Biogeosciences Discuss., 10, C1268–C1271, 2013
www.biogeosciences-discuss.net/10/C1268/2013/

Biogeosciences
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.

Anonymous Referee #2

Received and published: 26 April 2013

This study examines changes in taxonomic and functional composition of benthic hard bottom communities in the Western Baltic using a 6 year time series and attempts to define the boundaries of natural fluctuations in order to provide an appropriate baseline from which regime shifts may be detected. The argument that species poor environments such as the Baltic may be at particular risk from environmental change due to the lack of 'insurance' is well made and persuasive (although it is repeated in 2 parts in the introduction and should be brought under one topic). I agree entirely with the explicit statement regarding the view that monitoring had become viewed as old fash-

ioned, but now has seen a revival. If anything I think this argument can be made more strongly and expanded as a counter to funding agencies who can seem to have very short sighted views.

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? Also give the aim of establishing a baseline from which environmentally drive departure could be detected, how appropriate is this approach? Would it not have been better to sample the whole community with enough power to incorporate the variability imposed by mosaics of differently aged communities and hence incorporate all this real variability in the baseline? 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).

It is nice to see taxonomic and functional change being considered side by side and the overall conclusions of shifts in taxonomic composition but little change in functionality is of interest. However this conclusion, which features prominently in the abstract is not supported by the results where there is reported 'Interannual dissimilarity in the taxonomic composition of communities was closely followed by a dissimilarity in the composition of functional groups ($r2 = 0.87$, $p = 0.001$)'. It is only after discussing that a coarser view of functionality may be more appropriate that the conclusion stated in the abstract comes about. 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 bentho-pelagic coupling occurs, as is stated in the discussion.

I feel the statement in the abstract 'In addition, we propose a statistical procedure distinguishing directional shifts ("signal") from natural fluctuations ("noise")' rather over plays what the authors have done. 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.

Specific comments Introduction P2969 L5 Modify the phase ‘since long’

The last sentence of the introduction is hard to comprehend.

Methods

P2972 What does 60 grid mean?

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.

Results P2983 the discussion of how different environmental drivers may affect recruitment and succession should be removed from the results section. It merely obscures 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.

P2983 L24-28 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. The reason to make this clear is that this approach does hunt quite hard for significant relationships using multiple testing . Having said that I like the graphical approach in figure 11, which in my mind makes it even more important to point out that here the authors are searching for relationships and in some ways the output is rather qualitative (based on eyeballing figure 11). The phrase ‘in contrast to summer SST’ does not help.

Discussion 1st sentence is rather awkward

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 temper-

ature 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.

P2984/5 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 r2 values.

Interactive comment on Biogeosciences Discuss., 10, 2967, 2013.