smaller than spatial variability of these parameters throughout the Caspian Sea, i.e., the patterns that were used for ecoregions boundary delineation. For example, for a period from 1982 to 2000, a positive SST trend of about 0.05 to 0.1 $C°$ per year was observed in the Caspian Sea (Ginzburg et al., 2005). This increase is very small in comparison with the spatial variability of climatological SST throughout the Caspian Sea for the same period of time (4.24 to 24.9 $C°$; see Fig. 5). In other words, despite the inter-annual to decadal-scale variability in physical features, like other water bodies, a generally persistent spatial pattern in the environmental variables used for this classification has been described for physical properties of the Caspian Sea on decadal scales. For instance, Sur et al (2000) stated that the flow patterns in the Caspian Sea have not been affected by the observed sea level rise in the last few decades. Therefore, as stated in the discussion section, since in our classification we are looking for these general spatial patterns of physical variables of the Caspian Sea over a rather large span of time (and not only on a defined period of time), and since we average over periods of several years, we used all available data sets on oceanographic features of the Caspian Sea for the last two decades for clustering. The spatial and temporal resolutions used to create the climatologies of the variables have been presented in Table 1 of the original manuscript, and a discussion of the different lengths over which climatological monthly mean environmental variables have been calculated along with information about the order of magnitude of

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Comment

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Interactive Discussion

Discussion Paper

inter-annual to decadal scale variability of these variables have now been added to the methods section on page 7, lines 2-5.

RC11: I assume that the absolute value of the correlation coefficients became the linkage function in the clustering algorithm (please explicitly state). The goal of orthogonality between input variables is of great merit. But I wonder why the underlying distributions were not considered. The rank correlation is somewhat robust to this but may be overly so. The Kappa spatial overlap analysis (Supplementary Section) is important and should be placed in the regular document. Also include what level of hierarchy at which this was assessed (e.g. number of classes).

Response to RC11: With regard to the linkage function, we used the Euclidean distance as input for Ward's method as explained in our response to RC8. With regard to considering the underlying distributions in input variables while examining correlation between them, as many of our input variables were not normally distributed in the Caspian Sea, we chose to use nonparametric Spearman (distribution free) rank correlation test (Lehmann and D'Abrera, 1998; Pirhalla et al., 2009). Following the reviewer's suggestion, we now moved the sensitivity test for input variables to the main manuscript (please see sections 2.5 and 3.2). The level of hierarchy at which Kappa was determined was the same as what was used for the presented classification of the CS. All other classification parameters were exactly the same as those used for classification of the CS, with 20×20 number of neurons and cutting the hierarchy at the level where we get 11 number of classes. This information was added to page 12, lines 20-22.

RC12: The cross-validation section needs a little more detail. Based on the logic outlined above Equation S4 (1), I would suspect that error would be calculated at each class (2:15) by k (fold) interaction. This would also allow assessment of the error of classification (mean distance of validation set to classified centroids) for a given number of ecoregions to be assessed for each fold (iteration). While there certainly is some subjectivity involved (e.g. choosing 11 because it represents a drop in error from

6 to 5.9), the spread or variance across iterations would allow objective determination of whether a drop in error is significant. Finally, while this is minor, a 40/400 validation set does not seem a particularly robust test nor is it explicitly stated how the sets were partitioned and whether they were repartitioned between each fold.

Response to RC12: Each run of the Equation S3 (1) computes cross validation error for an individual number of classes (n=2:15) in each of the 10 iterations. The following equation (Equation 2) averages these 10 cross validation errors (for the 10 iterations) at a given class level. Each complete run of the Equation S3 (1) and the subsequent Equation (2) computes the final cross validation error at individual class level (n=2:15). In other words, these equations have been fully run for 14 times, i.e. for each final cross validation error (n=2:15). The previous version of the equations did not conduct the readers to this concept and made this confusion between i, which is the class numbers in each cross validation experiment, and the number of classes (hereafter, n). The value of n ranges from 2 to 15 and this value is fixed throughout each full run of the two equations, but i differs inside an individual equation 1, from 1 to number of class (n). For example, i ranges from i=1:2 for cross validation experiment for 2 number of classes (n=2) to i=1:15 for cross validation experiment for 15 number of classes (n=15). To avoid this confusion, we now add an index n to the equations which shows the class level under the cross validation experiment. Training/Validation sets were extracted from each of the six physical input variables used for classification of the Caspian Sea. Each of these variables contained 11760 pixels, 90% of which used for training (i.e. 10584 pixels) and the remaining 10% (i.e. 1176) was used for validation. For the set selection, we created a 120 by 98 matrix (similar to the input variables matrices) containing values from 1 to 10, each value made 10% of the matrix and was distributed evenly throughout the matrix. For validation set selection for each iteration (k =1:10), we selected those pixels in the newly created matrix that were equal to 'k' and extracted corresponding indices in our input variables matrices. Therefore, the validation data are distributed throughout the variables' matrices and each index used only once as validation set. The remaining indices were used for training the SOM. We

