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Discussions

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.

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Comments by Referee #1 and authors' answers:

Thank you for the very thorough review and a lot of thoughtful suggestions!

Comment #1: "I had, however, some problems to find support to the utility of the proposed CI, given the limited short duration of the time series."

Answer #1: We think the definition of a confidence interval of inter-annual dissimilarities in community structure is useful, since it defines a zone from which a departure

indicates with 5% incertitude a change which is not "normal". Obviously, our trust in the confidence interval and its meaning will increase with the number of monitored years. The risk, however, is to include in the calculation the beginning or ongoing global change driven re-structuring which we expect. So defining the monitoring period is always a compromise. At the moment we believe that a full decade should be enough. The samples of the remaining 4 years have been taken and will be analyzed in the near future.

Comment #2: "The manuscript would also benefit from including a prediction."

Answer #2: We believe that any prediction on the composition of future communities would be too speculative. Given the high number of positive and negative feed-back loops between biotic and abiotic stressors, nobody will be able to predict which species will increase, which will decrease in number.

Comment #3: "...I feel that the authors do not provide enough methodological information regarding the calculation of the CI"

Answer # 3: This is true. Additional information about the calculation of the CI was added to the methods section. In fact, the reviewer's comment made us re-consider our approach and we optimized the CI calculation considerably (see ms). The resulting pattern, however, did not change substantially which illustrates its robustness.

Comment # 4: "...the discussion does not provide any analysis of the advantages of RELATE over other method (e.g. Mantel test or formal time-series analyses). Therefore, I would suggest to downplay the statistical procedure and focus the abstract more on the results of the study.

Answer # 4: We downscaled the innovative approach aspect considerably

Comment #5: ""functional characteristics". Please, provide a brief definition for this concept and functionality early in the abstract or introduction."

Answer #5: we provided more information and references in the introduction

Comment #6: If species' fluctuations are too synchronous, then compensation of lost species within functional groups is unlikely [e.g. Loreau and Mazancourt 2008)].

Answer #7: This aspect, indeed, had been overlooked by us. We added an appropriate cautionary phrase to this purpose.

Comment #7: "This hypothesis..." Placed here, this sentence sounds like the aim of the study is to test the diversity-stability hypothesis.

Answer #7: True. The phrase was deleted.

Comment #8: "2969, 26 and so on: This paragraph is a bit lose, maybe it could be combined with the last paragraph of page 2970."

Answer #8: We were happy to follow this advice

Comment #9: "1971, 3: Experimental assemblagesâĂŤhow these assemblages compare with those developing on natural substata? Are the experimental assemblages representative enough to estimate parameters of the natural community?"

Answer #9: They are, indeed, so similar to natural communities that it is a challenge to distinguish the panels from their surroundings after one year of recruitment.

Comment #10: "2975, 19-23: The description of the PCA is a bit complicated. For example, I do not know how to calculate the covariance of a "distribution". Please try to rephrase it in order to make it clearer. In addition, PCA is a ordination method based on correlations. Were the assumption of correlation (e.g. linearity and normal distributions) assessed?

Answer #10: We have reformulated the entire paragraph improving its clarity.

Comment #11: "2976, 4: Variables with different units were analysed together with Bray-Curtis dissimilarites. Were these variables standardised before the analysis?"

Answer #11: We never included variables with different units in one and the same

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analysis. It was therefore not necessary to standardize them prior to the computing of the Bray-Curtis dissimilarities.

Comment #12: "2976, 16-21: Use of ANOVA and null-hypothesis testing in general. Spatial autocorrelation among observations may results in a lack of independence among residuals, leading to an underestimation of standard errors of estimators and overestimation of significance of the calculated statistic. Therefore, classical (frequentist) null hypothesis testing is not the appropriate way to analyse spatial data (e.g. Burnham and Anderson 2002). Legendre (1993) offers extensions of linear models that account for spatial autocorrelation by partitioning the variance in the dependent variable between the locality factor and environmental variables, for example. You could also explore the use of Mantel tests to assess the spatial variation in community structure. Of course, all this makes sense only after presenting the corresponding scientific hypotheses. Please also indicate if the factor time was included in these ANOVA, and if so, indicate how did you deal with temporal autocorrelation of observations."

Answer #12: The ANOVA was omitted.

Comment #13: "2976, 24: Use of PCA. Again, it is necessary to indicate whether the assumptions of correlations were assessed. Alternatively, you might explore the use of Canonical Analysis of Principal coordinates (CAP, Anderson 2004), which is based on distance measures (e.g. Euclidean distance or Bray-Curtis) and therefore does not have restrictive assumptions such as linearity and normality in the distribution of errors.

