

Interactive comment on “Natural variability in hard bottom communities and possible drivers assessed by a time-series study in the SW Baltic Sea: know the noise to detect the change” by M. Wahl et al.

M. Wahl et al.

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Thank you for your constructive review and your helpful suggestions!

Comment 1: “A point for consideration by the authors is to what extent the approach taken of monitoring one year old communities has an impact on the results obtained. Given the life cycles and generation times of the dominant organisms of the Baltic – to what extent does this approach only sample an early succession subset of the community?”

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Answer # 1: In order to quantify the fluctuations in community composition we have to keep as much as possible of the other variables constant. That is the reason why we used across all sites and years identical depths, identical patch size and identical community age. The latter was chosen as of 1 year because this period includes all seasons and reproductive cycles of the species. Reproductive output, the phenology of reproduction, the dispersal of propagules are all intimately linked to environmental conditions which in this study was of focal interest. These biological signals risk to be blurred in the course of succession intermingled with new events of recruitment if for the establishment of the community more than 1 year is allowed. So for the purpose of this study, i.e. to use community composition as a sum response to environmental conditions, we think that a recruitment period of 12 months is the most adequate duration.

Comment #2: “I don’t think the role of temporal variation in propagule pressure/ delivery has necessarily been give enough weight in considering what will drive temporal variation in these communities (given they are only one year old).”

Answer #2: As mentioned before, restricting community age to 1 year, i.e. one reproductive cycles, maintains a clear “propagule pressure/delivery” un-blurred. Its variation is quantified by monitoring its signal in consecutive years.

Comment #3: “In my opinion this coarse view of functionality is not valid. There must be more to functioning than simply determine that there is a producer present or that benthic-pelagic coupling occurs, as is stated in the discussion.”

Answer #3: The finer functional groups are defined the more congruent functional group and species become – as discussed in more detail earlier (Wahl et al. 2011: PLoS One 6 (5) e19514). Our aim was to highlight that despite substantial variation in species composition (and the composition in functional groups) certain important functions are not lost. We agree, however, with the referee that it may matter to which other traits (size, longevity) such functions are linked. We added these thoughts to the

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article.

Comment #4: The statement implies a new procedure but in fact all the authors do is propose the use of an existing procedure, RELATE. This is fair enough but perhaps the authors should downplay the statement in the abstract.

Answer #4: We downscaled the novelty aspect regarding the 2 statistical procedures suggested to detect a “signal” (i.e., departure from normal “noise”): RELATE and CI. We clarify that not the procedures per se are new, but their suggested use in the context of community re-structuring.

Comment #5: Modify the phrase ‘since long’

Answer #5: The sentence was rephrased.

Comment #6: The last sentence of the introduction is hard to comprehend.

Answer #6: The sentence was rephrased.

Comment #7: What does 60 grid mean?

Answer #7: An explanatory phrase was added.

Comment #8: For MDS and RELATE analyses what was the justification for not transforming data? Usual practice is to apply at least a moderate transformation to down weight the influence of dominant species.

Answer #8: Western Baltic Sea hard-bottom communities can be strongly dominated by single macroinvertebrate species such as the blue mussel *Mytilus edulis*. This is a characteristic trait of our study system and we therefore deliberately omitted any transformation of the data that could reduce the influence of these dominant species on the analysis.

Comment #9: the discussion of how different environmental drivers may affect recruitment and succession should be removed from the results section. It merely obscures

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what the actual results of the study are and should be removed to the introduction or discussion. In fact some of this is repeated in the discussion.

Answer #9: We moved this section to the discussion and deleted part of it to avoid redundancy.

Comment #10: It should be made clear that this description is based on regressions/correlations of all monthly environment data in the preceding year. It took me some to figure this out by going back to the methods.

Answer #10: We re-formulated the corresponding part in the result section. Please see lines 432-438 of the revised manuscript.

Comment #11: The phrase 'in contrast to summer SST' does not help.

Answer #11: The phrase was deleted.

Comment #12: Discussion 1st sentence is rather awkward.

Answer #12: The sentence was cut in smaller pieces and partially re-phrased.

Comment #13: The sentence: 'In contrast to this, temperature does affect the abundance of the aforementioned "driver" species which cause most of the structural change among consecutive years' followed by a description of what is already known about temperature effects on different species is confusing. Because of the tense used it is unclear if the temperature effect is a finding or simply a description from the literature. This would be solved by using the past tense in the description of the output.

Answer #13: Following this advice, we changed the tense in this paragraph.

Comment #14: I am not totally convinced that (based on the strength of evidence) around 15 sentences are needed to speculate on why warmer winters could affect mussels. From the results we do not really see whether significant relationships were found, only slopes and r^2 values.

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Answer #14: We now added information about which correlations were significant to the legend of figure 11.

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