now add detailed information on how the training/validation sets were selected to the main text (page 2, lines 12-19).

RC13: The phenology metrics (and the temporal resolution, monthly?) of the chl-a data need to be included in the methods.

Response to RC13: Done (Page 13, lines 10-16).

RC14: In situ clustering: Please include where in situ data were collected on a map. It is unclear whether all in situ samples within an ecoregion were lumped into a single presence column or whether clustering was conducted using multiple samples within an ecoregion (more robust). Please provide justification as to why only 1 and NaNs were included in the classification. The choice of this reflects whether your analysis is robust to errors in omission/commission or both.

Response to RC14: We have used mapped species information rather than raw in situ information for our biological validation. Raw data are based on various observations that have been collected, mapped and published by caspianenvironment.org in 2002. For these maps, the in-situ data have been separately mapped and extrapolated to the entire area of the Caspian Sea for individual species. Unfortunately, the geo-referenced in situ sampling data are not publicly available. Thus, it is impossible for us to include a map of raw observations in our current manuscript. Yet, the open access website from which the data have been extracted is available and has been referenced in Table S6. For using the species data for biological validation of our ecoregions, we first overlaid the map of the CS ecoregions on top of each species presence map and assigned '1' to ecoregions in which the given species presence had been marked on the map. Since the lack of presence of species in an ecoregion does not necessarily mean its absence in that ecoregion (It can be due to, for example, the lack or imprecision of sampling in that area for the target species), we assigned 'NaN' and not '0' to such an ecoregion for that species. This information has been included in more detail in section 2.2.2 of the methods, on page 8, lines 20-31.

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RC15: Organization: Several sections of the results (see detailed comments) should be relegated to the discussion or introduction. There are also omissions (e.g. the phenology methods are introduced in the results) and redundancies. The results section can be better organized to highlight 1) the robustness/sensitivity of the classification; 2) the spatial distribution of physical variables and 3) the independent verification of chl-a patterns, chl-a phenology and in situ community structure. If hierarchical organization was the goal as the main ecoregion names imply, then please discuss each of the three above in terms of that organization. Otherwise, explicitly state that a spatial hierachy is not intended (but again, the choice of HAC belies this) and discuss them North to South.

Response to RC15: The phenology method section has been now moved from the results to the methods section (Page 13, lines 10-16). We re-structure the results section according the reviewer's suggestion for sensitivity/robustness of the classification (Sect. 3.2), spatial distribution of physical variables (page 15, lines 15-16) and independent biological verification. We acknowledge that a hierarchical organization is important for the interpretation of our results. In order to take this into account, we now move Figure S7 (now Fig. 4) to the main text and discuss Figures 4, S4 and S5 in much more detail in section 3.1 on page 14, lines 10-18. Profound differences between ecoregions in the NCB and those in the MCB and SCB, observed in the hierarchical splitting of ecoregions (Fig. 4) and NMDS test result (Fig. S5), have been stated and considered in every part of the results section, i.e. when ecoregions are named (the assigned names start based on in which of these three basins the ecoregions are located) and where they are compared for their physical input variables (the NCB ecoregions and M/SCB ecoregions have been analyzed separately from each other). Also, ecoregions located in the NCB have been biologically analyzed separately from ecoregions of the middle and southern Caspian basins.