Answer #13: While some authors say that PCA does not require normality at all (see Zuur et al. 2007, page 194), others assume that it requires multivariate normal distribution but is very robust against violations of it. However, multivariate normality cannot be tested easily and cannot be concluded from normality of the individual variables. Since PCA is not producing any p-level for hypothesis testing in general and we do not use it for model-building here, we do not consider normality relevant and did not test for it. We checked for linear relationships between variables by visual inspection of scatter

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plots and found linear or close-to-linear relationships in 95% of all cases. In no case we observed a tight and curved relationship, what is actually the scenario that could spoil a PCA. We state this now in the ms in line 253-255.

Comment #14: 2977, 9: "...species abundance data were averaged..." I think this procedure is inappropriate, because the mean of raw abundance data of individual species does not necessarily resemble the average assemblage across time (McArdle and Anderson,2001). In addition, lack of normality can increase bias in the estimation of the mean. Itwould be advisable to generate the Bray-Curtis matrix, estimate the centroids of each cell (interaction between time and site), and then use them in the trend analysis.

Answer#14: We have changed the procedure for the calculation of interannual Bray-Curtis dissimilarities to meet these concerns: (1) Interannual dissimilarity at a given site was calculated as the average of all dissimilarities among replicate communities in 2 different years, (2) for each site we then calculated the mean inter-annual dissimilarity as the average dissimilarity between any 2 years of the monitoring period, (3) finally we calculated the 95% confidence interval of the average inter-annual dissimilarity. This procedure is described in more detail in the ms. The overall pattern of "noise" (i.e. mean interannual dissimilarity) across all sites did not change substantially as a consequence of these methodological changes.

Comment #15: 2977, 12-14: Use of SIMPER. Maybe I missed something, but it seems that the PCA described above (2976, 24) and this SIMPER analysis have the same aim; that is, to identify the species that explain most of multivariate dissimilarities. If these analyses are redundant, then I would leave only one of them.

Answer #15: We do not consider these analyses redundant. While the PCA was applied to the entire data pool in order to identify the most relevant species across all years and stations, SIMPER serves to spot the species that contributed most to year-to-year differences between communities. As a consequence the data bases for these

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two analyses are not identical and the results do not give the same information. For instance, the pool of species identified by SIMPER is three times larger than the one suggested by PCA.

Comment #16: 2977, 15-21: This is a good idea. As expressed above, however, use of classical hypothesis testing on spatial, autocorrelated data may be problematic. You should either adjust your model to account for autocorrelation (e.g. a mixed model) or maybe use a different approach (see above). On the other hand, the use of these species, and not others, should be justified. Were them identified with SIMPER?

Answer #16: We ran these analyses with the abundances of the three species that were identified by PCA as the most relevant ones in the entire data pool. We now clearly say so in line 277-278 of the manuscript. Spatial autocorrelation between residuals is not a relevant issue for our analysis, since we averaged the abundances across all data points of a given site and used the means for the correlations. Furthermore, we inspected the residuals of all correlations visually and found no evidence for any strong autocorrelation between them.

Comment #17: 2977, 21-23: This is a bit confusing. First, you say that data were not stratified by site or year, but then you state that data were averaged across replicates at a given site and year. Please clarify what was actually done.

Answer # 17: The procedure used is now described in more detail in the ms.

Comment #18: 2978, 12-15: Model selection method. Using significance (I guess you're using alpha=0.05) as threshold for retaining a term can be problematic because of the uncontrolled error Type I error caused by multiple hypothesis tests. You may want to use AIC to identify the appropriate model. Since you use R, you may want to check the meifly::fitall() function in order to run all possible models.

Answer #18: We re-modelled the data while verifying all simplification steps by comparisons of the model AIC before and after the simplification. This brought us to a

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minimal model that contained more interactions (including a four-way interaction) than the one that we suggested in the last version of the manuscript. Since the significant interactions do not allow the interpretation of the main effects, we have now decided to omit the multiple regression approach and only focus on a simple linear regression that models the biological dissimilarity between communities in consecutive years as a function of the averaged hydrographical dissimilarities.

Comment #19: 2979, 27 and so on: Species trends described here are hard to identify in the Fig. 4. As far I remember, you ran a PCA on abundance data. You could improve this analysis by including lat-long in a RDA (I would prefer a dbRDA a.k.a. CAP - see above) and show a biplot of species and lat-long.

Answer # 20: We hope to have made the description of the PCA clearer, especially we have described how in our case the covariance of the original particle distribution is needed to find the principal variances along a set of directions other than those of the longitudinal and latitudinal axis. PCA is based on correlations, however, the variance ellipses (dispersal kernels) appear to be circles if the correlation between the longitudinal and latitudinal axis of particles is 1. In case the correlation is 0, the variance is only along one of the axis of the original data set. Every correlation between 0 and 1 generates an ellipse shaped dispersal kernel with an axis orientation (derived from the covariances) other than those of the original axis.

Comment #20: 2980, 19 and so on: Was year included in the ANOVA model, or are you considering the years as replicates? The reader could better know what was done if you include the degrees of freedom in the description of the results.

Answer #20: ANOVA was omitted

Comment #21: 2982, 7-15: Interannual variability. It is unclear how this is presented in Fig. 8. Did you calculate an average lag-1 dissimilarity or was this value estimated by resampling? About the CIs, if the CI is the highlight of the manuscript, then it should be better described and defended in the text. For example, were the CIs obtained by

means of resampling (e.g. bootstrap)?

Answer # 21: To meet these and previous concerns, the method of calculating the CIs was improved and its description is much more detailed now.

Comment #22: 2983, 10: What do you mean with "total abiotic variability"? Are you talking about the Bray-Curtis dissimilarity matrix calculated from environmental data?

Answer # 22: Yes we do (and added this information to the ms).

Comment #23: 2983, 21 and so on: Effect of temperature on compositional variability. Since you are analysing spatial patterns, without the possibility of isolate any factor, it is risky to assume causality in regressions.

Answer #23: We replaced the word 'related' by 'correlated'. Revised manuscript: line 427

Comment #24: 2983, 26: Please explain what do you refer to with "warm winters" here and along the discussion.

Answer #24: We now specify this in the lines 434-435 of the revised manuscript.

Comment #25: 2984, 6: Prevalence corresponds to the proportion of observations that have a condition. This is different from percentage cover, as the latter is estimated for each observation.

Answer # 25: We replaced the term "prevalence" by more appropriate expressions.

Comment #26: 2985, 15 and so on: This is a good point. If you have in mind the hypothesis of "presence of redundancy", you may want to fit a non-linear, asymptotic function between taxonomic and functional richness; a good fit would indicate redundancy.

Answer # 26: We followed this advise and fitted an asymptotic function to the relationship functional versus taxonomic richness. It significantly indicates increasing func-

tional redundancy with increasing species numbers. This aspect is further discussed in the revised version of the ms.

Comment #27: 2986, 12-27: Another interesting point. Compositional variability decreased with decreasing species richness. I would add that compositional variability is predicted to be negatively related with aggregate variability. Aggregate variability (i.e. the variability in aggregate, community-level properties like total biomass) was the metric that decreased with species richness in the study of Valdivia and Molis (2009). Therefore, the relationship between diversity on stability depends on the organisation level that we observe (e.g. Micheli et al 1999).

Answer # 27: We refrained from adding the aspect of aggregate variability since we do not consider community dissimilarity as "aggregate": it depends on the identity and abundance of every single species, and the decline of one is not compensated by the increase of another species.

Comment #28: 2987, 28 and so on: Please, present and defend the definition of "noise" earlier in the manuscript. I say this because noise can have different ecological meanings, depending on the degree of temporal autocorrelation in the variable.

Answer #28: We added our definition for "noise" and "signal" to the introduction.

Comment #29: 2988, 25-27: I would replace this brief paragraph with a conclusive, take-home message of your work.

Answer # 29: This was done.

Technical corrections 2971, 5: Replace "which" with "that". Answer: This was done

2978, 7: I would suggest to replace the comma (,) with a semicolon (;). Answer: Done

2978, 12-20: I would swap the description of the maximal (full, isn't?) model and how you selected the minimal adequate (reduced, isn't?) model, placing first the full, and then the selection criteria. Answer: We omitted the modelling in the revised version of

the manuscript. This part has been deleted.

2980, 13: Shouldn't be 6 years? Answer: Right. Corrected.

2981, 10 and along the result section: 7a, b should be in uppercase, according to the figs. Answer: This was done

2983, 14: I would suggest to combine Figs. 3 and 9B into one figure. Answer: We did not do this because they contain different information

2984, 14 and along the discussion: "Explains". Please, use past tense in the discussion when referring to the results of the present study. Answer: This was done

2984, 25: "Polysiphonia suffers..." I am no sure if this is correct. Answer: We modified the phrase accordingly.

2985, 11: What is a "trophic situation"? I would use "food availability" or alike. Answer: This was done

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