RC16: Discussion: It would be helpful to include a brief discussion about how this classification compares to what is known about the Caspian Sea. Subjective, but expertly

informed classifications are dominated by bathymetry and circulation. One might expect an objective hierarchy of surface features to reveal this. Also, please return to the introduction and results regarding differences in ecosystem structure. The inclusion of the multivariate pelagic (and benthic) species distributions is a great asset and the primary validation of ecoregions. Otherwise they are simply (bio)physical regions. Such a discussion may also reveal two types of information: 1) where classified climatological ecoregions "work" and 2) where they don't (e.g. when there is top-down control by an invasive species). Both are critical for the effective management of a region and may help dictate where greater effort is placed for higher frequency and density in situ measurements.

Response to RC16: A new section (Sec. 4.2) was added to the discussion section, in which marine ecoregions are subjectively compared with their surrounding terrestrial ecosystems (Bailey, 1996) and climatic zones. Although biological validation of our ecoregions confirmed, to some extent, the validity of the assumption of a certain level of bottom-up control on species distribution patterns, the limited availability of in situ data does not allow us to examine the recent changes in community composition due to the effect of invasive species and food-web interactions between native and non-native species, pollution and other anthropogenic and natural influences. In section 4.3, on page 24, lines 23-28, we have suggested a comprehensive data synthesis for all major marine organisms inhabiting the CS, along with in-situ Chl-a concentration to (1) further validate ecoregions based on the bio-physical classification, and to (2) quantify the relative importance of top-down and bottom-up factors for ecosystem structure and functioning. While our current approach discards top-down pressures such as grazing rates and the relation between predator and prey biomasses, climate is likely to drive at least some aspects of lower trophic level dynamics (Day and Roff, 2000; Platt et al., 2005). On page 24, lines 31-32 and page 25, lines 1-6, we have suggested our ecoregions to be compared with remotely-sensed plankton functional groups from space because few primary producers could be included in our biological validation due to data limitation. Satellite algorithms designated at the detection of phy-

toplankton functional groups distribution and biomass patterns from space (Alvain et al. 2008; Raitsos et al., 2008; Zwirglmaier et al., 2008; Brewin et al. 2010; Hirata et al. 2011) may help to identify and monitor differences and changes in phytoplankton community structure between ecoregions beyond chlorophyll-a. These algorithms need to be validated for the CS. On the other hand, a correct characterization of zooplankton and higher trophic level biomass and distribution patterns still relies on in situ measurements in routine monitoring programs, which are both costly and time-consuming. We now add this information to the discussion section, page 24, lines 18-32 and page 25, lines 1-9.

RC17: Chl-a climatology. Why not log10-transform? The KruskalWallis test that you employ is good for large outliers, but chl-a is generally treated as a log normally distributed (see Campbell et al. 1995). One can log-transform then conduct a standard parametric ANOVA with multiple comparisons (e.g. the Tukey HSD).

Response to RC17: We now log-transformed Chl-a data according to the reviewer's suggestion, and redid the analysis using parametric One-way ANOVA and Tukey test. The manuscript is now revised based on the new results.

RC18: Pg 4412: Lines 25-30: "These authors: : :", "Their results: : :". Please rephrase. It is unclear to which study you are attributing differences in chl-a.

Response to RC18: Corrected.

RC19: P 4413 Line 5. Suggest that you simply start with " We applied: : :"

Response to RC19: Done.

RC20: P4420 Section 3.1. The spatial coherence is an obvious result, particularly at the scale of the study. I suggest you reframe this in terms of the natural gradients of the physical variables. Additionally you should discuss (tacitly in results, more completely in discussion) how well these recently classified boundaries compare to any existing boundaries, hydrological or political.

Response to RC20: We now add an introductory paragraph to the results (page 14, lines 6-9) where we highlight the spatial autocorrelation of ecoregions and the effect of environmental gradients on the coherent pattern of our ecoregions. A new section has also been added to the discussion (section 4.1), where we compare our marine ecoregions with the surrounding terrestrial ecoregions and climate zones.

RC21: P4421 Section 3.2 Line 8: Reference is redundant to methods, remove. Figure 5 contains redundant information to Figure 6 and is incredibly difficult to read. I would suggest that you simply report the results of your Kruskal Wallis' H to Dunn procedure on Figure 6. Simply annotate with letters above the box plot (with significant differences having unique letters). This might allow inclusion of the principal components analysis shown in the supplementary material (which is a better visualization of overall differences in physical state between ecoregions). Please do something similar with Figure 8. Just report a box-plot with the multiple comparisons results.

Response to RC21: We removed the reference. Figures 5 and 8 were transformed into box plots according to the reviewer's suggestion.

RC22: P4422 Lines 22-26. "The highest chl-a: : :. " These sentences are unclear and possibly redundant to one another. Please rephrase. For example: "The highest concentrations of chl-a are found in the NCB, specifically the two ecoregions at the mouth of the Volga (NCB-XX and NCB-XX). This river supplies 80

Response to RC22: Corrected.

RC23: P4424 Section 3.3.2. Lines 18-26. This belongs in the discussion section. Also without maps or more in depth discussion of M. leidyi distribution, this seems quite speculative.

Response to RC23: The explanation for the observed seasonal variability in Chl-a in ecoregions is now moved from results to discussion section, pages 21, lines 26-32 and page 22, lines 1-10. We now add reference to M. leidyi distribution map (Shiganova et

al., 2004) in the main text, page 21, line 32 and page 22, line 1.

RC24: P4424 Section 3.3.2. Lines 27-28. "Significant differences in the date: : :."You did not statistically quantify differences in phenology, please remove the term significant and rephrase in terms of the qualitative differences observed.

Response to RC24: Corrected.

RC25: Figure 9. In panel D, there appears an error in that the median seasonal cycle is always less than or equal to the annual median. This seems unlikely unless there were strong outliers across multiple months. In panel F, it appears that the bloom onset vertical line is offset from the timing that the median is above the annual median. Also, it is unclear whether phenology was assessed at the climatological level or within each year and whether the grey filled in values refer to temporal variability between years or spatial variability within an ecoregion. Please clarify in appropriate methods and results sections.

Response to RC25: The suggested information was added to the methods section (page 13, lines 10-16) and the caption to Fig. 9. To find the set threshold for detecting bloom initiation time in ecoregions, instead of using only the spatial median, we now use a two-step procedure, where we first compute temporal median over Chl-a monthly mean climatologies and then the spatial median of pixels within each ecoregion is considered as the set threshold for that ecoregion. We now revised the related section (section 3.4.2) and Fig. 9, accordingly and explained how phenological parameters have been calculated in the revised version of the methods section (page 13, lines 11-14).

RC26: P4425, Section 3.3.3 Polovina et al., 2001? This paper refers to the TZCF in the North Pacific and the convergence of planktonic species (including jellies) and the selective foraging by larger animals. This is not an appropriate reference if one is trying to say that transition zones contain both northern and southern species. Be certain to generalize in the text to transition zones.

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Response to RC26: We removed and replaced the reference.

RC27: P 4428. Line 21-24. "This is in spite of the fact: : :". Please rephrase. The location of high abundances of M. leidyi may be under physical (circulation) or bottom-up control.

Response to RC27: Corrected.

RC28: P 4429 Line 1-3. "Due to lack of comprehensive species data: : :" This should be stated upfront in the Methods Section.

Response to RC28: Corrected. Also, in order to include the comments by reviewer 2, the methods section has been extended to include a much more detailed discussion of data availability and the implications for our results. Modifications have been made in the methods section (page 7, lines 27-31), the discussion section (pages 24, lines 17-23), as well as in the conclusion section (page 25, line 15).

RC29: Figure 1. Please include locations of in situ benthic and pelagic data

Response to RC29: As discussed in detail in our response to reviewer 2, and in response to RC14 above, we do not have access to the raw data, as this data is not publicly available. We have used the derived and published species distribution maps, where raw data was mapped/extrapolated to the full Caspian Sea area based on raw observations. We have extended the methods section to give more details on the biological data used in this study (please see Set. 2.2.2).

RC30: Table 1. Please make sure that certain acronyms are consistent throughout text and supplementary material (e.g. MCB-TR and MCB-T). Figure S.5 is missing an axis label (distance?).Figure S3. Missing units. Also, please define behive. Figure S1 is missing the final column and may be more aptly described in Table form. Table S3. It is unclear whether this is mean error or error. Also please state the distance measure (Euclidean). Likewise, please state with Figure S4.

Response to RC30: Appendix A was corrected. In Figure S5 (now Figure S4), the

vertical axis was removed. Figure S3 (Now Figure S2) was corrected and its caption was rewritten. Figure S1 was transformed to table form (see Table S1). In the caption to Table S3, error was replaced with mean error. The distance measure (mean absolute error) has been added to the captions of both Table S3 and Figure S3.

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